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"*To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

*"To the solid ground
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THURSDAY, NOVEMBER 3, 1887.

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873–76 *under the Command of Capt. G. S. Nares, R.N., F.R.S., and of the late Capt. F. T. Thomson, R.N.* Prepared under the Superintendence of the late Sir C. Wyville Thomson, Kt., F.R.S., &c., and now of John Murray, one of the Naturalists of the Expedition. Zoology—Vols. XX. and XXI. (Published by Order of Her Majesty's Government, 1887.)

THE twentieth volume of the "Zoological Reports of the Voyage of H.M.S. *Challenger*" contains three memoirs, of which the first is "On the Monaxonida," by Stuart O. Ridley, M.A., and Arthur Dendy, B.Sc. The collection of this group of the Sponges was, in the first instance, intrusted to Mr. Ridley, who, to hasten the completion of the work, was afterwards joined by Mr. Dendy.

When about ten years ago Prof. Zittel gave the name Monactinellidæ to an order of Sponges, the position of this group became for the first time clearly defined. Prof. Sollas, some five years later, pointed out that Zittel's name implied a wrong idea, for that the characteristic spicules of the group were just as often "diactinal" as "monactinal," and suggested that, as both these forms were, however, "monaxonid,"—that is, having only one axis, which, in the case of the diactinal forms, passed through both the rays—the group should be called Monaxonidæ. As this group represents a division higher than that of a family, for which the termination "idæ" stands, all subsequent writers have adopted the name "Monaxonida." This group Sollas now regards as a tribe of the sub-class Demospongiæ, but the authors of this Report consider it with Zittel as one of the orders of the class.

The classifications of Gray, Bowerbank, Schmidt, or Carter, have now little but historic interest, while as for the more recent writers it would even seem as if each new series of novelties described necessitated a fresh shuffling of the orders, sub-orders, and families.

In the chapter on the anatomy and histology of the group the subject of the spicules is fully treated. It appears to us that no apology was needed for passing over the writings of Bowerbank on these forms; and when the authors too modestly refer us for further details as to the nature, &c., of siliceous spicules to, among other volumes, those of this author, we prefer, without meaning the slightest reflection on his great labours, to turn instead to the pages of the present Report.

The very difficult subject of a nomenclature for the spicules is treated at some length. Those of this order are divided into the two classes of "mega-" and "micro-" "sclera." In each of these there is a very numerous series of forms, all of which get separate names, founded on some prominent distinguishing character of the spicule. Let us hope that the majority of these names may find acceptance with writers on this group of Sponges, so that one difficulty in its study may be removed.

Passing over the descriptions of the spongin, the arrangement of the skeleton, and those of the ectosome and choanosome, we must briefly notice a very remarkable structure, which would appear to be quite unique, and which is found in a Sponge (*Cladorhiza tridentata*, sp. n.) from a depth of 1600 fathoms. The little Sponge in which this occurs is in shape somewhat like a miniature watch-stand. Embedded in the soft tissues, all around the upper margin of the concavity, a large number of small yellow globular bodies are found. Each globular body consists of a central, more deeply staining and granular portion, surrounded by and embedded in a matrix of faintly staining, perfectly hyaline ground substance. The granular appearance of the central mass is owing to very numerous embedded cells; these are irregular in shape and nucleated. Other peculiar cup-shaped bodies occur towards the periphery of the Sponge, embedded in the matrix. The authors think it probable that the cup-shaped bodies are aggregations of glandular cells similar to those met with in the ectosome of some other Sponges, and hint that the whole structure may be phosphorescent, and serve to attract minute organisms upon which the Sponge feeds. In regard to the canal-

system the authors' general conclusions are quite in accord with those of Vosmaer and Polejaeff. In the classification adopted the order is divided into two sub-orders, Halichondrina and Clavulina. The first of these is divided into four families: I. Homorrhaphidæ, II. Heterorrhaphidæ, III. Desmacidonidæ, and IV. Axinellidæ; the second into I. Suberitidæ, and II. Spirastrellidæ.

Over 200 species or well-marked varieties are described from the *Challenger's* dredgings, but all the new species were first diagnosed in the *Annals and Magazine of Natural History* during the course of last year (1886).

In reference to the geographical distribution, the authors remark that one cannot fail to notice the small number of stations at which these Sponges were found. Out of a total of 277 distinct stations, only 50 are represented as stations for these Monaxonids, and these are supplemented by 20 localities to which no station-number is attached; and these latter were not, it is to be assumed, deep-sea stations. We cannot agree with the suggestion that these forms were overlooked amongst the "rubbish" in sorting out the contents of the trawls and dredges; or with the idea that owing to their fragility they may have been destroyed. Doubtless the true explanation is that "the Monaxonida are not, on the whole, a predominant group in deep water."

While not a predominant group in deep water, still no less than 24 species were found at depths between 1000 and 2000 fathoms, while 46 occurred between depths of 200–1000 fathoms, and 140 species, or exactly double the previous number, were found at depths of from 0–200 fathoms.

The scarcity of Monaxonid Sponges at very great depths is somewhat compensated for by the unusual interest attaching to the species which do occur. Among other facts we find that, while the shallow-water forms are characteristically more or less shapeless in their external form or at the very most digitate or ramose, those from below the 1000-fathom depth have almost without exception beautifully symmetrical and definite shapes.

One of the most beautiful and extraordinary of the species described and figured is *Esperiopsis challengeri*, Ridley: it was taken in some quantity off the east of Celebes Island from a depth of 825 fathoms. From a slightly expanded attached base a slightly curved stem arises, which is composed of densely packed and firmly united stylote spicules: this stem is compressed laterally; numerous short simple branches arise from the concave edge at gradually increasing intervals, the longest of the internodes being at the top; the main stem and each of these branches terminate in fleshy sponge lamellæ, of which there may be six or seven in an apparently full-grown specimen. Each lamella presents the form of a deeply concave, transversely elongated cup: the oscula are confined to the convex surfaces of the lamellæ; the pores are found on the concave surfaces. This species is figured on Plate XVIII. Fifty-one plates accompany this Report.

The second Report in this volume is a supplement to Dr. L. von Graff's Report on the Myzostomida. It includes the description of seven new forms besides fourteen previously described species, all received from Dr. P. Herbert Carpenter; these were found by him while investigating the *Challenger* Crinoids. The author refers to the so-called cysts of *Antedon rosacea*, but declares that in no one case did he find therein any trace of a Myzostoma or any other encysted organism. On the contrary, both in the various pinnule deformities and in the arm swellings, he found a roundish brown foreign body, which was apparently the cause of the deformity. As to the nature or origin of this body nothing has been determined. Three plates of the new species and one of the cysts of *Antedon rosacea* accompany this Report.

The third Report is on *Cephalodiscus dodecalophus*, McInt., by Prof. W. C. McIntosh. This very remarkable new type of Polyzoon was dredged in the Strait of Magellan, and was, when first found, placed among the Compound Ascidians. The late Mr. Busk, Prof. Allman, and Prof. McIntosh, referred it to the Polyzoa. At first sight the flexible cœnœcium might easily be taken for a sea-weed, but it would seem to spread over the surface of the ground and not to grow erect. Of the numerous branches many anastomose; the general surface is spiny or fimbriated; the interior of the stem and branches contains an irregular series of wide canals. The Polypides are described as being perfectly free and at liberty to wander anywhere along the chambers, or even externally through the apertures. Each adult Polypide measures, from the extremity of the cephalic plumes to the tips of the pedicel, about 2 millimetres. Large buds in various stages of development arise from the Polypides. The twelve branchial plumes are very conspicuous. The author thinks that Cephalodiscus approaches Rhabdopleura. In an important appendix, by Mr. Sidney F. Harmer, the affinities of this form to Balanoglossus are ably pointed out, and he thinks that this genus (and perhaps Rhabdopleura also), must be removed from the Polyzoa and placed in Bateson's group of the Hemichordata. Seven plates and numerous woodcuts illustrate this Report.

Volume XXI. contains but one Report, that on the Hexactinellida, by Prof. F. E. Schulze, of the University of Berlin. This volume is issued in two parts, the first that of the text, comprising over 500 pages, and the second consisting of an atlas of 104 plates.

This is one of the most important of the fifty-three Reports hitherto published. This group of Sponges early attracted the special attention of the late Sir Wyville Thomson; and it was his intention to describe the Hexactinellids of the *Challenger* Expedition, but the work had scarcely been seriously commenced at the time of his death. It was fortunate that the services of Prof. Schulze were secured for the writing of this monograph, which is a most acceptable and welcome addition to our literature of this group.

In this Report, besides the collection made by the *Challenger*, the results of the previous cruises of the *Lightning*, *Porcupine*, *Knight Errant*, and *Triton* are also detailed, and the material has been further increased by a collection made at Japan by Dr. Döderlein.

The Hexactinellids are those forms of the Sponges in which the siliceous spicules belong to the triaxial type. Omitting the eighteenth century reference to a Sponge belonging to the genus Dactylocalyx, Dr. Gray was the first in the present century to describe some peculiar "glass rope-like" structures in the British Museum under the name of Hyalonema; though without recognizing until long afterwards their real affinities. This was in

1832, and in the following year Quoy and Gaimard figured and described their *Alcyonellum speciosum*. During the next twenty years only five more species were added to the list, the beautiful *Euplectella aspergillum*, Owen, being the most remarkable of these. The last twenty-five years have, however, witnessed an ever-advancing progress in our knowledge of these Sponges, thanks to the labours of Gray, Bowerbank, Wyville Thomson, Schmidt, Kent, Carter, Marshall, Sollas, and, above all, of Zittel, which labours have now culminated in the present Report.

It is scarcely to be wondered at that the beautiful glassy frame-work and the charmingly diversified spicules which form their "skeletons" have always attracted attention to these Sponges—an attraction that will be greatly intensified by the publication of this volume.

The Report opens with a general historical introduction, and then passes on to details of the forms and structures to be met with in the group: herein we find the nomenclature adopted for the spicules. This is followed by the description of the genera and species. It is pleasant to find in the synonymy and specific details that great pains have been taken to mention the work of all previous labourers in the field, and the author shows a due and kindly appreciation of what has been done by those who often had but little light to guide them on their way.

It is not easy to give any analysis of so elaborate a memoir, in which twenty new genera and sixty-five new species are described; while with scarcely an exception the numerous species already described are not only alluded to, but many fresh details are given about them. When it is recollected that but fourteen years ago only thirty species of this group of Sponges were known, the progress of our knowledge of them, it will be recognized, has been great.

These Sponges seem to be widely distributed in all the oceans; the largest number of forms—fifty-seven—being found in the Pacific Ocean; next comes the Atlantic Ocean, in which twenty-four species were found; while only sixteen were found in the South Indian Ocean; but it must not be forgotten that the South Indian Ocean has been very slightly investigated.

As to the bathymetrical distribution of the Hexactinellida, they would appear to be met with in depths of between 95 and 3000 fathoms, being more numerous between the depths of 100 and 900 fathoms, decreasing somewhat between those of 900 and 2500 fathoms, and again markedly diminishing between the depths of 2500 and 3000 fathoms, while below this depth no Hexactinellida Sponges have been found. Euplectellids seem to have a wide range, being met with at the moderate depth of 95 fathoms, and then being pretty evenly distributed down to a depth of 2750 fathoms. The four species of the new genus Holascus frequent great depths, varying from 1375 to 2650 fathoms. The maximum depth as yet known for any of these Sponges is that of 2900 fathoms, at which depth *Bathydorus fimbriatus* was found in the middle of the North Pacific Ocean.

It would be obviously impossible to give even a brief summary of the very remarkable new forms described in this splendid memoir of Prof. Schulze, and it is difficult to convey a correct notion of the beauty of the illustrations forming the large atlas of plates which accompanies the text. The diagnoses of the genera and the descriptions of the species are what one would expect from the well-known skill of the author.

We do not altogether agree with him when he writes that, "after a detailed investigation of a group of animals, it is incumbent on every naturalist who accepts the evolution theory to attempt the appreciation of his results in their relation to the phylogeny of the group." Looking at his array of facts, is it not possible for the thoughtful worker to bear in mind the incompleteness of the phylogenic record, and reverently to wait for more light? There may be nothing to object to in the stately genealogical tree of the Hexactinellida represented on p. 495, but is it not built up on but an incomplete and scanty framework?

One departure in this Report from the ordinary custom in the description of species we notice with regret—viz. that there is no synonymic list affixed to the species, neither are we referred, in connection with each form, to the place or places where it has been previously described. It seems scarcely necessary to point out the inconveniences attending such a style, or the great uncertainty it may on occasions give rise to. The volume opens at the description of *Rosella antarctica*, Carter. To find where it was first described by Mr. Carter we are obliged to refer to the synonymy affixed to the diagnosis of the genus; but here we get no certain information as to how many of the quotations given refer to this species; and this is of course much more confusing when we come to investigate a genus abounding with species, like, for example, Hyalonema. Indeed, by this method an author is very apt to overlook the fact that several writers may refer to the same species under quite different names, and a curious case of this nature, we suspect, occurs under Hyalonema. Dr. Gray, wrongly trusting to a Museum label, replaced the name *Hyalonema sieboldii*, which he had given to the first known species of this genus in 1835, by that of *H. mirabile*, under the impression that he had so named it in the "Synopsis of the Contents of the British Museum," 1832 (misprinted 1830), the year he had got the analysis of its glassy fibres from Mr. Pearsall. Depending on the accuracy of Dr. Gray, many authors referred to the species under this latter name; and further, for some time after the discovery of the Setubal species by Prof. Barboza du Bocage, this species, now known as *H. lusitanicum*, passed as the same species as *H. mirabile* = *H. sieboldii*. Even from the details given by Prof. Schulze, this seems clear; but in the description of Bocage's species (p. 225) no synonymic list is given, and not only does the before-mentioned fact not appear, but we find *H. lusitanicum* placed among those species "the upper end of which was not sufficiently preserved for deciding the question whether there is a sieve-plate or not." It is added that "neither on this specimen (the one figured in the Proc. Zool. Soc. Lond.) nor on others which Bocage afterwards obtained from the same locality could any portion of the sponge body be detected." But on p. 186 we find it stated that *H. lusitanicum* had been dredged from a depth of 480 fathoms south-west of Setubal, "bearing a sponge body with several oscular openings"; and again on the same page that among the Hyalonema found off the coast of Portugal by Barboza du Bocage and others, and named *H. mirabile*, there was one specimen

with an oval cup-shaped body about 8 inches in length and 4 inches in breadth, with a sieve-net on the upper truncated surface of the sponge body, extending evenly over the oscular opening and over the layer of the "spiculate cruciform spicules" in the net beams. We may further add that there were to be found in the Museum at Lisbon nearly a dozen specimens of Hyalonema which were taken at Setubal. "Most of them were preserved in spirits of wine; they were certainly the very finest collection of this remarkable Sponge in Europe. The largest had a stem about 18 inches in height; there were no parasites of any kind on it, and it was furnished with a sponge mass some 8 inches in diameter, and nearly as much in height. A second specimen was very curious, for here two apparently distinct individuals had become matted together: the two glass ropes were interlaced, and the two sponge masses had grown together" (Proc. Dublin Nat. Hist. Soc., vol. v., 1869). It would have been most important to have had the opinion of such an authority as Prof. Schulze as to whether all these specimens from Setubal are referable to Bocage's species; and whether, as we venture to think, Marshall's *H. thomsoni* may not be only a well-marked variety thereof. It is possible that by thus calling attention to the subject we may yet learn more of the treasures of the Museum of Lisbon, and nothing in these remarks can in the very slightest degree detract from the merits and importance of this splendid contribution to our knowledge of the vitreous Sponges. E. P. W.

THE FERN-ALLIES.

Hand-book of the Fern-Allies: A Synopsis of the Genera and Species of the Natural Orders Equisetaceæ, Lycopodiaceæ, Selaginellaceæ, Rhizocarpeæ. By J. G. Baker, F.R.S., F.L.S., First-Assistant in the Herbarium of the Royal Gardens, Kew. (London: George Bell and Sons, York Street, Covent Garden, 1887.)

AS the author states in the preface, "The present Handbook is planned upon the same lines as Hooker and Baker's 'Synopsis Filicum,' and the two, taken in connection, cover the whole series of the Vascular Cryptogamia." The total number of species described in the "Hand-book" is 566, and as we may now place the number of known ferns at about 3000, the fern-allies may be taken to represent about one-seventh of the recent Vascular Cryptogams. The fern-allies include only eleven genera, and about four-fifths of the species belong to the two genera *Selaginella* (335 species) and *Lycopodium* (94 species). The eleven genera are placed by Mr. Baker in four "natural orders," while the Filices form a fifth: three of these, Filices, Equisetaceæ, and Lycopodiaceæ, being isosporous; and two, Selaginellaceæ and Rhizocarpeæ, being heterosporous. In this way the relationship of the Rhizocarpeæ to the ferns is quite lost sight of; the Selaginellas and Lycopods are separated more widely than is desirable, and no place is left for the fossil heterosporous Equisetinæ. The arrangement adopted by Mr. Baker is very good for herbarium work; but for classificatory purposes it ignores certain palæontological facts which we cannot at the present day afford to overlook. Mr. Baker, however, does not deal with the fossil types, and now that we have such a complete account of the recent forms, let us hope that before long we may have as complete a synopsis of the fossil forms; a work which would be of the greatest interest and importance.

In regard to the geographical distribution of the fern-allies it is interesting to notice that *Equisetum*, *Isoëtes*, and *Pilularia* predominate in the North Temperate Zone. *Lycopodium*, *Psilotum*, *Selaginella*, *Salvinia*, and *Marsilea* are eminently tropical; and *Phylloglossum* is peculiar to the South Temperate Zone. Like the ferns, the fern-allies are best developed in the Tropics; and in the Tropics we also find the greatest number of peculiar species. Thus, out of the 566 species, 484 are met with in the Tropics of the Old and New World; and no less than 402, or 83 per cent., of these are peculiar to the Tropics. As with the ferns so also with the fern-allies, tropical America is richest in species, including 237 species, of which 212 are peculiar. The Southern Temperate Zone yields only 83 species, of which 42, or 51 per cent., are peculiar, the fern-allies being thus much less numerous than the ferns in the southern flora. In the North Temperate Zone 150 species are met with, and of these 48, or 32 per cent., are peculiar. The North Temperate Zone is thus, like the South, deficient in fern-allies as compared with ferns, and this is apparently due to the small number of fern-allies as yet reported from temperate Asia. Only 6 species occur in the Frigid Zone, and, like the ferns, represent about 1 per cent. of the whole, none of the species being peculiar.

It is difficult to realize the amount of labour and research that must have been spent upon the production of this book; but anyone who has attempted to study the genus *Selaginella* will appreciate the masterly manner in which Mr. Baker has dealt with the 335 species of the genus, more than one-fourth of which he has himself described for the first time. Most of the species of Selaginellaceæ and Rhizocarpeæ have been described by Mr. Baker in his papers on the subject which have appeared from time to time, since 1883, in the *Journal of Botany*, but several new species are described in "Fern-Allies" for the first time, recent additions to the rich treasures of Kew. It is to be regretted that Mr. Baker does not more particularly refer to his papers in the *Journal of Botany*, and it is hard to understand why, in the descriptions of *Marsilea concinna* and *M. condensata*, he has omitted the references to the *Journal of Botany*, 1886, pp. 179 and 281 respectively. Then in transferring the matter from the *Journal of Botany* he has altogether dropped out the habitat of *Azolla nilotica*. There are also in the book not a few misprints, and a want of care is shown in numbering and lettering the sections of *Selaginella*. The index is also not quite up to the mark, as in *Marsilea*, with numerous synonyms omitted, and the misprints in *Pilularia* and *Psilotum*. As the index of the "Synopsis Filicum" was published separately as a catalogue of ferns, we may perhaps be permitted to express a hope that this index will not be so published until it is carefully revised. All that is wanting, however, is only a little more careful editing, and the few faults in no way detract from the sterling value of the work.

As the only modern synopsis of the group, it is a work that must be in the hands of every botanist who deals with the Vascular Cryptogams, and it will be a lasting monument to Mr. Baker's critical accuracy and great power of dealing with a difficult set of plants. W. R. McNab.

OUR BOOK SHELF.

The Sailor's Sky Interpreter. By S. R. Elson. (Calcutta : Thacker, Spink, and Co., 1879)

THIS little book, which is written in verse, is a practical storm guide, dealing especially with the October cyclones in the Bay of Bengal. Many years of experience as pilot in the dangerous waters of the bay have made the author familiar with the phenomena of the weather in this part of the world. The details convey many a hint to students of Nature, and above all to navigators interested in the very violent storms which occur periodically at the change of the monsoons, and more especially about October at the close of the summer monsoon. The concluding stanza deals with the rules for avoiding the centre of a cyclone, and on this head the advice is both good and sound, and is at the same time put in a very concise form Sailors are very familiar with rhymes for the "Rules of the Road," but we can scarcely hope that the author's verse will be similarly mastered and remembered. Probably the author himself never contemplated such a use of his work ; but yet there are couplets and triplets of Admiral FitzRoy's which have lived for a quarter of a century, and are still valuable aids. In the last volume of the "Indian Meteorological Memoirs" full credit is given to Mr. Elson for his valuable observations on the False Point cyclone, and especial mention is made of the high value of his observations bearing on the movement of the clouds. The author possesses just that local knowledge which in a recent issue of the "Fishery Barometer Manual" the Meteorological Office lamented the want of among its observers around our coasts for the further perfecting of our "Weather Forecasts ;" and in the twenty stanzas which he has written he has pithily handed down his experiences for the benefit of his fellow-sailors.

Austral Africa. By John Mackenzie. Two Vols. (London : Sampson Low, 1887.)

THIS work, written by one who understands his subject thoroughly, ought to be cordially welcomed by all who have given any attention to the questions which have caused, during the last few years, so much trouble in South Africa. Mr. Mackenzie is convinced that these questions are not nearly so complicated and difficult as they are generally believed to be, and he has taken great pains to expound clearly and forcibly the policy which, in his opinion, would open up new markets for our commerce in South Africa, and secure the highest and best interests of the natives. The book is addressed rather to politicians than to persons interested in science, but students of the early forms of social institutions will find some statements worthy of their attention in Mr. Mackenzie's account of those native tribes with which he himself has come into contact. Archæologists will be interested, too, in what he has to say about the remarkable stone structures which are found over an extensive district to the east and north-east of Shoshong. These buildings, in the neighbourhood of which are the remains of ancient gold-mines, he compares with Persian towers of refuge and with the ancient round towers of Ireland and Britain.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

Medical Education at Oxford.

THE problem, how far the older Universities should undertake special training for the professions, is fast finding its own solution. A degree is no longer any evidence that its possessor has been through any course of wide general culture preparatory to his technical education. Recent legislation, both at Oxford and Cambridge, has all tended in the direction of enabling the undergraduate to specialize at the earliest possible point in his career. Whether advisable or not, some such movement seemed inevitable if, in the midst of the daily increasing pressure of competition, the Universities were to retain any hold on the educational development of the country. Even Prof. Freeman's articles in the *Contemporary Review* are marked by a tone of querulous despair, rather than by any hope that the tide of innovation may be checked. For knowledge as a luxury or an ornament there is neither leisure nor inclination. Cambridge was the first to yield ; but the multitudinous statutes which are every day promulgated at Oxford prove that the latter University is eagerly hurrying along the same path. New schools, new Boards of Faculties have been established ; old restrictions have been removed Large sums of money have been expended on new buildings, in which new professors may give instruction in arts and sciences unheard of by the last generation. All this has been done in order that the student may proceed as speedily as possible to those special researches which are to arm him for the battle of life.

The ordinary curriculum at Oxford is now so modified and subdivided that a Bachelor of Arts may have no more extensive general education than that smattering of school-boy knowledge required for the examination called Responsions. It is hardly realized by those who are chiefly responsible for this movement how much the whole life of the University must be altered by so radical a change in its methods and its aims. The statute-book, indeed, is in such a state of chaos that there are few, even among the officials, who can unravel the intricacies of any one Faculty. In the department of medicine an attempt has recently been made, in a pamphlet issued by the Clarendon Press, to afford concise and accurate information to the hitherto bewildered undergraduate. By means of this publication it is possible to trace out the curriculum of an Oxford medical student and to contrast the present with the older system of education. Responsions, or some equivalent test, can be passed while the candidate is still at school, and at the same time he can take an extra subject which will exempt him from the First Public Examination. After reaching the University, only an elementary pass examination in divinity will stand between him and his scientific work He may then give himself up to preparation for one of the Honour Schools of natural science. For this he will have to pass various "preliminaries," for which there are schedules of alarming proportions. Physiology and chemistry are suggested as the most suitable schools, as by their means exemption is gained from portions of the First M.B. Whichever he may select, two years of the most severe application are necessary in order to gain a satisfactory position in the Class List. He will then, in his third or fourth year, be enabled to take his B.A. degree. The study of human anatomy will next absorb his energies. The amplest opportunities are now afforded to those who desire to take up this subject while residing at Oxford. The ideal candidate depicted in this pamphlet is supposed to spend but one academical year in this department. Extraordinary, indeed, must be the powers of the teacher who could impart, and of the pupil who could receive, a sufficiently deep impression of so important a science in so brief a time. After the first examination for the M.B., residence in Oxford would come to an end, and the student would migrate probably to London. With everything in his favour he might be able to obtain his degree in six or seven years from the date of his matriculation. There are other ways in which the course of study might be arranged, but the details are of small consequence. It matters little to the public whether the degree can be obtained in five years or seven. In either case the professional acquirements will be above the average. The eminence of the examiners and the reputation of the University will be a sufficient guarantee that tests are applied of sufficient stringency to exclude the ignorant and incompetent. It is well, however, that the real state of affairs should be fairly recognized and understood by those who have been accustomed to attribute some special virtues to a University degree. It is important also to consider whether in leaving the older methods and yielding, however reluctantly, to the pressure of the hour, a retrograde step has not been taken in the history of medical education. It is always a loss when something even distantly approaching to an ideal is degraded to the level of every-day life.

The older Oxford system, if antiquated and imperfect, had

at least aims of a high and noble character—aims which could not fail to have an elevating effect on those by whom they were entertained. In former times to have taken a degree in arts, as a necessary preliminary to the beginning of a student's purely professional career, may not have meant, and, as a matter of fact, did not in the majority of cases mean, any very high standard of learning or culture. It did, however, carry with it some inestimable advantages which can never be attained under the existing system of specialization. It meant that a young man, while his mind was still plastic to all surrounding influences, was brought into contact with and joined in the same pursuits as fellow-students whose tastes would lead them to different pleasures, and whose circumstances would lead them to a variety of destinations. The physician or surgeon of the future became the companion of those who were afterwards to become clergymen, barristers, or schoolmasters. He read the same books, played the same games, belonged to the same clubs. In this way, however little actual knowledge he may have acquired, he gained an invaluable acquaintance with men's lives and habits. He formed friendships with men destined to follow very different careers. These associations could not fail to be of the greatest value to him in the pursuit of his special profession. I do not mean mere monetary advantage, but that derived from intercourse with men in other walks of life—that interchange of ideas so necessary to a healthy mind. Such a training must have been beneficial to all, but to the student of medicine it was an incalculable boon. Much of his success and much of the good he can hope to do depend on an intimate knowledge of mankind. Without that, no matter how highly trained he may be as a man of science, his acquirements will be of little avail, and his skill can never be used to the highest purpose.

How is the existing system likely to work in this direction? The student is advised and encouraged to enter at once on his special pursuits. He is to apply himself without delay to scientific study, associated with men like himself, plodding along the same track. If he aims at taking honours in natural science, he must curtail his exercise to the limits of a short "constitutional" and cut himself off from the common pleasures of the cricket-field and the river. His very social gatherings tend to consist more and more exclusively of men working in his own department. The Union and other such Clubs are given up for scientific Societies, where he thinks he can combine business with amusement. Such a life can hardly fail to narrow the most sympathetic mind, to hamper and confine the most commanding intellect; it is most unlikely to turn out a practitioner of the highest and most useful type. To live in a clique where priggishness is fostered by the worst kind of mutual admiration is hardly the ideal of University education. Fortunately the curriculum indicated in the pamphlet to which I have referred is not compulsory, and an intending medical student might not be altogether unwise if he decided to pass the first three years of his career in the ordinary pursuits of the University before turning his attention to more technical studies. Even the delay of a year or two would be more than counterbalanced to some by the benefits which such a course would undoubtedly confer.

Oxford. GEORGE I. WILSON.

Migration of Swallows along the Southern Coast.

THE following notes were made by me during a short stay at Lulworth, twelve miles east of Weymouth, from September 16 to 26. They may be of interest to some of your readers, as I have not been able to find the facts I observed recorded in any work on British birds.

When I arrived at Lulworth on the 16th, swallows and house-martins were about, but in no great numbers. On the 19th, in the course of a walk, I observed a few swallows apparently moving eastwards; and this caused me to spend the next morning on the top of a high and narrow ridge of down (Bindon Hill), running parallel with the sea—an admirable position for observation, as the movements of all birds were discernible from it at a long distance. The wind was east-north-east, and the air cold and very clear.

In half-an-hour it became clear to me that a general migration of swallows and martins was taking place along the coast in an easterly direction. The air would be thick with birds over my head for two or three minutes; then for a considerable interval hardly a bird would be visible. An ordinary glance at these dense parties was not enough to prove that they were travelling, or to show in which direction they were going; but by keeping the eye steadily upon them for some little time, and bringing the field-glass to bear on them when the eye failed, it became obvious that they were going east at a steady rate of speed, and apparently following the long spine of chalk down on which I stood, which extends from near Weymouth as far as Poole Harbour. The migration on this large scale lasted during the whole of that morning; in the afternoon the parties did not seem so large.

The next day (the 21st) a strong east wind was blowing, and the birds were not travelling high in air, but creeping steadily along the flanks of the down, and on the lower ground north and south of it. They were continually tacking, but every individual that I followed with my glass was moving swiftly towards the east. Those that were on the southern or seaward side of the down would come upon the sea at one point where the coast turns sharply northwards for a short distance: they did not attempt, however, to leave the land, but turned northwards with the coast, and pursued their way along the heights. On the 22nd and 23rd the same thing went on, but the numbers of the birds seemed to diminish, and they no longer went in parties that were plainly discernible. All this time there were a very few stationary swallows in one or two warm corners by the seaside.

From Dorset I went to Devonshire on the 26th. At Crediton and at Bideford (both warm and sheltered towns), I did not see a dozen swallows or martins in the course of a week; but I learnt that they had gathered for departure a few days before. I have since been informed that the gatherings had been noticed in Cornwall in the first week of the month. I infer that the migration I saw at Lulworth was that of the extreme West of England birds, who were proceeding along the coast to the point at which they cross the Channel. I should be glad to know where that point is.

I had reason to believe that one or two other species were moving up regularly in the same direction. The well-known migration of the pied wagtail was apparently over; but the large number of gray wagtails in a district almost destitute of water was very striking, and, as far as I could see, these also were passing eastwards. But I hope to make further observations next year.

I may add that, on returning to my home in Oxfordshire in the first week of October, I found swallows and martins passing over my village in parties during the earlier hours of each day; but, owing to the want of a convenient elevated position for watching, it was much more difficult to follow their movements than it had been at Lulworth. W. WARDE FOWLER.

Swifts.

THOUGH I cannot add anything to the interesting and valuable evidence given by your correspondent, in your last issue, with regard to swifts remaining on the wing during the dark hours of a summer night, it reminds me of a most beautiful exhibition of their flight which I witnessed at Moscow this last summer. It was on August 2, as the last rays of the setting sun were lighting up the domes and cupolas of that wonderful city, which we gazed upon from the heights of the Kremlin for the first time, that we noticed hundreds of these birds wheeling round their summits or darting hither and thither in every direction. At the same time the matchless Russian bells were pealing forth from every bell-tower in honour of the Empress's birthday, which was to be celebrated on the morrow, and it was surely difficult to believe that the swifts were not revelling in the music like ourselves, especially as I cannot remember ever seeing them again in such numbers, though our visit to Moscow was prolonged for ten days, and we frequently visited the Kremlin at the same hour. E. BROWN.

Further Barton, Cirencester, October 29.

Hughes's Induction Balance.

THE points noted by Mr. Cook on page 605 (vol. xxxvi.), are merely the well-known facts that a magnetic body has most effect when presented to the coils end-ways, *i.e.* with its greatest dimension along their axis, whereas a substance which acts mainly by conduction has most effect when presented flat-ways, or parallel to their face. Any possible effect due to diamagnetism is far too small to be thus easily noticed. OLIVER J. LODGE.

The Ffynnon Beuno and Cae Gwyn Caves.

I THINK it would be well for geologists and anthropologists to allow the age of the deposits and stone instruments found at these caves to remain an open question for the present. At present I have had no opportunity of seeing any papers on the subject, and I know of no opinions other than the one expressed by Dr. Hicks, NATURE, vol. xxxvi. p. 599. I am however fairly well acquainted with the glacial deposits of North Wales and with Palæolithic implements, and I have seen the caves and the tools found at and in them. My quite unbiased opinion is and will so remain,—unless I get very convincing proof to the contrary,—that the drift at the caves has been without doubt relaid; and is no more a true glacial deposit than the valley gravels of the Thames. As for the tools—one in the British Museum (South Kensington), and one in Denbighshire—they belong to the *very latest* of Palæolithic times, and might be passed for Neolithic; the Denbighshire example seen by me is a knife-flake with fine secondary chipping up one edge.

Dunstable. WORTHINGTON G. SMITH.

SYNTHESIS OF GLUCOSE.

ANOTHER important acquisition to our store of knowledge has recently been made. Glucose, commonly called grape-sugar, has been artificially prepared by Drs. Emil Fischer and Julius Tafel in the chemical laboratory of the University of Würzburg. This happy achievement, which is announced in the number of the *Berichte* just received, is one which has long been looked forward to, and which cannot fail to give deep satisfaction in chemical circles all over the world. As is generally the case in syntheses of this description, not only has the sugar itself been actually prepared, but, what is at least quite as important, considerable light has been thrown upon that much-discussed question—the constitution of sugars. A most remarkable, and yet only to be expected, attribute of this artificial sugar is that it is found to be entirely incapable of rotating a beam of polarized light. As is well known, there are several naturally-occurring varieties of glucose, all of which may be expressed by the same empirical constitution $C_6H_{12}O_6$, and all possessing the power of rotating the plane of polarization: dextrose, or grape-sugar, the best-known of these varieties, as its name implies deviates the plane of polarization to the right, as do several other less important varieties; while lævulose, or fruit-sugar, rotates the plane to the left. But in artificially preparing a glucose of the composition $C_6H_{12}O_6$ there is just as much tendency for one kind to be formed as another, and the probability is that both dextro and lævo are simultaneously formed, and thus neutralize each other, producing a totally inactive mixture. It may be that, as in the case of racemic acid, the two kinds are formed side by side and neutralize each other in the solution; or it may even be that, as is the case with truly inactive tartaric acid, there is a true neutralization within the molecule itself; which of these hypotheses is correct is a question for further work to decide.

The substance employed as the base of operations was acroleïn, $CH_2{=}CH{-}CHO$, the aldehyde derived by oxidation of allyl alcohol. The acroleïn was first converted to its dibromide, $CH_2Br \cdot CHBr \cdot CHO$, which was then treated with cold baryta water, whereupon the bromine was removed by the barium leaving the artificial sugar in solution. The real difficulty was found to be in the isolation of the sugar, but this was eventually overcome by the use of phenyl hydrazine, $C_6H_5 \cdot NH \cdot NH_2$, which forms a hydrazine compound of the formula $C_{18}H_{22}N_4O_4$ with the new sugar, very similar to the compounds formed by phenyl hydrazine with ordinary dextrose and lævulose. This phenyl hydrazine compound was then found to yield by reduction a base $C_6H_{13}NO_5$, which, on treatment with nitrous acid, parted with its nitrogen and left a syrupy substance, possessing all the properties of sugars, and distinguished only from ordinary grape-sugar by its optical inactivity.

The actual operations were performed briefly as follows:—

Seventy-five grammes of pure crystallized barium hydrate were dissolved in a little over a litre of water, and 50 grammes of previously redistilled acroleïn dibromide added drop by drop, the flask being continuously agitated, surrounded by ice-cold water, for about an hour. In a similar manner eight successive quantities were treated until in all about 400 grammes of acroleïn dibromide had been converted into sugar. These eight separate portions were then mixed, slightly acidified with sulphuric acid, and the barium precipitated with a solution of sodium sulphate. After removal of all the barium by filtration the solution was neutralized with soda and evaporated down to 1½ litres. On cooling, a solution of 50 grammes of the hydrochloride of phenyl hydrazine and 50 grammes of crystallized sodium acetate in 100 cubic centimetres of water were added; after standing twelve hours a reddish-brown resin separated out and was removed by filtration. 150 grammes more of phenyl hydrazine hydrochloride and the same quantity of sodium acetate were then added, and the solution warmed upon a water-bath; after again standing some time the solution became turbid, and in course of four hours a dark-coloured precipitate, partly crystalline and partly resinous, separated out. After washing and drying, and subsequent agitation with ether and trituration with alcohol to remove organic impurities, and extraction of the inorganic salts by hot water, the phenyl hydrazine compound was finally isolated.

Analysis of the recrystallized compound indicates that its composition is $C_{18}H_{22}N_4O_4$, and its properties are very similar to those of the phenyl hydrazine compound of ordinary grape-sugar, the melting-points of the two bodies being identical, 205° C. It is almost insoluble in water, ether, and benzene, and only with difficulty soluble in hot alcohol; it is more soluble in glacial acetic acid, but the solution soon becomes dark red. It crystallizes from hot alcohol in pretty little prism aggregates, while the ordinary grape-sugar compound crystallizes in spherical aggregates of fine needles. It is further distinguished from the latter compound inasmuch as a layer 20 cubic centimetres thick, is without action upon a beam of polarized light.

When reduced by means of zinc dust and acetic acid, a base was produced analogous to the one formed by the reduction of the corresponding phenyl hydrazine compound of grape-sugar. This base was difficult to isolate, owing to the non-crystallizable nature of its acetate; the fact was fortunately discovered, however, that its oxalate was crystalline, and readily obtained pure. Hence its analysis has been effected, and the numbers found point to the composition $(C_6H_{13}NO_5)_2 \cdot C_2H_2O_4$. This base reduces Fehling's solution strongly on warming, and with phenyl hydrazine regenerates the parent compound; but, once again, is optically inactive.

Finally, by the action of nitrous acid, nitrogen at once began to be evolved, and when the evolution ceased the liquid was neutralized with soda, evaporated *in vacuo*, and the residue extracted with alcohol. On evaporation of the alcohol the sugar was left as a bright brown syrup, free from nitrogen and ash, of sweet taste, and capable of instantly reducing Fehling's solution.

Up to the present time two hypotheses as to the constitution of sugars have pretty evenly balanced each other. According to one, sugars are considered, in virtue of their power of reducing ammoniacal silver solutions, as aldehydes containing also alcohol groups; on these lines grape-sugar would be formulated, $CH_2OH{-}(CHOH)_4{-}CHO$. But it has since been shown that the property of reducing ammoniacal silver solutions is not confined to aldehydes, for the series of bodies known as ketone alcohols also possess it; hence grape-sugar may also be written $CH_2OH{-}(CHOH)_3{-}CO{-}$.

CH_2OH. Both theories account for most of the hitherto known reactions of the glucoses, hence the matter has remained an open question. Drs. Fischer and Tafel, however, consider that their synthesis from acroleïn, which is itself an aldehyde, points to the probability of the former hypothesis being the correct one. The action of baryta water upon the dibromide evidently causes a simple exchange of bromine for hydroxyl, and the first product of the reaction is almost as certainly glycerine aldehyde, $CH_2OH—CHOH—CHO$. This latter substance, however, appears to polymerize at once under the influence of the baryta water into sugar, two molecules of glycerine aldehyde uniting to form a molecule of glucose.

In consideration of the fact of its derivation from acroleïn, the name acrose has been applied to the sugar which has been, with so much skill and steady determination, synthetically formed and isolated; and there can be no doubt that this name will stand as a memento of the progress made in organic chemistry during the year 1887.

A. E. TUTTON.

MODERN VIEWS OF ELECTRICITY.[1]

PART II.—CURRENT ELECTRICITY (*continued*).

IV.

Electrical Inertia.

RETURNING now to the general case of conduction, without regard to the special manner of it, we must notice that, if a current of electricity is anything of the nature of a material flow, there would probably be a certain amount of inertia connected with it, so that to start a current with a finite force would take a little time; and the stoppage of a current would also have either to be gradual or else violent. It is well known that if water is stagnant in a pipe it cannot be quite suddenly set in motion; and again, if it be in motion, it can only be suddenly stopped by the exercise of very considerable force, which jars and sometimes bursts the pipe. This impetus of running water is utilized in the water-ram. It must naturally occur, therefore, to ask whether any analogous phenomena are experienced with electricity; and the answer is, they certainly are. A current does not start instantaneously: it takes a certain time—often very short—to rise to its full strength; and when started it tends to persist, so that if its circuit be suddenly broken, it refuses to stop quite suddenly, and bursts through the introduced insulating partition with violence and heat. It is this ram or impetus of the electric current which causes the spark seen on breaking a circuit; and the more sudden the breakage the more violent is the spark apt to be.

The two effects—the delay at making circuit, and the momentum at breaking circuit—used to be called "extra-current" effects, but they are now more commonly spoken of as manifestations of "self-induction."

We shall understand them better directly; meanwhile they appear to be direct consequences of the inertia of electricity; and certainly if electricity *were* a fluid possessing inertia it would behave to a superficial observer just in this way.

But if an electric current really possessed inertia, as a stream of water does, it would exhibit itself not only by these effects but also mechanically. A conducting coil delicately suspended might experience a rotary kick every time a current was started or stopped in it; and if a steady current were maintained in such a coil it should behave like a top or gyrostat, and resist any force tending to deflect its plane.

Clerk Maxwell has carefully looked for this latter form of momentum effect, and found none. One may say, in fact, that nothing like momentum has yet been observed in an electric current by any *mechanical* mode of examination. A coil or whirl of electricity does not behave in the least like a top.

Does this prove that a current has no momentum? By no means necessarily so. It might be taken as suggesting that an electric current consists really of two equal flows in contrary directions, so that mechanically they neutralize one another completely, while electrically—*i.e.* in the phenomena of self-induction or extra-current—they add their effects. Or it may mean merely that the momentum is too minute to be so observed. Or, again, the whole thing—the appearance of inertia in some experiments and the absence of it in others—may have to be explained in some altogether less simple manner, to which we will proceed to lead up.

Condition of the Medium near a Circuit.

So far we have considered the flow of electricity as a phenomenon occurring solely inside conductors; just as the flow of water is a phenomenon occurring solely inside pipes. But a number of remarkable facts are known which completely negative this view of the matter. Something is no doubt passing along conductors when a current flows, but the disturbance is not *confined* to the conductor; on the contrary, it spreads more or less through all surrounding space.

The facts which prove this have necessarily no hydraulic analogue but must be treated *suorum generum*, and they are as follows:—

(1) A compass needle anywhere near an electric current is permanently deflected so long as the current lasts.

(2) Two electric currents attract or repel one another, according as they are in the same or opposite directions.

(3) A circuit in which a current is flowing tends to enlarge itself so as to inclose the greatest possible area.

(4) A circuit conveying a current in a magnetic field tends either to enlarge or to shrink or to turn half round according to the aspect it presents to the field.

(5) Conductors in the neighbourhood of an electric circuit experience momentary electric disturbances every time the current is started or stopped or varied in strength.

(6) The same thing happens even with a steady current if the distance between it and a conductor is made to vary.

(7) The effects of self-induction, or extra-currents, can be almost abolished by twisting the covered wire conveying the current closely on itself, or even by laying the direct and return wire side by side; whereas they may be intensified by making the circuit inclose a large area, more by coiling it up tightly into close coil, and still more by putting a piece of iron inside the coil so formed.

Nothing like any of these effects is observable with currents of water; and they prove that the phenomena of the current, so far from being confined to the wire, spread out into space and affect bodies at a considerable distance.

Nearly all this class of phenomena were discovered by Ampère and by Faraday, and were called by the latter "current-induction." According to his view the dielectric medium round a conducting circuit is strained, and subject to stresses, just as is the same medium round an electrically charged body. The one is called an electrostatic strain, the other an electro-magnetic or electro-kinetic strain.

But whereas electrostatic phenomena occur *solely* in the medium—conductors being mere breaks in it, interrupters of its continuity, at whose surface charge-effects occur but whose substance is completely screened from disturbance—that is not the case with electro-kinetic phenomena. It would be just as erroneous to conceive electro-kinetic phenomena as occurring solely in the insulating medium as it would be to think of them as occurring solely in the conducting wires. The fact is, they occur in

[1] Continued from vol. xxxvi. p. 585.

both—not only at the surface of the wires like electrostatic effects, but all through their substance. This is proved by the fact that conductivity increases in simple proportion with sectional area; it is also proved by every part of a conductor getting hot; and it is further proved in the case of liquids by their decomposition.

But the equally manifest facts of current attraction and current induction prove that the effect of the current is felt throughout the surrounding medium as well, and that its intensity depends on the nature of that medium; we are thus wholly prevented from ascribing the phenomenon of self-induction or extra-current to simple and straightforward inertia of electricity in a wire like that of water in a pipe.

We are thus brought face to face with another suggestion to account for these effects, viz. this: Since the molecules of a dielectric are inseparably connected with electricity, and move with it, it is possible that electricity itself has no inertia at all, but that the inertia of the atoms of the displaced dielectric confer upon it the appearance of inertia. Certainly they do sometimes confer upon it this appearance, as we see in the oscillatory discharge of a Leyden jar. For a displaced thing to overshoot its mean position and oscillate till it has expended all its energy, is a proceeding eminently characteristic of inertia; and so, perhaps, the phenomena of self-induction are similarly, though not so simply, explicable.

Further consideration of this difficult part of the subject is however best postponed to Part III.

Energy of the Current.

I have now called attention to the fact that the whole region surrounding a circuit is a field of force in which many of the most important properties of the current (the magnetic, to wit) manifest themselves. But directly we begin thus to attend to the whole space, and not only to the wires and battery, a very curious question arises. Are we to regard the current in a conductor as propelled by some sort of end-thrust, like water or air driven through a pipe by a piston or a fan, or are we to think of it as propelled by side forces, a sort of lateral drag, like water driven along a trough by a blast of air or by the vanes of paddle-wheels dipping into it? Or, again, referring to the cord models, Figs. 5, 6, and 13, were we right in picturing the driving force of the battery as located and applied where shown in the diagrams, or ought we to have schemed some method for communicating the power of the battery by means of belts or other mechanism to a great number of points of the circuit?

Prof. Poynting has shown that, on the principles developed by Maxwell, the latter of these alternatives, though apparently the more complicated, is the true one; and he has calculated the actual paths by which the energy is transmitted from the battery to the various points of a circuit, for certain cases.

We must learn, then, to distinguish between the flow of *electricity* and the flow of electric *energy*: they do not occur along the same paths. Hydraulic analogies, at least hydraulic analogies of a simple kind, break down here. When hydraulic power or steam power is conveyed along pipes, the fluid and its energy travel together. Work is done at one end of the tube in forcing in more water, and this is propagated along the tube and reappears at the distant end as the work of the piston. But in electricity it is not so. Electric energy is not to be regarded as pumped in at one end of a conducting wire, and as exuding in equal quantities at the other. The *electricity* does indeed travel thus—whatever the travel of electricity may ultimately be found to mean—but the energy does not. The battery emits its energy, not to the wire direct, but to the surrounding medium; this is disturbed and strained, and propagates the strain on from point to point till it reaches the wire and is dissipated. This, Prof. Poynting would say, is the function of the wire: it is to dissipate the energy crowding into it from the medium, which else would take up a static state of strain and cease to transmit any more. It is by the continuous dissipation of the medium's energy into heat that continuous propagation is rendered possible.

The energy of a dynamo does not therefore travel to a distant motor through the wires, but through the air. The energy of an Atlantic cable battery does not travel to America through the wire strands, but through the insulating sheath. This is a singular and apparently paradoxical view, yet it appears to be well founded.

Think of a tram-car drawn by an underground rope, like those in the streets of Chicago or Hampstead Hill. A contact piece of iron protrudes from the bottom of the car and grips the moving rope, which is thus enabled to propel the car. How does the energy of the distant stationary engine reach the car? *Viâ* the rope and the iron connector, undoubtedly. They both have to be strong, and are liable to be broken by the transmitted stress.

Next, think of an electric tram-car driven by means of a current taken up from an underground conductor, like that of Mr. Holroyd Smith at Manchester, or at the late Inventions Exhibition. A contact piece of wire rope protrudes from the bottom of the car and drags a little truck along the conductor, which is thus enabled to supply electricity to the electro-magnetic motor geared to the wheels. How does the energy of the distant dynamo reach the car in this case? *Not viâ* the wire connector; not even *viâ* the underground conductor. It travels from the distant dynamo through the general insulating medium between cable and earth, some little enters the conductor and is dissipated, but the great bulk flows on and converges upon the motor in the car, which is thus propelled. All the energy of the conducting wire is dissipated and lost as heat: it is the energy of the insulating medium which is really transmitted and utilized.

Phenomena peculiar to a Starting, or Stopping, or Varying Current.

There is a remarkable fact concerning electric currents of varying strength, which has been lately brought into prominence by the experimental skill of Prof. Hughes, viz. that a current does not start or stop equally and simultaneously at all points in the section of a conductor, but starts at the outside first. This fact is naturally more noticeable with thick wires than with thin, and it is especially marked in *iron* wires, for reasons which in Part III. will become apparent; but the general cause of it in ordinary copper wires can very easily be perceived in the light of the views of Prof. Poynting just mentioned.

For, remember that a current in a wire is not pushed along by a force applied at its end, so as to be driven over obstacles by its own momentum combined with a *vis a tergo*; but it is urged along at every point of its course by a force just sufficient to make it overcome the resistance there, and no more, the force being applied to it through the medium of the dielectric in which the wire is immersed. A lateral force it is which propels the electricity; and it naturally acts first on the outer layers of the wire or rod, only acting on the interior portions through the medium of the outside.

To illustrate this matter further, rotate a common tumbler of liquid steadily for some time and watch the liquid; dusting powder perhaps over it to make it more visible. You will see first the outer layer begin to participate in the motion, and then the next, and then the next, and so on, until at length the whole is in rotation. Stop the tumbler, and the liquid also begins gradually to stop by a converse process.

If the liquid sticks together pretty well, like treacle, the motion spreads very rapidly: this corresponds to a

poor conductor. If the liquid be very mobile, the propagation of motion inward is slow: this corresponds to a very good conductor. If the liquid were perfectly non-viscous, it would correspond to a perfect conductor, and no motion would ever be communicated to it deeper than its extreme outer skin.

Think now of a long endless tube full of water, say the hollow circumference of a wheel, and spin it: the liquid is soon set in rotation, especially if the tube be narrow or the liquid viscous; but it is set in motion by a lateral not an end force, and its outer layers start first.

Just so is it with a current starting in a metal wire. If the wire be fine, or its substance badly conducting, it all starts nearly together; but if it be made pretty thick, and of well conducting substance, its outer layers may start appreciably sooner than the interior. And if it were infinitely conducting, no more than the outer skin would ever start at all.

In actual practice the time taken for all the electricity in an ordinary wire to get into motion is excessively short—something like the thousandth of a second—so that the only way to notice the effect is to start and reverse the current many times in succession.

If the hollow-rimmed wheel above spoken of were made to oscillate rapidly, it is easy to see that only the outer layers of water in it would be moved to and fro; the innermost water would remain stationary; and accordingly it would appear as if the tube contained much less water than it really does. The virtual bore of the pipe would, in fact, for many purposes be diminished. So is it also with electricity; the sectional area of a wire to a rapidly alternating current is virtually lessened so far as its conducting power is concerned; and accordingly its apparent resistance is slightly higher for alternating than for steady currents. The effect is however too small to notice in practice except with thick wires and very rapid alternations.

By splitting up the conductor into a bundle of insulated wires, thus affording the dielectric access to a considerable surface of conductor, the force is applied much more thoroughly, and so the effect spoken of is greatly lessened. The same thing is achieved by rolling out the conducting-rod into a flat thin bar. Making the conductor hollow instead of solid offers no particular advantage, because no energy travels *viâ* the hollow space, it still arrives only from the outside; *unless*, indeed, the return part of the circuit is taken along the axis of the hollow like a telegraph cable. In this last arrangement all the energy travels *viâ* the dielectric between the two conductors, and none travels outside at all. It will be perceived therefore that, as in static electricity, the term "outside" must be used with circumspection: it really means that side of a conductor which faces the opposite conductor across a certain thickness of dielectric.

We learn from all this that, whereas in the case of steady currents the sectional area and material of a conductor are all that need be attended to, the case is different when one has to deal with rapidly alternating currents, such as occur in a telephone, or, again, such as are apt to occur in a Leyden-jar discharge (see Part I., p. 560), or in lightning.

In all these cases it is well to make the conductor expose considerable surface to the propelling medium—the dielectric—else will great portions of it be useless.

Hence, a lightning-conductor should not be a round rod, but a flat strip, or a strand of wires, with the strands as well separated as convenient: and though I have not yet mentioned the special effect of iron, I may as well say here that iron is about 90,000 times worse than copper for the purpose of a lightning-conductor in respect of the phenomenon just described, seven times as bad on account of its inferior conducting power, and about twice as good as copper because of its higher melting-point and specific heat.

The Question of Electrical Momentum again.

We are now able to return to the important question whether an electric current has any momentum or not, as it would have if it were a flow of material liquid. Referring to Part I. (p. 533), a hint will be found that the laws of flow of a current in conductors—the shape of the stream-lines, in fact—are such as indicate no inertia, or else no friction. Now Ohm's law shows that at any rate *friction* is not absent from a current flowing through a metal; hence it would appear at first sight as if *inertia* must be absent.

The stream-lines bear upon the question in the following kind of way. If an obstacle is interposed in the path of a current of water, the motion of the water is unsymmetrical before and behind the obstacle. The

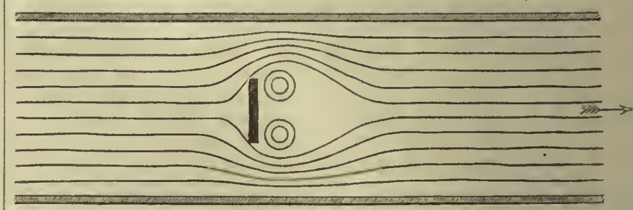


FIG. 14.—Stream-lines of water flowing through a pipe with an obstruction in it.

stream-lines spread out as the water reaches the obstacle, and then curl round it, leaving a space full of eddies in its wake (Fig. 14).

But if one puts an obstacle in the path of an electric current—say by cutting a slit in a conducting strip of tinfoil—the stream-lines on either side of it are quite symmetrical, thus—

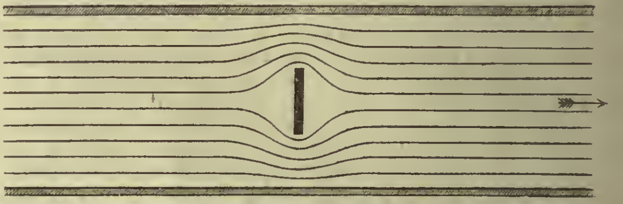


FIG. 15.—Electrical stream-lines past an obstacle.

And this is exactly what would be true for water also, if only it were devoid either of friction or of inertia, or of both.

Is not this fact conclusive, then? Does it not prove the absence of momentum in electricity?

Plainly the answer must depend on whether there is any other possible mode of accounting for this kind of flow. And there is.

For suppose that water, instead of being urged by something not located at or near the obstacle—instead of being left to its own impetus to curl round or shoot past as it pleases—suppose it were propelled by a force acting at every point of its journey, a force just able to drive it at any point against the friction existing at that point and no more; then the flow of water would take place according to the electrical stream-lines shown in Fig. 15.

An illustration of such a case is ready to hand. Take a spade-shaped piece of copper wire or sheet, heat it a little, and fix it in quiescent smoky air; looking along it through a magnifier in a strong light you will see the warmed air streaming past the metal according to the stream-lines of Fig. 15; and this just because the moving force has its location at the metal surface, and not in some region below it. (See Lord Rayleigh, NATURE, vol. xxviii. p. 139). One cannot indeed say that it is propelled at every

point of its course, but it is propelled at the critical points where the special friction occurs, and this comes to sufficiently the same thing.

We learn, therefore, that stream-lines like Fig. 15 prove one of three things, not one of two; and the three things are: (1) that the fluid has no friction; or (2) that it has no inertia; or (3) that it is propelled at every point of its course.

If any one of these is true of electricity, there is no need to assume either of the others in order to explain the actual manner of its flow. Now we have just seen that, according to Prof. Poynting's interpretation of Maxwell's theory, the third of the above is true—electricity is propelled at every point of its course; consequently, as said in Part I. (p. 533), the question of its inertia so far remains completely open.

Voltaic Battery.

Leaving this singular mode of regarding the subject for the present, to return to it perhaps after Part III., let us proceed to ask how it comes about that a common battery or a thermopile is able to produce a current.

If we allow ourselves to assume the existence of an unexplained chemical attraction between the atoms of different substances, an explanation of the action of an ordinary battery cell is easy. You have first the liquid containing, let us say, hydrogen and oxygen atoms, free or potentially free—that is, either actually dissociated or so frequently interchanging at random from molecule to molecule that the direction of their motion may be guided by a feeble directive force. Each of these atoms in the free state possesses a charge of electricity—the hydrogen all a certain amount of positive electricity, the oxygen twice that amount of negative. Into this liquid you then plunge a couple of metals which attract these atoms differently: for instance, zinc and copper, which both attract oxygen, but zinc more than copper; or, better, zinc and platinum, the latter of which hardly attracts it at all; or, better still, zinc and peroxide of lead, one of which attracts oxygen, the other hydrogen.

Immediately, the free oxygen atoms begin moving up to the zinc, the free hydrogen atoms to the other plate.

When one speaks of the plates attracting the atoms, it is not necessary to think of their exerting a force on all those in the liquid, distant and near: all that is necessary is to assume a force acting on those which come within what is called "molecular range" of its surface—a distance extremely minute, and believed to be about the ten-millionth part of a millimetre. If the zinc plate removes and combines with all the oxygen atoms which come within this range, they will be speedily replaced by others from the next more distant layer by diffusion, and these again by others, and so on. And thus there will be a gradual procession of oxygen atoms all through the liquid towards the zinc, the rate of the procession being regulated by the force acting, and by the rate of diffusion possible in the particular liquid used. All the atoms which reach the zinc neutralize a certain portion of its electricity by means of the positive charge they carry, and thus very soon it would become positively electrified enough to neutralize its attractive power on the similarly charged oxygen atoms, and everything would stop. But if a channel for the escape of its electricity be provided by leading a wire from it to the copper plate, the circuit is completed, the electricity streams back by the wire, and the procession goes steadily on. The electricity thus imparted to the copper, or platinum, neutralizes any repulsion it exerted on the negatively charged hydrogen atoms, and makes them in a similar way begin a procession towards it, deliver up their charges to it, combine with each other, and escape as gas.

Without going into all the niceties possible, this mode of thinking of the matter at least calls attention to some of the more salient features of a battery.

If, instead of two different plates, plates of the *same* metal be immersed, they will need to be oppositely electrified by some means before they are able to cause the two opposite processions, and so maintain a current in the liquid. This plainly corresponds to a voltameter.

Taking advantage of the known fact that the atoms are charged, Helmholtz avoids the necessity for postulating any chemical (non-electrical) force between zinc and oxygen, by imagining that all substances have a specific attraction for electricity itself, and that zinc exceeds copper and the other common metals in this respect.

He would thus think of the zinc attracting, not the oxygen itself, but its electric charge; and so would liken a battery cell still more completely to a voltameter. The polarization or opposition force acting at the hydrogen-evolving plate he would account for by the attraction of hydrogen for negative electricity, and the consequent repugnance of the hydrogen atoms to part with their charges.

Thermo-electric Pile.

A thermopile may be thought of in the following way, but in trying to understand the nature of these actions at present one must admit that some speculation and vagueness exist.

We have seen that when electricity is propelled through or among the molecules of a metal it experiences a certain resistance or opposition force which is exactly proportional to the speed of its motion. In other words, there is a connexion between matter and electricity in many respects analogous to fluid friction but varying accurately as the first power of the relative velocity. Hence, if an atom of matter be vibrating about a fixed point, it will tend to drive electricity to and fro with it; but if it be only one of a multitude, all quivering in different phases, they will none of them achieve any propulsion. This may be considered the state of an ordinary warm solid. But if from any cause a set of atoms could be made to move faster in one direction than in the reverse direction—to move forwards quickly and backwards slowly—then such an unsymmetrically-moving set *will* exert a propulsive tendency and tend to drive a current of electricity forwards, simply because the force exerted is proportional to the velocity, and so is greater on the forward journey than on the return.

Wherever conduction of heat is going on along a substance the atoms are in this condition. They are driven forward infinitesimally quicker, by the more rapidly moving atoms at the hot end, than they are driven back by the less rapidly moving atoms in front. And hence such a slope of temperature exerts a propulsive tendency: there is an electromotive force in a substance unequally heated.

This fact was discovered theoretically and verified experimentally by Sir William Thomson.

But not only is there such a force at a junction of a hot and cold substance, there is also a force at the junction of two substances of different kinds, even though the temperature be uniform. It is not quite so easy to explain how it now comes about that the atoms at this kind of junction are moving faster one way than the other; nevertheless, such a thing is not unlikely, considering the state of constraint and accommodation which must necessarily exist at the boundary surface of two different media. However it be caused, there is certainly an E.M.F. at such a junction.

Thus, then, in a simple circuit of two metals, with their junctions at different temperatures, there are altogether four electromotive forces—one in each metal, from hot to cold or *vice versâ*, and one at each junction; and the current which flows round such a circuit is propelled by the resultant of these four.

But the contact force at a junction is by no means confined to metals. It occurs between insulators also, and it is to it that the striking effects produced by all frictional electric machines are due.

By thus noticing that the connexion between matter and electricity, known as resistance and defined by Ohm's law, is competent to produce contact electromotive forces, we may perceive how it comes to pass that in good conductors such forces are so weak, while in insulators they are so strong. Electricity slips through the fingers of a metal as it were, and the driving force it can exert is very feeble; while an insulator gets a good grip and thrusts it along with violence.

The metals differ in their gripping power, and, roughly speaking, the best conductor makes the worst thermo-electric substance. A bad conductor, like antimony, or, still better, galena, or selenium, or tellurium, makes a far more effective thermo-electric element than a well-conducting metal. Not that specific resistance is all that has to be considered in the matter; there is also a specific relation between each metal and the two kinds of electricity. Thus, iron is a metal whose atoms have a better grip of positive than of negative electricity, and so a positive current gets propelled in iron from hot to cold. Copper, on the other hand, acts similarly on negative electricity, and it is a negative current which is driven from hot to cold in copper. And all the metals can be classed with one or other of these two, except perhaps lead, which appears to grip both equally, and so to exert no differential effect upon either.

Passage of Electricity through a Gas.

There remains to be said something about the way in which electricity can be conveyed by *gases*.

The first thing to notice is that there is no true conduction through either gases or vapours; in other words, a substance in this condition seems to behave as a perfect insulator—perhaps the only perfect insulator there is. Not even mercury vapour is found to conduct in the least. This shows that mere bombardment of molecules, such as is known to go on in gases, is not sufficient either to remove or to impart any electric charge.

The commonest way in which electricity makes its way through a gas, setting aside the mere mechanical conveyance by solid carrier, is that of disruptive discharge. Let us try and look into the manner of this a little more closely, if possible.

First of all, since locomotion is possible to the molecules of a gas the same as of any other fluid, it is natural to ask why electrolysis does not go on as in a liquid. Now, for electrolysis in a liquid two conditions seemed necessary: first, that the atoms or radicles in a molecule should be oppositely charged with electricity; second, that they should be in such a condition (whether by dissociation or otherwise) that interchanges of atoms from molecule to molecule, or, in some other way, a procession of atoms, could be directed in a given direction by a very feeble or infinitesimal force.

Since a gas does *not* act as an electrolyte, one of these conditions, or perhaps both, must fail. Either the atoms of a gas-molecule are not charged, which is a plausible hypothesis for elementary gases, or else the atoms belonging to a gas-molecule remain individually belonging to it, and are not readily passed on from one to another.

When one says that a gas does not act as a common electrolyte, the experimental grounds of the statement are that a finite electrostatic stress certainly is possible in its interior—a stress of very considerable amount; and when this stress does overstep the mark and cause the electrode to yield, the yielding is evidently not a quiet and steady glide or procession, but a violent breaking down and collapse, due to insufficient tenacity of something. One may therefore picture the molecules of a gas, between two opposite electrodes or discharge terminals maintained at some great difference of potential, as arranged in a set of parallel chains from one to the other, and strained nearly up to the verge of being torn asunder. In making this picture one need not suppose any fixture of individual molecules: there may be a wind blowing between the plates; but all molecules as they come into the field must experience the stress, and be relieved as they pass out.

If the applied slope of potential overstep a certain limit, fixed by observation at something like 33,000 volts per linear centimetre for common air, the molecules give way, the atoms with their charges rush across to the plates, and discharge has occurred. The number of atoms thus torn free and made able to convey a charge by locomotion is so great that there has never been found any difficulty in conveying any amount of electricity by their means. In other words, *during* discharge the gas becomes a conductor, and, being a conductor by reason of locomotion of atoms, it may be called an electrolytic conductor.

But whether the charge then possessed by each carrier atom intrinsically belonged to it all the time, or whether it was conferred upon the components of the molecules during the strain and the disruption, is a point not yet decided.

What is called "the dielectric strength" of a gas—that is, the strain it can bear without suffering disruption and becoming for the instant a conductor—depends partly on the nature of the gas, and very largely on its pressure. Roughly, one may say that a gas at high pressure is very strong, a gas, at low pressure very weak. An ordinary electrolyte might be called a dielectric of zero strength.

One reason why pressure affects the dielectric tenacity of a gas readily occurs to one: it is certainly not the only one, but it can hardly help being at least partially a *vera causa;* and that is, the fact that in a rare gas there are fewer molecules between the plates to share the strain between them.

Thus if 40,000 volts per centimetre break down ordinary air, 40 volts per centimetre ought to be enough to effect discharge through air at a pressure of about $\frac{3}{4}$ millimetre of mercury; and at a pressure of 50 atmospheres 2,000,000 volts per centimetre should be needed.[1]

A Current regarded as a Moving Charge.

To review the ground we have covered so far. We first tried to get some conception of the nature of electrostatic charge, and the function of a dielectric medium in static electricity. We next proceeded to see how far the phenomena of current electricity could be explained by reference to electrostatics. For a current, being merely electricity in locomotion, need consist of nothing but a charged body borne rapidly along.

Charge a sphere with either positive or negative electricity, and throw it in some direction: this constitutes a positive or a negative current in that direction. There is nothing necessarily more occult than that. And a continuous current between two bodies may be kept up by having a lot of pith balls, or dust particles, oscillating from one to the other, and so carrying positive electricity one way, and negative the other way. But such carriers, as they pass each other with their opposite charges, would be very apt to cling together and combine. They might be torn asunder again electrically, or they might be knocked asunder by collision with others. Unless they were one or other, the current would shortly have to cease, and nothing but a polarized medium would result.

Instead of pith balls, picture charged atoms as so acting, and we have a rough image of what is going on in an electrolyte on the one hand, and a dielectric on the other. The behaviour of metals and solid conductors is more obscure. Locomotive carriage is not to be thought of in them; but, inasmuch as no new phenomenon appears in their case, it is natural to try and

[1] It is true that tension per unit area, or energy per unit volume, is proportional to the *square* of the potential-slope, and I attach no special importance to the simple proportion assumed in the text. There is a great deal more to be said on these subjects, but this is scarcely the proper place to say it.

picture the process as one not wholly dissimilar; and this is what in one place we tried to do; with, however, but poor success.

I have said that an electric current need be nothing more occult than is a charged sphere moving rapidly; and a good deal has been made out concerning currents by minutely discussing all that happens in such a case. But, even so, the problem is far from being a simple one. One has to consider not only the obviously moving charge, but also the opposite induced charge tied to it by lines of force (or tubes of induction, as they are sometimes called), and we have this whole complicated system in motion. And the effect of this motion is to set up a new phenomenon in the medium altogether—a spinning kind of motion that would not naturally have been expected; whereby two similarly charged spheres in motion repel one another less than when stationary, and may even begin to attract, if moving fast enough; whereby also a relation arises between electricity and magnetism, and the moving charged body deflects a compass needle. Of which more in the next Part. OLIVER J. LODGE.

(*To be continued.*)

THE TWEEDDALE COLLECTION.

THE great collection of birds formed by the late Marquess of Tweeddale has now safely arrived in London, and has been deposited in the Natural History Museum at South Kensington. It is sufficient to say that it equals in extent the valuable donation of American birds presented by Mr. Osbert Salvin and Mr. F. Du Cane Godman, numbering about 27,000 specimens; and though inferior in number of individual skins to the great Hume collection, which reached the phenomenal number of 63,000 specimens, it is not inferior in interest to either of these wonderful collections. Mr. Hume thoroughly worked the territory of the British Asian Empire from Scinde to Assam and Manipur, from Khatmandu to Ceylon, and from Tenasserim to Singapore; but to the eastward of these countries the work had been continued by other naturalists, and the results of their labours are largely represented in the Tweeddale collection, which now forms part of the British Museum.

On the death of the late Marquess, his entire collection and library were bequeathed by him to his nephew, Capt. R. G. Wardlaw Ramsay, of Whitehill, a naturalist of high promise and performance; and in the moment of satisfaction at receiving his magnificent donation one cannot help feeling great regret that the many cares and duties incident upon his succession to the family estates at Whitehill have temporarily deprived him of the leisure necessary for the working out of the great collection left to him by his uncle. The facilities for ornithological study, however, at the Natural History Museum, are now rapidly becoming so perfect that one may reasonably hope that he will, in common with all ornithologists, be able to work in that institution with the same comfort as in his own museum in Scotland. If in future years the student of birds finds that at South Kensington the work he loves can be done more expeditiously and with command of a larger series of specimens than in any other Museum in the world, his gratitude will be largely due to the four naturalists we have mentioned—Mr. Allan Hume, Messrs. O. Salvin and F. D. Godman, and Capt. Wardlaw Ramsay—for the unexampled generosity which has led them to present to the British nation the wonderful collections which will make our Ornithological Museum famous for all time.

Many naturalists who read this article will remember how, twelve years ago, the entire collection of bird-skins in the British Museum was contained in a few book-cases in a dingy cellar at Bloomsbury, where all the skins were kept in wooden boxes—a barbarous method, which was not only clumsy, but actually harmful to the specimens themselves. The development of the collection since that era is one which any English naturalist may consider with pride. Not only is the invaluable series of skins in the British Museum now well cared for and properly housed, but the *raison d'être* of the large collections in private hands has been removed. It is admitted on all sides that had the facilities of study in the old days been such as they now are in the Natural History Museum, there would have been no need for ornithologists to devote their private means to the formation of the collections which have, however, now become the foundation of the greatest Ornithological Museum in the whole world.

The three great collections which have enriched the British Museum during the last two years have each been, in their way, of supreme importance for zoological science. The Hume collection was a perfect marvel in the way of complete series of specimens. Not only are the various plumages of the Indian birds exemplified in a manner hitherto unheard of, but even the geographical ranges of most of the species are illustrated in a perfect way by the series of specimens contained in the collection. The Salvin-Godman donation consisted of American birds, and added hundreds of species to the British Museum which were desiderata to that collection. Though not so rich in series of various plumages as the Hume collection, the number of gaps in the quota of American birds which their donation filled was simply enormous, and from being one of the most backward in regard to its neotropical collection of birds, the British Museum is now one of the foremost as regards the value of its American series.

The Tweeddale collection "takes up the running," so to speak, where Mr. Hume left off, and it must not be supposed that the donation now made by Capt. Wardlaw Ramsay is merely the collection of skins left to him by his uncle. To imagine this would be but a poor appreciation of the energy which has led him during the last few years to develop and greatly increase the collection by the addition of a large number of birds obtained during his military career in the East, and by hundreds of other valuable specimens acquired since his uncle's death. Thus the skins from the Kurrum Valley in Afghanistan, and from the Karen Hills in Burmah, obtained by Capt. Ramsay himself, are supplementary additions of the highest value to the Hume collection, inasmuch as Mr. Hume never had correspondents in these parts, and the specimens from the Andamans and Nicobars are also of great importance; but of course the interest of the Tweeddale collection centres round the expedition to the Philippine Archipelago made by Mr. Alfred Everett for the late Marquess. Mr. Everett visited several islands on which no zoologist had previously trod, and as a natural result he discovered some beautiful new species of birds which are still unrepresented in any other collection but that of Capt. Ramsay. Altogether Mr. Everett furnished material for twelve important memoirs by the Marquess of Tweeddale, and the number of Philippine types now presented to the British Museum adds immensely to the wealth of the donation. Lord Tweeddale was also greatly interested in an obscure family of birds—the Drongos, or Crow-shrikes (*Dicruridæ*)—and possessed a wonderful collection of these birds, although it may be stated that there is scarcely a family of Oriental birds which is not strongly and completely represented in the collection.

Ornithologists will understand the nature of this noble gift of Capt. Ramsay when they learn that in addition to the collection of birds he has also presented the whole of the splendid Tweeddale library (nearly 3000 volumes) to the British Museum, to be placed in the Bird-Room, alongside of the collection of skins, for the benefit of students of ornithology. The Tweeddale library is one of the best in the world, containing many rare volumes which

we have not seen elsewhere, and this donation alone is worth several thousands of pounds. With a series of bird-skins now numbering nearly a quarter of a million, and with the best ornithological library in the world, it will be strange if the work done at the British Museum in future be not rendered an easy and an enjoyable task, though it must be remembered that the very magnitude of the collection contributes to the difficulty of its exact study. The writer may be excused an expression of deep gratitude to the ornithologists who have enriched the collection under his charge, so that from a series of (at the most) 40,000 skins, the number of bird-skins has been raised in fifteen years to more than 200,000, and he merely adds a hope that he may see the British Museum become the repository of all the work of English ornithologists, not only from this country, but from all parts of the Empire.

This article has dealt merely with the three great donations which have been received during the last two years, and has not recorded the many other collections, of almost equal importance, which have been acquired by the Trustees of the British Museum since 1872, the results of the life-work of such naturalists as Sclater, Wallace, Gould, and others of whom the country is proud, the acquisition of whose collections also is a source of the greatest encouragement to the writer.

R. BOWDLER SHARPE.

THE STORM OF OCTOBER 30.

THE gale which swept over the southern part of England on the morning of Sunday the 30th was both sudden and severe. On the previous day the weather was exceptionally fine over the country generally, and in many places it was a truly "pet" day. The Meteorological Office, in their morning report referring to the barometric rise which was going on in the south and west, remarked that "some improvement in the weather is therefore likely in the south." In the afternoon of Saturday, however, there were signs of approaching bad weather, and by six o'clock a disturbance was shown to be situated off Scilly, the barometer reading 29·4 inches. The Meteorological Office considered the situation sufficiently menacing for the issue of storm signals, and the south cone was hoisted in the south and south-west districts. During the night the storm passed in an east-north-east direction over the southern counties of England, travelling at the rate of about thirty miles an hour. The centre passed almost directly over London at about five o'clock in the morning, when the wind changed suddenly about 180°, the barometer at the time registering 28·86 inches, and in the next two hours the mercury rose 0·4 of an inch. At Greenwich Observatory the anemometer recorded 17·2 lbs on the square foot at 7·5 a.m., which is equivalent to an hourly velocity of about sixty miles. By 8 a.m. the centre of the disturbance had passed to the eastward of our islands and was situated a short distance off Yarmouth. The storm afterwards travelled in a north-easterly direction, maintaining somewhat its former rate of movement, and on Monday morning the central area was in the neighbourhood of Stockholm. The gale was rather severe on our southern coasts, but its principal violence was felt in the English Channel and on the French and Danish coasts. The Paris *Bulletin* shows that at many of the stations the wind reached the full force of a hurricane, and the sea was terrific. The amount of rain which fell during the storm was unusually heavy, 1·59 inches being registered at Scilly, and upwards of an inch at other stations in the south of England and also in the north of France. As is commonly the case with these quick-travelling and rapidly-developing storms, the disturbance was a "secondary" to a larger disturbance which was passing from off the Atlantic to the northward of our islands.

ROBERT HUNT, F.R.S.

MR. ROBERT HUNT, whose death we have already briefly announced, was born at Devonport, then called Plymouth Dock, on September 6, 1807. His father was a naval officer who perished, with all the crew, in H.M.S. *Moucheron*, in the Grecian Archipelago. Robert Hunt, left to his mother's care, was destined for the medical profession; and, having been placed with a surgeon in London, he attended the anatomical lectures of Joshua Brooks; but his studies were interrupted by failing health, and his medical training was never completed. In 1840, Mr. Hunt became secretary to the Royal Cornwall Polytechnic Society at Falmouth. His earliest contributions to science were in connection with photography—a subject to which he applied himself with assiduity immediately on the announcement of Daguerre's discovery in 1839. Mr. Hunt's investigations led to the discovery of several new processes, which were either described in the *Philosophical Magazine* or announced to the British Association. His experimental researches on the chemical activity of the highly refrangible rays of the solar spectrum, his work with the actinograph, and his study of the influence of light upon the germination of seeds and the growth of plants, formed the subject of numerous papers between 1840 and 1854. Mr. Hunt's "Researches on Light" appeared in 1844. His "Manual of Photography," which was the first general work on the subject published in this country, passed through six editions.

While Mr. Hunt was in Cornwall he undertook some interesting inquiries, conjointly with the late Mr. Robert Were Fox, into the electrical phenomena of mineral veins; and he also entered upon an examination of the air in some of the Cornish mines. In 1845 he came to London, at the invitation of Sir H. T. De la Beche, to succeed Mr. Thomas Jordan, as Keeper of Mining Records at the Museum of Economic Geology, then recently established in Craig's Court. On the establishment of the Government School of Mines in 1851, he was appointed Lecturer on Mechanical Science, and opened his course with an address on the importance of cultivating habits of observation. After holding this position for two sessions he resigned it to the late Prof. Willis, and undertook for a short time the duties of Lecturer on Physics. In 1854 Mr. Hunt was elected a Fellow of the Royal Society.

For the last thirty years Mr. Hunt's energies have been mainly directed to the collection and collation of statistical information relating to British mining and metallurgy. From 1853 until the abolition of the Keepership of Mining Records he published regularly the annual volumes of "Mineral Statistics," containing a vast mass of voluntary returns obtained by his personal influence. As a member of the Royal Coal Commission of 1866, he undertook the statistical part of the inquiry, and published detailed information on the coal resources of the country.

The technical education of the metal-mining population of the West of England was a subject that Mr. Hunt always had at heart. He was an earnest advocate for the establishment of local mining schools, and should be regarded practically as the founder of the Miners' Association of Cornwall and Devon—a body now amalgamated with the Mining Institute. In 1883, Mr. Hunt published a voluminous work on "British Mining." After the death of Dr. Ure he consented to edit the "Dictionary of Arts," and brought out successively the fifth (1860), sixth (1867), and seventh (1875) editions of this work. At the same time Mr. Hunt possessed great literary taste, which found scope in several lighter works, such as his "Poetry of Science," "Panthea, or the Spirit of Nature," and the "Romances of the West of England." Mr. Hunt's long, busy, and useful life was closed on the 17th ult. His remains were interred in Brompton Cemetery.

NOTES.

THE vacancy in the representation of Cambridge University, caused by the death of Mr. Beresford Hope, raises again the question of the desirability that the Universities should be represented in Parliament by men of distinguished culture, whether literary or scientific. Men of science will be glad to hear that a movement is on foot in Cambridge to induce the President of the Royal Society to allow himself to be nominated as a candidate for the membership of the University. A meeting will be held on Saturday for the purpose of considering the question of the representative. It is believed that Prof. Stokes, if he finds the feeling to be strong in favour of his acceptance, will regard it as his duty to place his services at the disposal of the electors. A more distinguished representative never offered himself for the suffrages of any University in this country. His presence in the House of Commons would be another pledge that questions involving the interests of science would be discussed with adequate knowledge in that assembly.

IN presenting the prizes to the successful students of the Bath Lane Science and Art School at Newcastle-on-Tyne, Lord Randolph Churchill expressed the opinion that when "the State has laid the foundation by freely contributing to elementary education, localities ought to come in and ought to build on that foundation whatever edifice may be necessary for the further and higher technical education of the artisan." "This school," he continued, "is essentially the result of pure local effort, pure local energy, and pure local pride. You have, I understand, carried on the whole work of this school without the smallest assistance from Government of any sort or kind. Now, I was saying that technical education is supposed to be a great requirement of the present day, and I was using the illustration of your school to show that localities can if they wish, if they have the energy and the determination, supply that technical education for themselves." In a letter to the *Times* a writer signing himself "Y." has exposed the ignorance displayed by Lord Randolph Churchill in this astonishing statement. The Newcastle school, instead of being carried on simply by means of "local effort," has been largely aided by the Government. As "Y." points out, the last Report of the Science and Art Department shows that in 1886 the payments on results made to the school were for science £1212 10*s*., and for art £238 10*s*., while the students' fees for instruction in science were £1228, and in art £150. It is discreditable that a man in Lord Randolph Churchill's position should be capable of making such a mistake s this. The incident is important, for it indicates the spirit in which too many of those who talk wildly about "economy" approach the consideration of the grave question as to the duty of the State with regard to education. Lord Randolph Churchill has been Chancellor of the Exchequer, and may some day be Chancellor of the Exchequer again. With his crude notions and inaccurate information, the injury he might do in this position to our educational system is simply incalculable.

THE College of State Medicine, lately incorporated, ought to be a remarkably successful institution, if we may judge from the names of its officers. The Chairman of Council is Sir Joseph Fayrer, K.C.S.I., F.R.S. The following are the members of the Council:—Sir John Watt Reid, K.C.B., Sir Thomas Crawford, K.C.B., Sir William Guyer Hunter, K.C.M.G., M.P., Sir Henry Roscoe, F.R.S., Sir Douglas Maclagan, Surgeon-General William Robert Cornish, C.I.E., Richard Quain, F.R.S., Edward Klein, F.R.S., Robert Brudenell Carter, and Arthur Trehern Norton. The following are *ex-officio* members:—The President of the Sanitary Institute, the President of the Society of Medical Officers of Health, the President of the Public Health Medical Society, the Professor of Public Health to the College. Mr. James Cantlie is the honorary secretary.

THE first Congress of the Dutch Society of Naturalists lately met at Amsterdam under the presidency of Dr. Stoknis, who delivered an address on nationality and natural science. Among the other addresses were the following: on Martinus of Marum, who made a large electric machine at the end of the last century, by Prof. Bosscha (Delft); and on the education of future naturalists, by Prof. Spruyt (Amsterdam).

AN Exhibition of Textile Goods and Machinery will be held at Warsaw about the middle of December next. It will be open to all countries.

THE expedition which the Finnish Archæological Society despatched to the Upper Yenisei last summer, to prosecute archæological researches in that locality, has just returned to Helsingfors. It has brought back drawings of about thirty stone figures, and copies of a large number of inscriptions, hitherto not deciphered, on a rock, on nine raised stone slabs, and on many stones along the upper course of the Yenisei. The expedition has also gathered a vast collection of objects belonging to the Siberian Bronze Age.

IN his latest Annual Report, Mr. Putnam, Curator of the Peabody Museum of American Archæology and Ethnology, says that during the past year several large collections of special interest have been added to the Museum. The most important is the Bucklin collection from ancient graves in Peru, principally at Ancon. This collection is particularly rich in textiles and in ornaments and implements made of silver and bronze; and among the objects in pottery there are many new forms and styles of ornamentation. Another collection of over 300 specimens of pottery obtained from the province of Piura, Peru, has also been purchased, and nearly every vessel adds some important feature to the already instructive Peruvian collection in the Museum. A third collection consists of 337 pottery vessels, a number of whistles and other objects made of pottery, 245 stone implements, and several large carved stones, some circular, and others resembling animals, supposed by some archæologists to be seats, and by others to be metates. This collection has been catalogued and placed in the exhibition cases with the other objects from the ancient graves in Chiriqui. It was obtained from the well-known collector of antiquities in Chiriqui, Mr. J. A. McNiel, who has resided in the State of Panama for many years. Mr. Putnam expresses much regret that Mr. McNiel has not been able to keep together the contents of each grave. He is dependent on the sale of the specimens for the means to carry on his work, so that many of the objects he obtained are now widely scattered, and archæologists have no means of tracing the development of the arts of the people, which could have been done had the collection been kept together and the associations of every object carefully noted.

WE have received the Proceedings of the U.S. National Museum during the year 1886. This is the ninth volume of the series. It contains many interesting and valuable papers, some by members of the scientific corps of the National Museum, others by writers who have made portions of the collections of the Museum subjects of special study. The volume opens with a list of fishes collected in Arkansas, the Indian Territory, and Texas, in September 1884, with notes and descriptions, by Mr. D. S. Jordan and Mr. C. H. Gilbert. Among the other papers are: notes on fulgurites, by Mr. G. P. Merrill; a review of Japanese birds, by Mr. L. Stejneger; a catalogue of animals collected by the Geographical and Exploring Commission of the Republic of Mexico, by Mr. F. Ferrari-Perez; a description of six new species of fishes from the Gulf of Mexico, with notes on other species, by Mr. D. S. Jordan and Mr. B. W. Evermann; and Norsk naval architecture, by Mr. G. H. Boehmer. At the end of the volume there are twenty-five plates, each accompanied by its explanation.

Some of the difficulties with which the curator of a museum in tropical climates has to contend are described in the last report on the Colombo Museum. Mr. Haly states that naphthaline is not so powerful a protection against the effects of climate as was anticipated. It seems to prevent the attacks of mites, but it is powerless against fungus. It is hoped that it will ward off the attacks of the fish insect on the labels. As an instance of the rapidity with which this pest works, it is mentioned that one case was re-painted, and the objects rearranged and labelled. No naphthaline was procurable at the time, and in a fortnight several labels had been defaced and several numbers lost. Carbolic acid and corrosive sublimate have both been mixed with the gum, but their use is objectionable, as they discolour the labels, and do not afford permanent protection. Every object in a tropical climate, Mr. Haly says, ought to be exhibited on its own stand, and that stand labelled by hand in black or white paint. The Museum has also been attacked lately by a fungus. Not only have the specimens themselves been attacked, but the wood of the teak cases, and even the glass, has been covered. In one case the insects were absolutely rolled round and connected together by its fine filaments—filaments so fine as to be invisible through the glass. Naphthaline, benzine, cyanide of potassium, carbolic acid, and other substances have all been tried in vain: the only check to its growth was citronella oil.

The "Educational List and Directory of the United Kingdom for 1887–88" (Sampson Low), edited by Mr. William Stephen, has just been published. This is the second issue of the work. The editor's aim is to concentrate within reasonable space the names of the chief educational institutions of the Kingdom. Besides being a guide for the use of parents and guardians, and a directory for all who give attention to educational matters, the volume is interesting, as Mr. Stephen claims, on account of the fact that it is the first methodical effort to unite for practical purposes the designations of our educational institutions, from the Universities downwards, in England, Wales, Scotland, and Ireland. No "descriptive matter" has been introduced.

The Cardiff Naturalists' Society have issued a valuable descriptive list of the indigenous plants found in the neighbourhood of Cardiff, with a list of the other British and exotic species found on Cardiff Ballast Hills. The compiler is Mr. John Storrie, Curator of the Cardiff Museum.

A stalactite cave has been discovered near Steinbach in the Upper Palatinate. It can only be approached by a shaft 1 square metre in diameter and 40 metres deep. The cave is divided into several compartments, through one of which a stream of water slowly flows. The numerous stalactites are of great beauty. Another stalactite cavern, equalling the celebrated Dechen cavern, both in extent and peculiarity of form, has been discovered in the so-called Billstein, between Hirschberg and Warstein (Westphalia). The interior consists of several chambers. Numerous animal remains (probably prehistoric) have been found in the cave.

The death is announced of Herr August Kappler, whose excellent book on Dutch Guiana is well known. He died at Stuttgart, aged seventy-one.

We regret to announce the death of Dr. E. Luther, Professor of Astronomy at the Königsberg University, also Director of the Observatory. He was born February 24, 1816.

The weather in Iceland during the summer has been very unusual. The ice did not leave the north and east coast till the middle of September, or quite a month later than usual. Storms and fogs have been very frequent. The last mail brings news that the weather was then (the middle of October) dry and fine. Frost had, however, set in in several parts. This is the last news we shall have from the island until next spring.

At a recent meeting of the Wellington (New Zealand) Philosophical Society, Sir James Hector exhibited samples of trachyte tuff and breccia, constituting the auriferous deposit lately found in the level ground west of Te Aroha. The material, which appeared to be somewhat of the nature of an infiltrated quartz reef which had been decomposed and then distributed as a surface deposit, was found to contain gold at a rate varying from two ounces to fourteen ounces to the ton. The gold occurs in twisted angular flakes and grains, and is associated in a light feldspar sand with heavier grains of quartz mica and titanic iron. Sir James Hector is of opinion that it will probably prove to be the outcrop of an important reef, from which the sulphides have been removed by decomposition, so that gold is left in its free state. The gold is the usual alloy of the district—consisting of gold 80·47 per cent., silver 16·91, loss 2·62, previous assays having varied from 77 to 84 per cent.

The last number of the *Excursions et Reconnaissances* of Saigon contains an account by M. Navelle of a journey which he made in Annam from the port of Thi-Nai, commonly called Quin-hon, to Bla. The route lay through the great town of Binh-Dinh, and by the ruins of Quin-hon, at one time the capital of the Chams or Ciampois. This leads the traveller to narrate the vicissitudes of the once powerful kingdom of Ciampa, which was overthrown in the fifteenth century, after seven centuries of contest with Annam. The narrative is mainly interesting from the circumstance that the traveller visited a number of important towns hitherto unseen by Europeans. At the town of Dong-pho, he met an official who at one time performed curious functions. The Kinh-li was an Annamite official appointed to reside beyond the frontiers to organize Annamites who fled from their native country, and to direct their raids against neighbouring States. These vagabonds, thus directed, acted as the van-guards of regular Annamite invasion. M. Landes, in the same number, continues his researches into the folk-lore of the races of French Indo-China. In the present instance he gives the tales and legends of the Tjames, Chams, or Ciampois, above-mentioned. They have long been subjugated, and are now divided into two groups, one inhabiting the Bin-thuan province, the other Cambodia. Until recently they were amongst the most unknown peoples of the peninsula, but M. Aymonier's accounts of his long exploration in Binh-thuan, which were published in recent numbers of *Excursions*, have thrown much light on the subject. The stories published by M. Landes were collected from the mouth of a Cham, and are mostly fairy tales.

Dr. Karl Pettersen, Director of the Tromsö Arctic Museum, has lately written a pamphlet on the state of the drift-ice in the Arctic seas during the last few years. In this pamphlet he offers some suggestions as to the way in which attempts to reach the North Pole should be made. "It seems to me," he says, "that every year shows more and more clearly that it is a sheer waste of life and money to despatch casual and erratic expeditions to the North Pole. In my opinion the result would be attained most easily, surely, and cheaply by despatching every year, for a period of ten or eleven years, a certain number of well-equipped steamers from certain suitable spots towards the Pole. As the ice-masses in the Polar Basin are, without doubt, in a constant but varying motion, this plan would enable one or another of the expeditions to seize the right moment for a dash northward. We could not, of course, be absolutely certain of success, for experience has proved that the state of the ice in a particular locality at a particular time does not enable us to predict what it will be in the same locality in the following year. Still, the opportunity to reach a high latitude would present itself sooner or later. The expeditions of past years having almost conclusively demonstrated that it will be

impossible to reach the North Pole along the west coast of Greenland, the *point d'appui* for the journeys on the plan advocated would be confined to the European and Asiatic Polar seas. The routes I should recommend are four: viz. one along East Spitzbergen to Franz Josef Land, and northwards, starting from the north of Norway; one east of Franz Josef Land, starting from the Yenisei or Obi; one *viâ* Franz Josef Land, starting from the New Siberian Islands or the Lena; and one from a suitable spot in Behring Strait. I have every reason to believe that if four such expeditions were in readiness in these localities every year during a period of eleven years, we should by the end of that time, by one or another of the routes, have solved the problems which still face us around the Pole. Probably the scheme might be carried out most advantageously by international co-operation, as in the case of the Polar Research Expeditions of 1882–83. In any case, I venture to think that the plan of any expeditions should not be finally formed before July, or, if possible, August. By that time many of the huntsmen have returned from their first voyage to several parts of the Arctic Sea, and the expeditions would be in possession of a fair knowledge of the state of the ice in each. I believe that, should the route chosen be *viâ* Spitzbergen or Novaya Zemlya, a careful study of the weather and wind in North Norway during the spring and early summer would benefit Polar expeditions immensely, showing whether the route to the north or east of Spitzbergen should be followed, or the more eastern one by Novaya Zemlya."

THE additions to the Zoological Society's Gardens during the past week include a Grand Eclectus (*Eclectus roratus*) from Moluccas, presented by Miss P. Lockwood; a Goffin's Cockatoo (*Cacatua goffini*), habitat uncertain, presented by Miss Barton; a Water Rattlesnake (*Crotalus adamanteus*), a Water Viper (*Cenchris piscivorus*), two American Black Snakes (*Coluber constrictor*), a Chicken Snake (*Coluber quadrivittatus*), two Mocassin Snakes (*Coluber fasciatus*) from Florida, presented by the Natural History Society of Toronto; two Green Lizards (*Lacerta viridis*), twelve Spotted Salamanders (*Salamandra maculosa*), two Common Snakes (*Tropidonotus natrix*) from Italy, presented by Messrs. Paul and Sons; an Algerian Tortoise (*Testudo mauritanica*) from Algeria, deposited; an Aye-Aye (*Chiromys madagascariensis*) from Madagascar, purchased; six Painted Terrapins (*Clemmys picta*), two Corn Snakes (*Coluber guttatus*), two Milk Snakes (*Coluber eximius*), two Mocassin Snakes (*Tropidonotus fasciatus*), two Ribbon Snakes (*Tropidonotus saurita*), two Hog-nosed Snakes (*Heterodon platyrhinos*), two Grass Snakes (*Cyclophis vernalis*), six Dekay's Snakes (*Ischnognathus dekayi*), nine American Green Frogs (*Rana halecina*), ten Noisy Frogs (*Rana clamata*), a Wood Frog (*Rana sylvatica*), a Changeable Tree Frog (*Hyla versicolor*), nine Red-backed Salamanders (*Plethodon erythronotus*) from Canada, received in exchange; a Blood-breasted Pigeon (*Phlogœnas cruentata*), bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *NOVEMBER* 6–12.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 6

Sun rises, 7h. 4m.; souths, 11h. 43m. 44·2s.; sets, 16h. 24m.: right asc. on meridian, 14h. 45·2m.; decl. 15° 59′ S. Sidereal Time at Sunset, 19h. 26m.

Moon (at Last Quarter November 8, 17h.) rises, 19h. 50m.*; souths, 3h. 54m.; sets, 11h. 58m.: right asc. on meridian, 6h. 54·4m.; decl. 20° 14′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	9 9	13 3	16 57	16 4·7	23 21 S.
Venus ...	2 57	8 59	15 1	11 59·6	0 22 S.
Mars ...	1 16	8 0	14 44	11 1·4	7 59 N.
Jupiter ...	7 12	11 53	16 34	14 54·4	15 44 S.
Saturn ...	21 48*	5 35	13 22	8 35·5	19 0 N.
Uranus ...	4 17	9 54	15 31	12 55·4	5 14 S.
Neptune..	17 7*	0 49	8 31	3 49·1	18 16 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich).

Nov.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.	
			h. m.	h. m.	°	°
6	*g* Geminorum	$5\frac{1}{2}$	22 34	22 51	338	302
8	7 Leonis	$6\frac{1}{2}$	22 30	23 8	90	182
9	ψ Leonis	6	1 52	2 42	78	181

Nov.	h.	
7	8	Mercury stationary.
8	0	Saturn in conjunction with and 1° 1′ north of the Moon.
9	2	Jupiter in conjunction with the Sun.
12	1	Venus in conjunction with and 3° 42′ south of the Moon.

Saturn, November 6.—Outer major axis of outer ring = 42″·2; outer minor axis of outer ring = 13″·5; southern surface visible.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Nov. 7,	2 49	*m*
			,, 12,	2 29	*m*
U Monocerotis	7 25·4	9 33 S.	,, 9,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 7,	3 9	*m*
		and at intervals of		20 8	
U Sagittarii	18 25·2	19 12 S.	,, 7,	0 0	*m*
R Scuti	8 41·5	5 50 S.	,, 7,		*m*
β Lyræ	18 45·9	33 14 N.	,, 6,	3 0	*M*
S Vulpeculæ	19 43·8	27 0 N.	,, 8,		*m*
η Aquilæ	19 46·7	0 43 N.	,, 8,	5 0	*m*
S Sagittæ	19 50·9	16 20 N.	,, 6,	1 0	*m*
			,, 9,	1 0	*M*
δ Cephei	22 25·0	57 50 N.	,, 10,	3 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near the Pleiades	60°	20° N.	Bright; rather slow.
From Camelopardalis	102	73 N.	Very swift.
Near η Leonis	149	22 N.	Swift; streaks.

GEOGRAPHICAL NOTES.

THE Danish Government has decided upon hydrographically measuring and charting the Guldborg Sound, the new harbour at Odense, in the Island of Funen, and the Randers and Mariager fjords in Jutland. Two vessels will also be engaged in preparing a naval chart of the coast around Denmark. It has also been decided to despatch an Expedition—the cost being estimated at £4000—to Iceland next summer, for the purpose of effecting hydrographical measurements around that island. Great fjords and waterways are here still unmeasured—a danger to navigation and a loss to science. It is believed that these researches may lead to important scientific discoveries, principally as regards zoology, meteorology, and geography. Moreover, they would probably be of great benefit to the Iceland fisheries, which are far from being thoroughly developed on account of the ignorance of existing fishing-banks, the temperature of the sea, &c. The researches, which will be similar to those carried out around Norway for some years past, will be effected with the Government schooner *Ingolf*, a vessel particularly adapted for the purpose. It is intended to employ the months of May, June, July, and August in this work, which, it is estimated, will be fully accomplished in five years.

IN the *Bollettino* of the Italian Geographical Society for September Signor E. Modigliani concludes his series of papers on Nias, with a detailed account of the physical features, natural history, and social condition of that island. The hilly surface is

relieved in some places by extensive open plains covered with tall grasses, and the forest vegetation is rapidly disappearing, owing largely to the wasteful habits of the natives. The accompanying seismic tables contain records of the earthquakes that occurred between the years 1843–86, some of which were very violent and attended by marine disturbances destructive to shipping, and driving boats and barges hundreds of yards inland. But there are no active volcanoes, and the prevailing formation appears to be a much-weathered compact limestone resting on gray or bluish Miocene marls and other argillaceous clays. The geological as well as the zoological conditions show that Nias, like the other islands running parallel to the west coast of Sumatra, must have formerly been connected with the mainland; and as Sumatra itself at one time certainly formed part of the Malay Peninsula, this chain of insular groups would appear to mark the original line of the Asiatic seaboard in this direction. Signor Modigliani's collections include 178 birds, representing 62 species, of which 8 are described as new, but allied to corresponding species in Sumatra; consequently the separation must have taken place at a very remote period—a conclusion also confirmed by other considerations. His rich zoological collection, comprising over 7000 specimens, has been presented to the Museum of Genoa.

METEOROLOGICAL NOTES.

THE Report on the Meteorology of India in 1885, being the eleventh year of the series, has just been published, and contains an immense mass of valuable information. The accumulation of Indian statistics during the last ten years may be best shown by a comparison of the following figures:—The number of stations at which the mean temperature is recorded has increased from 51 to 127, and the rainfall stations from 134 to 471. From this huge volume of 516 large quarto pages we can only note here a very few general remarks. Considerable attention is paid to solar radiation, the chief feature of which is found to be that the maximum intensity generally occurs during the winter half of the year (October to March), when the sun is in southern declination, and the thickness of the absorbing atmosphere traversed by the sun's rays is at a maximum. This is said to be apparently due to the fact that over a large part of India the atmosphere is most cloudy in the summer and autumn months. It would appear from the mean result of the sun-thermometer readings in all parts of India that the average intensity of solar heat had reached a minimum in 1884, and in 1885 underwent an appreciable increase. The duration of bright sunshine is now regularly recorded at four stations, and Mr. Blanford considers that the sunshine-recorder promises to be even more important than the sun-thermometer, since the duration of sunshine is a more direct measure of the amount of solar heat reaching the earth's surface than the registry of its mean maximum intensity. Anemometers are in use at nearly all the stations, and fourteen of them are large anemographs of the Kew pattern. The resultant direction of the winds is, however, computed by Lambert's old formula, which is based on the assumption that the force of all winds is equal, an assumption which is obviously often very misleading. The work is accompanied by maps showing the positions of the meteorological observatories and the mean distribution of temperature, pressure, and wind.

M. GARRIGOU-LAGRANGE describes, in the *Annuaire de la Société Météorologique de France*, his apparatus for registering the ascending and descending air-currents. The experimental anemometer is fixed on a mast at Limoges, and consists of four small fans moving round a vertical axis, and recording, by electrical arrangements, on a drum covered with ruled forms. The curves obtained by this method show plainly the upward or downward movement of the air, and the velocity is easily read off. The experiments show that ascending winds are generally stronger and more frequent than the descending, owing no doubt to the eddies caused by obstacles met with by the currents.

IN *Das Wetter* M. Seemann discusses the results of the storm-warnings issued by the *New York Herald* between September 1886 and January 1887, and finds that out of twelve warnings only three were quite successful, and three partially so. The wording of the telegrams is not so absolute as formerly; many of the depressions pass to the northward of our islands, and our weather is *disturbed*, although the gales do not always strike our coasts; judged in this light, the warnings may be more successful than when actual storms are predicted. On pp. 83–87 of the same journal Dr. C. Lang describes his method of predicting night-frost, from the position of the dew-point. From the Munich observations for 1879–86 he finds that 441 predictions of night-frost could be given, of which 89 per cent. were successful. In 8 per cent. of the cases, night-frost did not follow, and in 3 per cent. frost occurred unexpectedly. This method may be of considerable benefit to agriculturists; reference to this subject has been made by Mohn, Buchan, and others.

DR. GROSSMANN has published "Meteorologische Divisionstafeln" (Hammerich and Lesser, Altona), which will be very useful to meteorological observers in the calculation of monthly means, and daily means from hourly observations. For a dividend of four figures the quotient is given by simple inspections, and for more figures by interpolation by means of a table of proportional parts. The principle of construction is due to Dr. Köppen, and some of the tables were printed in *Aus dem Archiv der Deutschen Seewarte*, vol. i. 1878. Dr. Crelle's well known "Tables de Calcul" (Reimer, Berlin, 1869) are equally useful, but being in a large volume are not easily accessible to observers generally. The chief merit of Dr. Grossmann's tables lies in their publication in a form specially suitable to the wants of meteorological observers, and at the low price of about sixpence for single copies. They extend to four pages only, and are intended to be pasted on cardboard.

THE Italian Central Meteorological Office has published its *Annali* for the year 1884, in three thick folio volumes, containing a mass of meteorological, magnetical, and astronomical data. The meteorological services of Italy are very complicated, as in addition to the Central Office there are many large and independent establishments from which we possess long series of observations, some of them dating from the middle of last century. The Government established a Committee for Weather Telegraphy in 1863, under M. Matteuci, and in 1865 the Meteorological Section of the Ministry of Agriculture commenced the issue of the *Meteorologia Italiana*; from this originated the present office in 1877, located at the Collegio Romano. The service is now under the able superintendence of M. Tacchini, and includes 135 stations of the second order, some of which, *e.g.* those of Stelvio, Valdobbia, Cimone, and Etna are important mountain observatories; in addition to which there are 515 rainfall stations, and stations at the military settlements of Assab and Massowah. The work contains valuable discussions on thunderstorms, evaporation, and on the microscopical examination of atmospheric dust.

A SPECIAL meeting of the Meteorological Society of Mauritius, in honour of the Queen's Jubilee, was held on August 6, on which occasion Dr. Meldrum gave an interesting sketch of the origin and labours of the Society, and of the Royal Alfred Observatory. Charts were exhibited showing the tracks of the cyclones in the Southern Indian Ocean in each year from 1856 to 1886, also magnetograms showing simultaneous disturbances at Zi-ka-wei and Mauritius on January 9, 1886; and curves showing the apparent connection between solar, magnetical, and auroral phenomena, and also between solar phenomena and the frequency of cyclones, rainfall, and the depth of water in rivers, together with other articles of interest. Dr. Meldrum stated, with reference to the history of meteorology in the island, that a memorial was presented to the Governor in April 1851, suggesting the desirability of systematic observations, and the Meteorological Society was consequently founded on August 1 of that year. Shortly afterwards the office of Government Meteorological Observer was created, to which office Mr. Meldrum succeeded in 1862, and the two institutions, although distinct, have co-operated with each other. Regular observations were commenced at the new Observatory (see NATURE, vol. xxxvi. p. 546) in November 1874. We cannot enumerate here the many useful works which have been carried on, but the Hydrographer to the Admiralty has expressed the wish that the track charts should be published, and Mr. Meldrum has been informed that the Meteorological Council would probably publish them. He also states that tide-gauges will soon be erected at two points on the coast, and expresses a desire for the establishment of a high mountain station.

IN the *American Meteorological Journal* for September, Mr. H. Allen discusses the theory of the outflow of air under falling rain. It has been assumed by some meteorologists that the rain, drops carry with them and compress the air, which, flowing out-produces at times a considerable wind velocity (see article on

"Lightning" in Johnson's Encyclopædia). Several hypothetical cases are considered, and the author concludes that, although his computations may need some modification after further study, we can safely say that no appreciable velocity of air is produced by compression from falling rain. The same journal contains a long paper on the theory of the wind-vane by Prof. G. E. Curtis, reprinted from the *American Journal of Science* for July last. He discusses the relative stability of a straight vane, and that with a double or spread tail. The first reference to the latter is apparently in *Voigt's Magazin*, 1797, and this form has been in common use in England since about 1840. The formulæ show that for a frictionless bearing (1) that the oscillations of both vanes are smaller as the vanes are longer and larger; (2) that the spread vane is always more stable than the straight vane; and (3) that this advantage in stability is greater for long vanes than for short vanes, and is independent of the wind velocity. The author finds that, with equal friction, a spread vane is the more sensitive, and that consequently for two vanes of equal sensitiveness the spread vane will have the greater friction and will come to rest more quickly.

THE *Jahresbericht* of the Central Physical Observatory of St. Petersburg for 1885 and 1886, and the *Annalen* for the year 1885, have recently been published. The Russian system is very important, not only as being the most extensive on the globe, but on account of the great climatic contrasts and the completeness of the observations. The present Director, Dr. H. Wild, of Berne, was appointed in 1867, and under his able superintendence the number of stations has greatly increased and the quality of the observations has much improved. The central Observatory is situated about a mile from the sea, on the island of Wassili-Ostrow; the principal observing department has been transferred since 1878 to Pawlowsk, about four miles distant, and is placed under the superintendence of Dr. E. Leyst. The observations for 1885 are contained in two quarto volumes of about 700 pages altogether. In addition to the first class observatories, the number of stations of the second order amounts to 255, and of these the monthly and yearly results of 208 are published on the international scheme; from 38 of the stations the observations are published *in extenso*. Many new stations have been added recently, especially in Siberia, and in newly-acquired territories, *e.g.* Merv, Batoum, &c. One of the Siberian stations, viz. Werchojansk (lat. 67° 34′, long. 133° 51′) is stated by Dr. Köppen to be the coldest known point of the earth. The mean temperature there for the year was − 2°·9 F. The mean for January and December was − 62°·9, and the minimum in January − 88°·6 (far beyond the range of the usual tables). The mean temperature of July rose to 60°·6, and the minimum for that month was 39°·2. The number of rainfall stations for which the observations are given is 650, against 252 in the previous year; the data published are the monthly values, the maximum fall in 24 hours, and the number of days of rain and snow. A complete catalogue of the meteorological observations in Russia and Finland, by Dr. E. Leyst, giving the life-history of each station, has been published in the *Repertorium für Meteorologie* this year. This work also contains many valuable discussions of the vast amount of materials available for the purpose. The index of the *Annalen*, being mostly in Russian, is difficult to refer to.

THE WORK OF THE INTERNATIONAL CONGRESS OF GEOLOGISTS.[1]

I.

ELEVEN years ago the Association met at Buffalo. It was the year of the Centennial Exhibition, and we were honoured by the presence of a number of European geologists. This naturally opened the subject of the international relations of geology, and the proposition to institute a Congress of Geologists of the world took form in the appointment by the Association of an International Committee. The project thus initiated found favour elsewhere, and there resulted an international organization, which up to the present time has held three meetings. It was convened first at Paris in 1878, then at Bologna in 1881, and at Berlin in 1885. Its next meeting will be held in London next year, and an endeavour will be made to secure for the United States the honour of the fifth meeting. The original Committee of the Association has been continued, with some change of membership, and has sent representatives to each session of the Congress.

The work of the Congress, as originally conceived and as subsequently undertaken, has for its scope geologic nomenclature and classification, and the conventions of geologic maps. The particular classifications attempted are the establishment of the major divisions used in historic and stratigraphic geology and the subdivision of volcanic rocks. In nomenclature three things are undertaken: (1) the determination of the names of historic and stratigraphic divisions, (2) the formulation of rules for nomenclature in palæontology and mineralogy, and (3) the establishment and definition of the taxonomic terms of chronology (period, epoch, &c.) and of stratigraphy (system, series, &c.). The map conventions most discussed are colours, but all signs for the graphic indication of geologic data are considered. The Congress has also undertaken the preparation of a large map of Europe, to be printed in forty-nine sheets.

The work was for the most part planned at the Paris meeting, and Committees were appointed to formulate subjects for action by the Congress at subsequent sessions. Briefly stated, the work accomplished to the present time is as follows. Agreement has been reached as to the rank and equivalence of the taxonomic terms employed in chronology and stratigraphy, a set of rules for palæontologic nomenclature has been adopted, and many sheets of the map of Europe have been prepared for the engraver. A partial classification of stratified rocks has been agreed to, and also a partial scheme of map colours, but the reports of proceedings indicate that action in these matters is tentative rather than final.

It is understood that both of these subjects will have prominent place in the proceedings at the London meeting, and the American Committee is endeavouring to prepare itself for representative action at that meeting by ascertaining the opinions of all American geologists on the various subjects. It has asked this Section to set apart a day for the discussion of some of the more important questions, and it can hardly be doubted that the Section will realize the mutual advantage of thus assigning the time requested. I am personally so impressed with the importance of the possible work of the Congress that I shall devote the present hour also to its consideration.

The first thing the Congress did was to select names for a set of categories to express the taxonomic rank of stratigraphic divisions on the one hand, and of chronologic divisions on the other. In the terminology of zoology and botany the words kingdom, class, order, family, genus, species, and so forth, however difficult of definition they may severally be, nevertheless are used always in the same order of inclusion. No systematist in those sciences would think of grouping orders together and calling them a family, or of styling a group of families a genus. But in geology there is no such uniformity of usage. With some writers a group is larger than a series, with others it is smaller. With some an age includes several periods, with others a period includes several ages. There are even writers who ignore the distinction between stratigraphy and chronology; and among the classifications submitted to the Congress is one in which an age is subdivided into systems. There is a manifest advantage in bringing order out of this chaos, and so great is the utility of uniformity and perspicuity that the decisions of the Congress in this regard will unquestionably be followed by future authors. The terms and the order adopted by the Congress are as follows. Of stratigraphic divisions, that with the highest rank is *group*, then *system*, *series*, and *stage*. The corresponding chronologic divisions are *era*, *period*, *epoch*, and *age*. This order of rank is strange to most English readers and writers, and so is one of the terms—*stage*; but the strangeness is only a temporary disadvantage, and will not seriously retard the adoption of the convention. The fact that we have previously used the words in a different sense, or that their etymology might warrant a different meaning, need not deter us, for we know from frequent experience that the connotations of a word transferred from one use to another quickly disappear from consciousness, leaving it purely denotative. The introduction of the word *stage*, which can hardly be said to have had an English status heretofore, or at least the introduction of some new word for that part of the column, was necessitated by the restriction of the word *formation* to a special meaning—the designation of mineral masses with reference to their origin.

[1] Vice-Presidential Address read to Section E of the American Association for the Advancement of Science, August 10, 1887, by Mr. G. K. Gilbert.

The same restriction vacated another office that had been filled by formation, and to this office no appointment was made. I refer to the use of the word to denote indefinitely an aggregate of strata—as in saying, this formation should be called a series rather than a system. This is an important function, for which some provision must be made. I suggest that we may advantageously enrich our language by the permanent adoption of *terrane*, a word whose English meaning has not been well established.

The fixation of the chronologic terms creates a similar difficulty. We have crystallized out of our magma the terms *era*, *period*, *epoch*, and *age*, and there remain in the ground-mass only *eon*, *cycle*, and *time*. Of these, *eon* has a poetic connotation which seems to unfit it for this particular use; *cycle* implies repetition or recurrence; and *time* has been so generally applied to unlimited duration that it is difficult to apply it also to limited duration, even though the nature of the limitation be indefinite. On the whole, *time* seems open to the least objection, but I cannot help regretting that either *period* or *age*, both of which have heretofore passed current in the indefinite sense, was not reserved by the Congress for that function. With English-speaking peoples the word *eon* could have been better spared for the definite series.

But while the terms selected by the Congress are not beyond criticism, the benefits to be derived from an agreement in an orderly system are so great that I for one shall unhesitatingly adopt them as they stand—provided, of course, that the Congress makes no effort to improve its selection. A small reform of this nature yields its profit to this as well as future generations, and I hold it a duty to favour even those reforms which involve so much effort and pains that their blessings cannot be realized by those who initiate them. Such are the exchange of our English spelling for a rational system, and the exchange of decimal notation in arithmetic for a binary notation. My application of the new nomenclature begins with this address, in the preparation of which I have experienced its utility. That you may have no difficulty in interpreting my reformed language, I have placed the taxonomic legend on the wall, with the addition of the complementary indefinite terms—terrane and time.

Terranes.		Times.
Group.	...	Era.
System.	...	Period.
Series.	...	Epoch.
Stage.	...	Age.

There are propositions before the Congress to distinguish the names of individual groups, systems, series, and stages by means of terminations, those of the same rank having the same termination. Thus it is proposed by a Committee that every name of a group shall end in *ary*—Tertiary, Primary, Archeary; it is proposed that names of systems end in *ic*—Cretacic, Carbonic, Siluric; it is proposed that names of series end in *ian*—Eifelian, Laramian, Trentonian; and it is proposed that stage names terminate with *in*. Another Committee suggests that *ic* be used for stages instead of systems. The adoption of such a plan would enable a writer or speaker to indicate the taxonomic rank of a terrane without adding a word for that purpose. If he regarded a certain terrane taking its name from Cambria as a system, he would call it the Cambric; if he esteemed it only a series, he would say Cambrian; and there would be no need of adding the word system or series in order to express his full meaning. Conversely, the reader or hearer would always learn its taxonomic rank, or supposed rank, whenever a terrane was mentioned. These I conceive to be the advantages derivable from the change, but they would not be the only effects. It would become impossible for a geologist to name or allude to a terrane without declaring its rank, and the consequences of this would be evil in many ways. In the first place, one could not discuss terranes from any point of view without expressing an opinion as to their taxonomy, and the change would thus contravene one of the most important rights of opinion—namely, the right to reserve opinion. Again, geologists who differed as to the rank of a terrane would necessarily terminate its title differently, and a needless synonymy would thus be introduced. In the third place, the created necessity for taxonomic discrimination on all occasions would tend to direct undue attention to taxonomic problems. Taxonomy would be conceived by many geologists as an end instead of a means, just as correlation has been conceived, and energy would be wasted in taxonomic refinement and taxononomic controversy. It is convenient for purposes of description and comparison to classify the strata that constitute a local columnar section in phalanges of various magnitude or rank, but the criteria on which we depend for discrimination are in the nature of things variable, and offer ground for endless difference of opinion; and it would be extremely nnfortunate to have such differences perpetually brought to the foreground.

Another subject considered by the Congress is the nomenclature of palæontology. A Committee appointed for the purpose formulated rules for the establishment of the names of genera and species, and their report was adopted by the Congress. I have no opinion to express as to the wisdom of the rules, but it is a matter of surprise that a body of geologists assumed to speak with authority on the subject. From one point of view palæontology is a part of geology; from another point of view it is a part of biology. In so far as it names genera and species it is purely biologic, and it would seem proper that the students of fossils unite with the students of living animals and living plants in the adoption of rules of nomenclature.

A similar remark applies to the nomenclature of mineralogy, in regard to which no action has yet been taken. The most intimate relations of systematic mineralogy are with chemistry.

Yet another projected work of the Congress is the classification of eruptive rocks. Up to the present time action has been deferred, and it may reasonably be hoped that no scheme of classification will be adopted. If there existed a system of classification which gave general satisfaction and had stood the test of time, there would be little harm—and little or no advantage—in giving it the official stamp of approval. If the main features of a classification were well established and the residuary discrepancies were recognized as unessential, it is conceivable that some benefit might be derived from the submission of the matter to an assembly of specialists. But the actual case is far different. Not only is there wide difference as to the classification of volcanic rocks, but there is no agreement as to the fundamental principles on which their classification should be based, for we still lack an accepted theory of volcanism. At the same time observation is being pushed with great vigour, and with the aid of new and important methods. With the rapid growth of knowledge and ideas, classifications are continually remodelled, and the best is in danger of becoming obsolete before it has been printed and circulated. Should the Congress enter the lists, one of two things would occur. Either its classification would be treated like that of an individual, and ignored as soon as a better one was proposed; or it would be regarded as more authoritative, and new facts would for a time be warped into adjustment with it. In either case the reputation of the Congress would eventually suffer, and in one case science would suffer also.

There remain to consider the two most important undertakings of the Congress, the classification of terranes and the unification of map colours. The Congress is attacking these subjects indirectly by means of a third undertaking, the preparation of a geologic map of Europe, and this method of approach has had the effect of making it difficult properly to interpret its action. There can be no doubt that those who originally organized the work contemplated the enactment of a stratigraphic classification to be applied to the entire earth, and the selection of a colour scheme for use either in all geologic maps or in all general geologic maps. But at the Berlin session the Committee in charge of work on the map of Europe pressed the Congress for the determination of questions on which hung the completion of the map, and many hasty decisions were reached, while not a few disputed points were referred to the Map Committee. The debates indicate that much or all of this work was provisional or of merely local application, but the resolutions adopted show little qualification. It should be added that the official minutes of the meeting are still unpublished. In view of the uncertainty thus occasioned I shall not attempt to characterize the attitude of the Congress on the subject of classification, but shall merely develop my individual view.

It is the opinion of many who have discussed the general classification of terranes by convention of geologists that the smallest unit of such classification should be the stratigraphic system. What is a stratigraphic system? The Congress implies a definition in saying that a system includes more than a series and less than a group, and that the Jurassic is a system; but this gives only a meagre conception, and we need a full one. As the problem of classification demands a true conception of a system, and as there is reason to believe that a false conception is

abroad, it is proper that in seeking the true one we begin with the elements.

The surface of the land is constantly degraded by erosion, and the material removed is spread on the floor of the ocean, forming a deposit. This process has gone on from the dawn of geologic history, but the positions and boundaries of land and ocean have not remained the same. Crust movements have caused the submergence of land, and the emergence of ocean bottom, and these movements have been local and irregular, districts here and there going up while other districts went down. The emergence of ocean bottom exposes the deposit previously made on it, and subjects it to erosion. In this way every part of the known surface of the globe has been the scene of successive deposition and erosion, and in many districts the alternations of process have been numerous. It is manifestly impossible that either erosion or deposition should ever have prevailed universally, and it has been established by the study of stratigraphic breaks that a time of erosion has often interrupted deposition in one region while deposition was uninterrupted in another.

In transportation from its region of erosion to its place of deposition detritus is assorted, and it results that the simultaneous deposits on the bottom of an ocean are not everywhere the same. Equal diversity is shown in the ancient deposits constituting geologic formations. It is a general fact that synchronous formations have not everywhere the same constitution.

Many of the variations in deposits are correlated with depth of water and distance from shore, and it results that elevation and subsidence in regions of continuous deposition produce changes in the nature of the local deposit.

The animals and plants of the earth are not universally distributed, but are grouped in provinces. In the geologic past similar provinces existed, but their boundaries were different, shifting in harmony with the varying geography of the surface. From time to time the barriers separating contiguous provinces have been abolished, suffering them to coalesce; and conversely new barriers have arisen, creating new provinces. From the earliest Palæozoic to the present time the species of animals and plants have been progressively modified, the nature of the modification depending on local conditions. The faunas and floras of different provinces thus become different, and the longer the provinces remain distinct the greater is the divergence of life. The removal of a barrier either produces a new fauna by the fusion of the two previously separated, or else obliterates one and extends the area of the other. In either case there is a change toward the unification of life, and in either case there is an abrupt change in a local fauna. Thus the secular evolution of species, combined with the secular and kaleidoscopic revolution of land areas, leads to two antagonistic tendencies, one toward diversity of life on different parts of the globe, the other toward its uniformity. The tendency toward uniformity affords the basis for the correlation of terranes by comparison of fossils; the tendency toward diversity limits the possibilities of correlation.

If now we direct attention to some limited area and study its geology, we find that under the operation of these general processes it has acquired a stratigraphic constitution of a complex nature. Its successive terranes are varied in texture. Breaks in the continuity of deposition are marked by unconformities. The fossils at different horizons are different, and when they are examined in order from the lowest to the highest, the rate of change is found to vary, being in places nearly imperceptible and elsewhere abrupt. It is by means of such features as these —that is, by lithologic changes, by unconformities, and by life changes—that the stratigraphic column is classified into groups, systems, series, and stages. A system is a great terrane separated from terranes above and below by great unconformities or great life breaks, or both. Smaller unconformities, smaller life changes, and lithologic changes are used for the demarcation of series and stages; and, on the other hand, exceptionally great unconformities and life breaks are used to delimit groups. As the same criteria determine groups, systems, and series, differing only in degree, the precise definition of the term system is impossible, and in many cases the gradation of a terrane as a group, a system, or a series is largely a matter of convenience. From this point of view a system is somewhat artificial, but there is a more important sense in which it is natural. It is limited by stratigraphic or palæontologic breaks above and below, and these breaks are natural. The taxonomist is not warranted in dividing systems where no such break exists.

Transferring now our attention to some other area, distant from the first, and studying its stratigraphy, we find that the same principles enable us to divide it independently into stages, series, systems, and groups. Its fossils are not the same, but they are to a certain extent similar, and the sequence of life is approximately parallel. We cannot compare stage with stage, nor series with series perhaps, but we can compare system with system, and making the comparison we discover that the breaks are at different places. While one area was upraised and subjected for a time to erosion, the other received continuous deposition. While life in one area, enjoying constant conditions, was almost unchanged for long ages and even epochs, it was revolutionized in the other by the irruption across some obsolescent barrier of strong and aggressive faunas and floras. The systems of one area, therefore, do not coincide with the systems of the other in their beginning and ending. They may differ in number, and they may differ greatly in magnitude, and in the duration they represent. They are, in fact, a different set of systems.

The case I have described is ideal, but not false. It represents the common experience of those who have developed the geologic histories of remote districts, and attempted to correlate them with the geologic history of Europe. There does not exist a world-wide system nor a world-wide group, but every system and every group is local. The classification developed in one place is perfectly applicable only there. At a short distance away some of its beds disappear and others are introduced; further on, its stages cannot be recognized; then its series fail, and finally its systems and its groups.

If I have properly characterized stratigraphic systems—if they are both natural and local—it goes without saying that the classification of the strata of all countries in the dozen or so systems, as proposed by some of the members of the Congress, is impossible.

I hasten to add that from the point of view of these gentlemen what they advocate is not necessarily impossible, for they have a different conception of a system. They regard it not as local but as universal. It is their privilege to define their terms as they please, and we will not dispute about mere words, but I cannot too strongly or too earnestly insist that a system which is universal is artificial. It may be natural in one geologic province, but it is artificial in all others. Take for example the Jurassic. It is a natural system in Europe. In the eastern United States no strata are called Jurassic with confidence, and at the west the rocks called Jurassic merge with those called Triassic. In India, Medlicott tells us, a Jurassic fauna occurs at the summit of a great natural system containing a Permian fauna near its base. In New Zealand, according to Hutton, a continuous rock-system, dissevered by great unconformities from other systems, bears at top fossils resembling those of the lower Jurassic, and lower down fossils of Triassic facies. To establish a Jurassic system in either of these countries it is necessary to divide a natural system, and a Jurassic system thus established would be necessarily artificial.

This is the sort of classification implied by the assumption that systems are world-wide. It is not impossible, but it is highly unadvisable. It is classification for the sake of uniformity, and its uniformity is Procrustean. The natural systems of a region are the logical chapters of its geologic history. If you group its strata artificially according to the natural divisions of another region, you mask and falsify its history. The geologic history of the earth has as great local diversity as its human history. As in human history, there are inter-relations and harmonies and a universal progress, but these are perceptible only in the general view, and the student whose preconceptions lead him to exaggerate the harmonies and ignore the discrepancies perverts the meaning of every page.

I prefer, therefore, my own definition of system, making it natural and consequently local, and I earnestly oppose any attempt to coerce the geology of one country in a rigid matrix formed over and shaped by the geology of another country.

The ideas I oppose have arisen in connection with the work of correlation. Some geologists appear to regard correlation as the determination in distant localities of identities; the more philosophic regard it as the determination of the actual relations, whether they be of identity or difference. With the former the basis of correlation is the universality of geologic systems; with the latter it may be said to be the universality of geologic time.

Now in the comparative study of local geologic histories, just as in the comparative study of local human histories, it is a

matter of convenience to have a common scale of time. It is not essential, but it is highly convenient. In human history we use an astronomic scale of equal parts, designating each unit by a number. In geology no scale of equal parts is available, and we employ the eras and periods, and to some extent the epochs, of the local geologic history first deciphered—that of Europe. These time-divisions bear the same names as the groups, systems, and series of strata whose deposition occurred within them.

So far as the science of geology is concerned the selection of Europe as its first field of study was a matter of chance, and the adoption of the European time scale as a general standard may therefore be said to have been accidental. Though the local rock scheme on which it is based is natural, the time scale, considered as universal, is arbitrary. Another locality would have afforded a different scale, but its authority would neither be greater nor less. The scale being recognized as arbitrary, and a mere matter of convenience, it is legitimate to modify and fix it by formal convention. The Congress can do good service to geologic technology by putting it in the best possible shape and giving it an official status. In my judgment only a small number of divisions should be admitted, not more than the number of periods of the European scheme. In a general way the durations represented by the co-ordinate divisions should be as nearly equal as practicable, but a certain concession might be made to chronologic perspective on account of our superior opportunities for studying the later history. Some of the shorter periods might perhaps be united under new names. Each line of division between periods should be defined by means of a stratigraphic plane of division, and this can be done with precision if a locality is made part of the definition.

Especially should pains be taken to declare the arbitrary nature of the scale. Even with this precaution it will be misconstrued by many, for there is a tendency of the mind to attach undue weight to classification. Wherever we draw lines of separation, we lose to a certain extent the power to recognize continuity. When, for example, the clock strikes twelve on New Year's Eve, time seems to stop and begin again. We speak of the achievements of the nineteenth century—and despite ourselves we think of them too—as though a new industrial epoch began in A.D. 1800. And so it is easy for the beginner in geology to accept as discontinuous the eras and periods of which his text-book treats, and it is hard for him afterward to unlearn the lesson.

There is reason to believe that confusion of ideas in regard to geologic classification has been fostered by the employment of the same set of names for the divisions of the time scale and for the local terranes on which they are founded. It might be well to furnish the time scale with names suggesting times—such names as the brothers Rogers applied to the terranes of Pennsylvania; but so radical a change is hardly feasible, especially as we should thus lose the mnemonic connection of times with corresponding terranes. I propose, as a means of accomplishing the end with the least inconvenience, that a set of time words be derived from the terrane names by modifying the final syllables. The time words should all have the same termination, and that should differ from any terminations occurring in the terrane names. I suggest for the ending of time words the syllable *al*. With such a nomenclature, Jurassic and Devonian would denote only certain European rock systems, while Jural and Devonal would denote periods of the standard time scale; and we could speak of the Chico-Tejon series as partly Eocenal and partly Cretaceal without seeming to imply the existence in California of the Eocene and Cretaceous systems of Europe.

A few minutes ago I opposed the differentiation of words by terminations because it abrogated the power of indefinite expression; I now favour it for the same reason. It is well to be indefinite as to the taxonomic rank of terranes while their characters are imperfectly known, but it is not well to confuse terranes with times.

It is not to be assumed that a time scale adopted now as the best possible will continue indefinitely to be the best possible; the day will inevitably come when it can be improved. In the fuller light of the future we may recognize as very unequal periods that we now deem equivalent, and the possibilities of defining pre-Cambrial periods are unlimited. Even now there are announced beneath the lowest fossil-bearing terrane of the Lake Superior region two systems of clastic rocks limited above and below by great unconformities, and Irving demands their recognition as a group, distinct from the Archæan. If his voice heard, the time scale will include an era between the Palæozoal and the Archæal, and this era will supply the needs of the systematist until great additions have been made to our present knowledge of the older rocks.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The resignation of Prof. Prestwich and the continued illness of Prof. Moseley have produced two gaps in the scientific professoriate this term. Though the Professor of Geology has left Oxford, his successor will not be appointed until after Christmas. In Prof. Moseley's absence the work of the morphological department is being carried on by Mr. Hatchett Jackson and Mr. G. C. Bourne, who has just been elected to a Fellowship at New College.

In the department of physics, Prof. Clifton is giving no honour lectures, but has an elementary course on mechanics and acoustics. Mr. Selby is lecturing on elasticity and on electrostatics, both treated mathematically. At Christ Church, Mr. Baynes gives lectures and practical instruction in electricity and magnetism. Sir John Conroy and Rev. F. J. Smith are lecturing on elementary physics at Balliol and Trinity.

The chemical department at the Museum has been strengthened by the addition of Mr. V. H. Veley to the staff; the usual courses of lectures are being given by Mr. Fisher and Dr. Watts, and Prof. Odling is lecturing on glycerine and its derivatives. At Christ Church, Mr. Vernon Harcourt has a class for quantitative analysis, and lectures on elementary chemistry. Sir John Conroy is lecturing on chemical physics at Balliol.

The Physiological Laboratory is continuing its usual work with little alteration. Prof. Burdon Sanderson's lectures are on circulation, respiration, and bodily motion.

Prof. Balfour, besides lecturing on extinct plants for the Professor of Geology, is giving a course on general morphology. Dr. Tylor continues to expound the Pitt-Rivers Collection, a fresh portion which will shortly be rearranged and opened to the public.

Mr. Primrose McConnell is lecturing on the principles of agriculture, mainly for the benefit of India Civil Service candidates.

During this term there will be an examination for Scholarships in Natural Science at Balliol, Exeter, Christ Church, and Trinity jointly, beginning on November 17.

A pamphlet has just been issued from the Clarendon Press giving full information as to the methods of obtaining medical degrees in Oxford, and the instruction in medicine provided by the University.

CAMBRIDGE.—The Museum and Lecture Rooms Syndicate have issued another report pressing for new accommodation for Pathology and Physiology, and suggesting that the department of Pathology, including its Museum, could be located in the old chemistry buildings when the new laboratory is completed. They recommend the building of new rooms for Human Anatomy and Physiology on the Corn Exchange site at the northern end of the Museum site. It is an important feature of this scheme that it would leave free the present Human Anatomy Schools and Museum to be added to the frontage available in Downing Street for a Geological Museum.

Messrs. W. H. Macaulay and W. B. Allcock have been appointed Moderators for the year beginning May 1, 1888. Messrs. J. Larmor and W. Welsh are appointed Examiners in Part I. of the Mathematical Tripos for the same year.

Dr. Smith's prizes for the present year have been adjudged as follows:—(1) Mr. Augustus Edward Hough Love, St. John's College, for an essay on "Small Free Vibrations and Deformation of a thin Elastic Shell," and on "The Force and Forced Vibrations of an Elastic Spherical Shell containing a Given Mass of Liquid." (2) Mr. Arthur Berry, King's College, for an essay on "Joint Reciprocants."

SCIENTIFIC SERIALS.

The Quarterly Journal of Microscopical Science for August 1887, vol. xxviii. part 1, contains the following papers:—On the anatomy of the Madreporia, part 3, by Dr. G. Herbert Fowler, (plates i. and ii.) In this memoir the author deals with the

anatomy of *Turbinaria mesenterina* (?), of *Lophohelia prolifera*, of *Seriatopora subulata*, and of Pocillopora, with a note on the skeleton of Flabellum.—On the anatomy of *Mussa corymbosa*, and *Euphyllia glabrescens*, and on the morphology of the Madreporian skeleton, by G. C. Bourne (plates iii. and iv.)—On the intra-ovarian egg of some osseous fishes, by Dr. Robert Scharff (plate v.)—Observations on the structure and distribution of striped and unstriped muscle in the animal kingdom, and a theory of muscular contraction, by C. F. Marshall (plate vi.) The author concludes that in all muscles which have to perform rapid and frequent movements a certain portion of the muscle is differentiated to perform the function of contraction, and this portion takes on the form of a very regular and highly modified intracellular network. This network, by its regular arrangement, gives rise to certain optical effects, which cause the peculiar appearances of striped muscle; the contraction of the striped muscle-fibre is probably caused by the active contraction of the longitudinal fibrils of the intracellular network; the transverse networks appear to be passively elastic, and by their elastic rebound cause the muscle to rapidly resume its relaxed condition when the longitudinal fibrils have ceased to contract; they are possibly also paths for the nervous impulse.—On the fate of the muscle-plate and the development of the spinal nerves and limb plexuses in birds and mammals, by Dr. A. M. Paterson (plates vii. and viii.)—On the ciliated pit of Ascidians and its relation to the nerve-ganglion and so-called hypophysial gland, and an account of the anatomy of *Cynthia rustica* (?), by Lilian Sheldon, Bathurst Student, Newnham College (plates ix. and x.). Suggests that the original function of the ciliated pit was the aëration of the brain, with which it communicates in the case of Clavellina; where also its posterior part acts as a reservoir to carry off the secretion of the gland.—On the tongue and gustatory organs of *Mephitis mephitica*, by Dr. Frederick Tuckerman (plate xi.) This memoir is preceded by an interesting account of the literature relating to the position and structure of the taste organs of vertebrates.—On the quadrate in the Mammalia, by Dr. G. Baur. He thinks that there is little doubt but that the quadrate of the lower vertebrates is contained in the sygomatic process of the mammals.—On the hæmoglobin crystals of rodents' blood, and on an easy method of obtaining methæmoglobin crystals for microscopic examination, by Dr. W. D. Halliburton.

Bulletin de l'Académie Royale de Belgique, August.—Note on the oscillations of a pendulum produced by the displacement of the axis of suspension, by E. Ronkar. The object of these researches is to ascertain the possibility of recording the slight oscillations in the crust of the earth by means of a freely suspended pendulum. It is shown that the pendulum retains a movement imparted by a certain number of horizontal undulating impulses, whenever the duration of the oscillation of the pendulum is the same as that of the axis, but not otherwise. From this may be deduced an experimental process for determining the periodical irregularities in the movement of diurnal rotation.—On the colloidal sulphuret of cadmium, by Eug. Prost. To the colloidal solutions of arsenious, stannic, and other sulphurets already determined, the author here adds the sulphuret of cadmium, which was hitherto known only in the insoluble state. He obtains a colloidal solution of this compound by passing hydrosulphuric acid through water holding in suspension freshly precipitated cadmic sulphur, and afterwards eliminating by the action of heat the hydrosulphuric acid dissolved in the liquid. A spectroscopic study of this clear yellowish liquid shows that the cadmic sulphur is really in a state of solution, the solution presenting all the characters hitherto ascribed to all dissolved colloidal substances.—Description of some new Cucurbitaceæ, by M. Alfred Cogniaux. This paper contains an account of fourteen new species and of several varieties, forming an important addition to the author's general monograph of this family published in De Candolle's "Monographiæ Phanerogamum."

SOCIETIES AND ACADEMIES.

London.

Entomological Society, October 5.—Dr. Sharp, President, in the chair.—Mr. Jacoby exhibited a specimen of *Aphthonoides beccarii*, Jac., a species of Haltica having a long spine attached to the posterior femora; also a specimen of *Rhagiosoma madagascariensis*.—Mr. Stevens exhibited a very dark specimen of *Crambus perlellus* from the Hebrides, which its captor supposed to be a new species. Mr. Porritt remarked that this brown form of *Crambus perlellus* occurred at Hartlepool with the ordinary typical form of the species, and was there regarded as only a variety of it.—Mr. Slater exhibited a specimen of *Gonepteryx cleopatra*, which was stated to have been taken in the North of Scotland. Mr. Jenner Weir remarked that although the genus *Rhamnus*—to which the food-plant of the species belonged—was not a native of Scotland, some species had been introduced, and were cultivated in gardens.—Mr. South exhibited about 150 specimens of *Boarmia repandata*, bred from larvæ collected on bilberry in the neighbourhood of Lynmouth, North Devon, including strongly marked examples of the typical form, extreme forms of the var. *conversaria*, Hüb., a form intermediate between the type and the variety last named, and examples of the var. *destrigaria*, Haw. Mr. South said that an examination of the entire series would show that the extreme forms were connected with the type by intermediate forms and their aberrations.—Mr. Poulton exhibited young larvæ of *Apatura iris*, from the New Forest; also eight young larvæ of *Sphinx convolvuli* reared from ova laid on the 29th of August last. He said the life-history of the species was of extreme interest, throwing much light upon that of *Sphinx ligustri*, as well as upon difficult points in the ontogeny of the species of the allied genera *Acherontia* and *Smerinthus*. Mr. Stainton said he was not aware that the larvæ of *Sphinx convolvuli* had ever before been seen in this country in their early stages. Mr. McLachlan remarked that females of this species captured on former occasions, when the insect had been unusually abundant, had been found upon dissection to have the ovaries aborted.—Mr. R. W. Lloyd exhibited specimens of *Elater pomonæ*, and of *Mesosa nubila*, recently taken in the New Forest.—Mr. Porritt exhibited a series of melanic varieties of *Diurnea fagella*, from Huddersfield, and stated that the typical pale form of the species had almost disappeared from that neighbourhood.—Mr. Goss exhibited, for Mr. J. Brown, a number of puparia of *Cecidomyia destructor* (Hessian Fly), received by the latter from various places in Cambridgeshire, Norfolk, Suffolk, and Wiltshire. He also exhibited a living larva of *Cephus pygmæus*, Lat. (the Corn Sawfly), which had been sent to Mr. Brown from Swaffham Prior, Cambridgeshire, where, as well as in Burwell Fen, the species was stated to have been doing considerable damage to wheat crops. Mr. Verrall, in reply to a question by Mr. Enock, said he believed that the Hessian Fly was not a recent introduction in Great Britain, but had been here probably for hundreds of years. He admitted that he was unable to refer to any but recent records of its capture. Prof. Riley said he was unable to agree with Mr. Verrall, and believed that the Hessian Fly had been recently introduced into this country. Its presence here had not been recorded by Sir Joseph Banks, by Curtis, by Prof. Westwood, by the late Mr. Kirby, or by any other entomologist in this country who had given especial attention to economic entomology. It seemed highly improbable, if this insect had been here so many years, that its presence should have so long remained undetected both by entomologists and agriculturists. Prof. Riley said it had been stated that the insect was introduced into America by the Hessian troops in 1777, but this was impossible, as its existence at that date was unknown in Hesse. Mr. McLachlan, Capt. Elwes, Mr. Verrall, Mr. Jacoby, and Dr. Sharp continued the discussion.—Mr. J. Edwards communicated the second and concluding part of his "Synopsis of British *Homoptera-Cicadina*."—Prof. Westwood contributed "Notes on the life-history of various species of the Neuropterous genus *Ascalaphus*."—Capt. Elwes read a paper "On the Butterflies of the Pyrenees," and exhibited a large number of species which he had recently collected there. Mr. McLachlan said he spent some weeks in the Pyrenees in 1886, and was able to confirm Capt. Elwes' statements as to the abundance of butterflies in that part of the world. The discussion was continued by Mr. Distant, Mr. White, Dr. Sharp, and others.

Mineralogical Society, October 25.—Anniversary Meeting.—Mr. L. Fletcher, President, in the chair.—After the reading of the Report, the following were elected Officers and Council for the ensuing session:—President: L. Fletcher. Vice-Presidents: Rev. S. Haughton, F.R.S., W. H. Hudleston, F.R.S. Council (in place of Messrs. Burghardt, Danby, Dobbie, and Lewis, the retiring Members): Prof. A. H. Church, Townshend

M. Hall, Colonel C. A. M'Mahon, J. Stuart Thomson. Treasurer: Rev. Prof. T. G. Bonney, F.R.S. General Secretary: R. H. Scott, F.R.S. Foreign Secretary: T. Davies. Auditors: B. Kitto, F. W. Rudler.—The following papers were read:—On a meteoric iron, containing crystallized chromite, found in Greenbrier Co., West Virginia, about the year 1880, by L. Fletcher, President.—On the nature and origin of clays, by J. H. Collins.—Note on the occurrence of what may prove to be a new mineral resin, by J. Stuart Thomson.—On a variety of glaucophane from the Val Chivone (Cottian Alps), by Rev. Prof. T. G. Bonney, F.R.S.—On the discovery of leucite in Australia, by Prof. J. W. Judd, F.R.S.—On proustite containing antimony, by H. A. Miers and G. T. Prior.—Description of a new student's goniometer, by H. A. Miers.—On rutile needles in clays, by J. J. H. Teall.

PARIS.

Academy of Sciences, October 24.—M. Janssen in the chair.—On naphthol as an antiseptic, by M. Ch. Bouchard. From the experiments here described it is shown that naphthol, hitherto limited to the local treatment of certain cutaneous diseases, may with perfect safety be applied inwardly. Its antiseptic and toxic properties have been accurately determined, with the result that, owing to its slight solubility, it is to be preferred in certain cases to all known antiseptic medicines.—Remarks on the physical principle on which is based M. Clausius's new theory of steam-motors, by M. G. A. Hirn. The view here contested is that the cylinder may be regarded as impermeable to heat, and consequently that the exchange of heat between its walls and the steam at each stroke of the piston is a factor which may be neglected by the practical mechanician. M. Hirn claims that most English and American engineers have adopted his views in the "Hirn-Zeunner controversy."—On the congelation of ciders, by M. G. Lechartier. The author's experiments make it evident that the fermentation of ciders is not destroyed but only diminished even after being kept for nine days at a temperature of 18° C. below freezing-point.—Remarks accompanying the presentation of the "Statistique de la Superficie et de la Population des Contrées de la Terre," by M. E. Levasseur. This work, which appeared originally in the *Bulletin de l'Institut international de Statistique* for 1886–87, comprises 103 tables, in three parts—the first devoted to Europe, the second to the other divisions of the globe, the third to general conclusions and comparative details for the whole earth. In this part the area and population of the various divisions of the world are thus tabulated for the year 1886:—

	Area in millions of square kilometres.	Population. In millions.	Density per square kilometre.	Ratio to the whole population of the world.
Europe	10·0	347	34	23·4
Africa	31·4	197	6	13·3
Asia	42·0	789	19	53·2
Oceania	11	38	3·5	2·6
North America	23·4	80	3·4	5·4
South America	18·3	32	1·7	2·1
	136·1	1483	10·9	100·0

It is pointed out in the introduction that nearly two-thirds of mankind are concentrated in a relatively small space, about 11 millions of square kilometres, or one-twelfth of all the dry land, divided into three great groups: West, Central, and South Europe (245 millions of inhabitants, and 3·5 millions of kilometres); the Anglo-Indian Empire (254 and 3·6); and China, with Manchuria and Japan (430 and 4).—On the third scientific voyage of the *Hirondelle*, by Prince Albert of Monaco. Besides many hundreds of floats sent adrift between the Azores and Newfoundland, several captures were made from great depths with the sounding-gear, which worked easily down to 3000 metres from the surface. Amongst the prizes were several undescribed fishes, Gorgons, siliceous Sponges of the Hexactinellid family, a soft Urchin (*Phormosoma*), numerous Amphipod and Isopod Crustaceans, Solasters, Ophiures, and Hyas of great size, besides a moon-fish weighing nearly 300 kilogrammes, and furnished with a true caudal appendage.—On Newton's chromatic circle, by M. G. Govi. It is shown that this law, of which Newton himself offered no demonstration, is often at fault, because it expresses no certain theoretic principle, nor any rigorously observed theoretic fact. Nevertheless it may still yield approximately correct useful results when it is required to express the complex sensations experienced by the organ of sight.—Positions of Brooks's Comet (January 22, 1887) measured with the 8-inch equatorial of the Observatory of Besançon, by M. Gruey. The positions are calculated for the period ranging from February 24 to April 29.—On magnetizing by influence, by M. P. Duhem. The author communicates the chief results of some studies based on the principles of thermodynamics, and undertaken for the purpose of removing some of the difficulties presented by Poisson's theory.—Action of sulphureted hydrogen on the salts of cobalt, by M. H. Baubigny. Some years ago the author showed that all the salts of nickel are transformed to sulphides when their solutions are treated with hydrosulphuric acid at the ordinary temperature. He now shows that a like treatment of the salts of cobalt yields very similar results.—On the quantitative analysis of titanic acid, by M. Lucien Lévy. A new method of analysis is described, which is more rapid and yields more accurate results than that hitherto in use.—On certain processes capable of increasing the resistance of the organism to the action of microbes, by M. Charrin. It is shown, by experiments carried out on rabbits, that under specified conditions the resisting power of the animal may be greatly increased and rendered more or less complete and lasting by inoculating or injecting the soluble products of the cultivated virus of certain microbes.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Philip's Handy Volume Atlas of the British Empire (Philip).—Practical Chemistry: Muir and Carnegie (Clay).—Elementary Chemistry: Muir and Slater (Clay).—Essays relating to Indo-China, 2nd series, 2 vols. (Trübner).—On a Surf-bound Coast: A. P. Crouch (Low).—The Mammoth and the Flood: H. H. Howorth (Low).—The Natural History of Commerce, 3rd ed.; The Technical History of Commerce, 3rd edition; The Growth and Vicissitudes of Commerce, 3rd edition; Recent and Existing Commerce, 3rd edition: Dr. J. Yeats (Philip).—Proceedings of the American Academy of Arts and Sciences, December 1886 to May 1887 (Boston).—Bulletin de la Société Impériale des Naturalistes de Moscou, 1887, No. 3 (Moscou).—Zeitschrift für Wissenschaftliche Zoologie, xlv. Band, 4 Heft (Williams and Norgate).—Morphologisches Jahrbuch, Eine Zeitschrift für Anatomie und Entwickelungsgeschichte, xiii. Band, 1 Heft (Engelmann, Leipzig).—Encyklopædie der Naturwissenschaften. Zweite Abth. 44 und 45 Lief. Handwörterbuch der Chemie; Erste Abth. 52 und 53 Lief. Handwörterbuch der Zoologie, Anthropologie, und Ethnologie (Williams and Norgate).—Journal of the Scottish Meteorological Society, 3rd series, No. iv. (Blackwood).—Animals from the Life, edited by A. B. Buckley (Stanford).

CONTENTS.

THURSDAY, NOVEMBER 10, 1887.

A CONSPIRACY OF SILENCE.

THE Duke of Argyll is eminent as a statesman, and has won distinction as a man of science. The mental qualities, however, which lead to success in these capacities are widely different; nay, in the opinion of some, are almost oppugnant. To the man of science, truth is as a "pearl of great price," to buy which he is ready to part with everything previously obtained; to the statesman, success is the one thing needful, for the sake of which hardly any sacrifice appears too great. This is not said wholly as a reproach: it "takes all sorts to make a world." The ardour of the follower of the ideal, which may degenerate into recklessness, is wholesomely checked and beneficially qualified by the calmness of one who has to deal practically with mankind, and has learned by experience that evolution rather than catastrophic change is the law of life, and is in accordance with the analogy of Nature. Still the two types of mind are commonly diverse, and the Duke of Argyll has recently afforded a remarkable instance of the extreme difficulty of combining in one person these apparently opposite characters.

This instance is afforded by an article which appeared in the *Nineteenth Century* for September last, and is commented on by Prof. Huxley in the number for the present month. The Duke's article bears the somewhat imposing title of "The Great Lesson." Prof. Huxley's reply forms a part of an article entitled "Science and the Bishops." As the charge which the Duke has in effect brought against men of science is a very grave one, and as some of the readers of NATURE may not be constant readers of the chief monthly magazines, a brief notice of both accusation and reply may not be without interest.

The moral of "The Great Lesson" is, practically, "beware of idolatry." The scientific world, in the Duke's opinion, has been for some time bowing down to the idol of Darwin and the theory of evolution, which is the fundamental dogma of that cult. Like a prophet of old he raises a warning voice, and points out that the feet of the golden image are in part composed of clay. In the North has been hewn the stone which shall shatter those fragile supports and lay the idol prone in the dust! To abandon metaphor, this is the state of the case. Among the results of Mr. Darwin's labours during the voyage of the *Beagle* in the years 1831-36, when he accumulated that vast store of observations which served as a foundation for "the Origin of Species by means of Natural Selection," was a theory of the formation of Coral Reefs and Atolls, set forth in a volume entitled "On the Structure and Distribution of Coral Reefs" (published in 1842 and republished in 1874). Of this theory the Duke gives an outline in "The Great Lesson," executing this portion of his task so fully in the spirit of a just judge, and with so little of the craft of an advocate, as to leave nothing to be desired for lucidity of statement and cogency of reasoning. In fact, in the judge's summing up, the case for the defence appears stronger than that for the prosecution—so much so, indeed, as to suggest that the difference is due to their inherent merits rather than to the mode of statement. However, be that as it may, the Duke thus pronounces judgment, and in so doing passes a censure, stinging if deserved, on the men of science of this generation.

These are his words (*Nineteenth Century*, p. 305):—

"Mr. Murray's new explanation of the structure and origin of coral reefs and islands was communicated to the Royal Society of Edinburgh in 1880, and supported with such a weight of fact and such a close texture of reasoning that no serious reply has ever been attempted. At the same time, the reluctance to admit such an error in the great idol of the scientific world, the necessity of suddenly disbelieving all that had been believed and repeated in every form for upwards of forty years, of cancelling what had been taught to the young of more than a whole generation, has led to a slow and sulky acquiescence, rather than to that joy which every true votary of science ought to feel in the discovery of a new truth, and—not less—in the exposure of a long-accepted error."

Again:—

"The overthrow of Darwin's speculation is only beginning to be known. It has been whispered for some time. The cherished dogma has been dropping very slowly out of sight. Can it be possible that Darwin was wrong? Must we indeed give up all that we have been accepting and teaching for more than a generation? Reluctantly, almost sulkily, and with a grudging silence so far as public discussion is concerned, the ugly possibility has been contemplated as too disagreeable to be much talked about; the evidence old and new has been weighed again and again, and the obviously inclining balance has been looked at askance many times. But, despite all averted looks, I apprehend it has settled to its place for ever, and Darwin's theory of the coral islands must be relegated to the category of the many hypotheses which have indeed helped science for a time, by promoting and provoking further research, but which in themselves have now finally kicked the beam."

This, then, is "The Great Lesson":—

"It is that Darwin's theory is a dream. It is not only unsound, but is in many respects the reverse of the truth. With all his conscientiousness, with all his caution, with all his powers of observation, Darwin in these matters fell into errors as profound as the abysses of the Pacific."

This is plain speaking. In words which admit of no ambiguity the Duke declares that Darwin was wrong; that Mr. Murray set him right; and that the latter, instead of receiving a welcome, was met with a virtual conspiracy of silence on the part of scientific men. Of these three assertions—which are to a considerable extent independent one of another—the first and second are obviously very much matters of opinion, because, if the third statement be true, it is clear that no verdict has been delivered by experts, but that, like an Irish jury, they have professed themselves unable to agree, because the facts were so strong that even they could not bring in a verdict of acquittal. The third assertion, however, is much more a matter of fact, not difficult to substantiate, and at any rate, if incorrect, easy to disprove.

In regard, then, to the first and second it may suffice to follow Prof. Huxley's example and be content with expressing a doubt as to the accuracy of the Duke's

assertions. In the face of statements so definite as those quoted above, this may seem presumptuous. They read almost like the sentence of an ecclesiastical court, which it is heresy to question. *Caledonia locuta est, causa finita est*, seems to be their tone; and if one whisper a doubt, one expects the familiar conclusion, *Anathema sit!* But men of science, as all the world knows, are sceptics. Have they yet awakened and rubbed their eyes, and said of Darwin's theory "Lo! it was a dream"? What says Prof. Huxley? He asserts that Darwin's confidence in the accuracy of his own theory was not seriously shaken, as the Duke alleges, and quotes as conclusive evidence a letter from Prof. Judd, who gives the results of a conversation which he had with Darwin no long time before the death of the latter. Prof. Huxley also intimates that to himself—though tolerably familiar with coral reefs—the new theory is at first sight so far from fascinating that, until he can devote a considerable time to a re-examination of the whole subject, he must be content to remain "in a condition of suspended judgment," and that Prof. Dana, "an authority of the first rank on such subjects," has pronounced against the new hypothesis in explicit terms. Undoubtedly, Mr. Murray has obtained distinguished converts, but with such differences of opinion among those best qualified to judge, it is certainly going further than is warranted by facts to insinuate if not to assert that he has convinced the scientific public. Very probably more than a minority of them are in my own position, which perhaps I may be pardoned for stating. They, like myself, have never had the opportunity of forming an independent judgment upon the matter, but they see some very serious difficulties—difficulties which are of a general rather than of a special nature—in the new explanation. At present these difficulties do not appear to them to have been overcome; so that, while admitting that Mr. Murray's hypothesis may sometimes apply, and that Darwin either may have expressed himself a little too sweepingly, or may have been understood so to do, the theory of the latter is capable of a more general application, and presents less serious general difficulties, than does that of Mr. Murray.

We come, then, to the third charge, which is the most serious one, because it affects the morality of scientific men; and many of them, like myself, are old-fashioned enough to resent being called a knave more than being called a fool. Has Mr. Murray been met by "a conspiracy of silence"? The Duke, in asserting this, must have been strangely oblivious of, or, among the cares of a statesman, have failed to keep himself *au courant* with, the literature of geology. Prof. Huxley denies the assertion, and adduces in his support an answer to an inquiry which he had addressed to Prof. Judd. The facts, according to these authorities, are briefly as follows:—Mr. Murray's views were duly published, as the Duke himself states; they were favourably regarded by the authorities at the *Challenger* Office; they were expounded, one might almost say advocated, on more than one occasion (*e.g.* in this very journal) by Dr. A. Geikie. His text-book in the year 1882 not only took the leading place, as it still does, but also was then the only complete text-book on a large scale for this country. On p. 468 is a full statement of Mr. Murray's views. They have also been referred to at more or less length in many treatises and journals, both English and foreign. As Prof. Judd remarks, "If this be a 'conspiracy of silence,' where, alas! can the geological speculator seek for fame?"

Thus the main charge is disproved. One special item in it, however, as peculiarly offensive, yet calls for a brief notice. The Duke states: "Mr. John Murray was strongly advised against the publication of his views in derogation of Darwin's long-accepted theory of the coral islands, and was actually induced to delay for two years." Now, if these words do not amount to an imputation of bad faith on the part of Mr. Murray's adviser, and are not by insinuation extended to others, I do not know what they mean, or why they have been penned. But, as Prof. Huxley observes, "whether such advice were wise or foolish, just or immoral, depends entirely on the motive of the person who gave it." The remark is perfectly just. Who, I would ask, who is old enough to look back on a quarter of a century of work, has not occasionally said, "Wait a bit," to some younger friend, who has come in the first incandescence of a brilliant hypothesis? I have so sinned. Sometimes I have been wrong and my young friend right, but not always. Still, I know myself fallible. As the late Master of Trinity said, "We are all fallible mortals, even the youngest amongst us." Yet I am not ashamed. I will not put on sackcloth and ashes, and I mean to sin again. Perhaps it is because I am naturally unimaginative; perhaps I am come to the season of autumn leaves; but I have always looked askance at a brilliant hypothesis, and now distrust it more than ever. I have lived long enough to see many a one go up *whoosh!* like a sky-rocket, all stars and sparks, and come down exploded, all stick and stink!

So the "great lesson" has been read, and the scientific world, I fear, has not repented or rent its clothes. But it has heard, and not without indignation. The Duke of Argyll has made grave charges against the honour and good faith of men of science, and they ought to be grateful to Prof. Huxley for his prompt repulse of the attack and his stern rebuke of the assailant. As it seems to me, reply is only possible on one point—namely, the special charge mentioned above. Hence the Duke of Argyll is bound to establish or to withdraw the accusation.

Men of science are justly sensitive on this question. Doubtless they are no more exempt from human frailty than any other class of men: we all fail sometimes—nay, too often—to live up to our ideal standard; still, such shortcomings are not common, and anything like a "conspiracy of silence" or any kind of scientific "boycotting" is a thing so improbable as to be almost incredible. Each man must testify according to his own experience; so in conclusion, though it may be deemed impertinent, I will express my own. I have lived now for not a few years among the rank and file of scientific men on more intimate terms than can have been possible for the Duke of Argyll, owing to his exalted station and his high occupations of State, and I am bound to declare that, in a fairly wide experience, I have never found men as a class less self-seeking or more earnest in their desire for truth, more steadfast as friends, or more generous as antagonists.

T. G. BONNEY.

A TEXT-BOOK OF ALGEBRA.

A Text-book of Algebra. By W. Steadman Aldis, M.A (Oxford: Clarendon Press, 1887.)

THIS work is, as we are told in the preface, "the outcome of lectures delivered in the College of Physical Science at Newcastle-upon-Tyne." It discusses, more fully than is usual in books on algebra, the fundamental principles of the science, and its aim is to be of service to the independent student who has not the advantage of "access to large libraries, or intercourse with other mathematical scholars." The object of the author, as might be expected from his eminence as a mathematician and his experience as a teacher, is, in our judgment, likely to be successfully attained in the use of his work. The book is hardly adapted for those students whose object it is to attain such skill and facility in algebraical work as is necessary to face an examination paper in algebra, set by the University examiner of the present day. The examples, though sufficient for illustrating the principles, are not numerous enough for the purpose of developing such skill, nor selected with that special object; and such aids to the attainment of exactness as the various tentative methods of finding the factors of algebraical expressions of different forms, and other aids to insight into their constitution, are only incidentally alluded to. Still, even this class of students will find it a book worthy of reference, when they are revising the fundamental principles on which the science is based, and realizing that all its operations are reducible to a few fundamental laws.

The book is divided into four sections, treating respectively of the fundamental laws and the algebraical operations founded thereon, of equations, of series, and of arithmetical applications.

In the first chapter, headed "Arithmetical Notions," the arithmetical basis of algebra is laid down in a careful discussion of the laws of the simple operations of arithmetic; the commutative laws of addition and multiplication, and the distributive and associative laws of multiplication, being shown to result, both for integral and fractional numbers, from our fundamental conceptions of number. In this chapter particular numbers only are used, and the expression of the results by the use of letters denoting any numbers whatever is relegated to the following chapter on "Algebraical Notation." This would seem to indicate that Prof. Aldis agrees with a commonly accepted notion, that algebra begins with the introduction of letters to denote unspecified numbers. We hold, on the contrary, that, in arithmetic, letters may, and ought to, be freely used to express the unknown quantities of a question, or to sum up in general terms properties of numbers or rules which have been established for solving particular problems; and that only when a result has been obtained by means of organized algebraical operations, instead of by ordinary reasoning, has algebra, properly so called, been employed.

In the second chapter the general results of the first are summed up in a series of formulæ, numbered (1) to (21), to which are afterwards added others, numbered (22) to (25), expressing the laws of indices. The extension of the use of the signs + and − to indicate opposite affections of the quantities denoted by the letters to which they are prefixed is carefully explained; and it is shown by the illustration of "steps" that still wider interpretations may be given to the symbols and formulæ. Upon this foundation the subsequent chapters dealing with the elementary operations on algebraical expressions are based, explicit reference being made to one or other of the formulæ by its number to justify each step in the establishment of the various processes. This method of procedure is sound and logical in itself, yet we fear that the effect of referring to so many apparently independent formulæ must be confusing to the student, and likely to give him incorrect ideas as to the number of independent laws to which all algebraical operations are reducible. This might have been avoided by a preliminary discussion of the formulæ, showing that with the understanding that the letters may denote either positive or negative quantities they are reducible to some five or six fundamental laws, to which, rather than to the particular exemplifications of the laws in these formulæ, it would have been better in the sequel to refer. Thus the formulæ numbered (1), (2), (3) are all included in the commutative law of addition or aggregation—that in an aggregate of positive and negative terms the order of aggregation is indifferent: so, too, (3), (4), (5), (6) are summed up in the "Rule of Signs"—that the addition of a positive aggregate of terms is equivalent to the addition of each term with its actual sign, and that of a negative aggregate is equivalent to the addition of each term with its sign reversed, and similarly for other groups of the formulæ.

The discussion of the highest common factor, lowest common multiple, and fractions, is followed by a chapter on fractional and negative indices, at the outset of which the question of incommensurables is discussed, and it is shown by apt illustrations that the literal symbols of algebra may represent incommensurable as well as commensurable quantities, since the same laws hold good for the former as have been established for the latter. We should have expected that, as a natural sequel to this chapter, logarithms and their properties and uses would have been discussed, but we find no mention even of the word till we come, much later on in the book, to the Exponential Series. There is no logical necessity for postponing the discussion of the nature and properties of logarithms till we can show how their values can be practically calculated, while the enormous practical importance of an acquaintance with their theory and use is a good reason for its introduction at the earliest possible stage.

A chapter on surds and impossible quantities concludes the first section. In this it is shown that the impossible quantity of ordinary algebra is only relatively impossible, since it becomes interpretable as an "operational quantity" when the letter to which it is attached is taken to denote a length in a definite direction—a view which is further illustrated by the discussion of the cube roots of unity as "operational quantities." This, though not a full account of the matter, is satisfactory so far as it extends, and sufficient for the student at this stage.

The specially distinctive features of Prof. Aldis's work are contained in this first section. We trust we have made it plain that we think it well worthy of the study of

the student who desires to attain a clear, logical view of the foundations of algebraical science. The remaining sections demand less comment as presenting less novelty of treatment.

The most noticeable feature in the section on equations is the introduction of the notation, and a discussion of some of the properties, of determinants. We cannot but regret the space that is devoted in this section to the discussion of the processes for the extraction of square and cube roots in the old traditional shape. In the chapter on division the law of formation of the quotient and remainder of a rational integral function of x after division by $x-a$ has been established. Starting from this, a discussion involving nothing more than elementary considerations would lead up to Horner's process in all its generality, which might then be exemplified in the extraction, not only of square and cube roots, but of roots of any degree, both for algebraical expressions and for numbers. How long shall we have to wait for a due recognition in elementary treatises of this comprehensive method, which, whether from a theoretical or from a practical point of view, is one of the most valuable results of a study of algebra?

The section on series commences with the establishment of the usual formulæ for permutations and combinations, as preliminary to the binomial theorem. We observe with satisfaction that the path to the proof of each general formula is smoothed by the prior discussion of a particular case, by which, as every good teacher knows, the principle involved may be more distinctly brought out than in the general proof, where it is too likely to be lost sight of in the generality of the symbols employed. To the chapter on geometrical progression is attached, as we think it always should be, one of its most important applications—namely, that to compound interest and annuities. The chapters on the binomial theorem and other series usually discussed in elementary algebra are clear and satisfactory, though we think a little more prominence should have been given to the distinction of *convergent* and *divergent* series, and a fuller discussion of the tests of convergency and divergency.

The last section includes under the general heading of "Arithmetical Applications," chapters on proportion, continued fractions, indeterminate equations (limited to those of the first degree), inequalities, notation and numbers, and probabilities. It is not intended, we presume, that the study of some at least of these should be postponed till after the study of the previous sections, but that as *Applications* they do not form a necessary part of the general sequence of algebraical results, though it appears to us rather strange that a place for proportion, at any rate, should not have been found in such sequence.

R. B. H.

PRACTICAL BOTANY.

Practical Botany. By F. O. Bower and Sydney H. Vines. Part II. (London: Macmillan and Co., 1887.)

ABOUT twelve or fourteen years ago there occurred in England two events which have had so marked an influence on the development of scientific botany in this country that they are likely never to be lost sight of by our younger school of morphologists and physiologists. One of these events was the introduction into this country of the teaching of Prof. Sachs, of Würzburg; the second and even more important one was the institution by Mr. Thiselton Dyer of a course of botanical instruction at South Kensington on a scale never before attempted. Those who had the good fortune to attend Mr. Dyer's courses of practical botany in the well-known laboratory at what is now the Normal School of Science must always carry with them the stimulating remembrance of the thorough teaching there instituted; and the effect of the exact instruction and inspiriting demonstration so efficient at South Kensington can be obviously traced in the excellent teaching and work of the enthusiastic younger botanists of to-day. The influence took effect on the early development of the present productive botanical laboratories at Cambridge and elsewhere, and the stimulus has since radiated thence in all directions, as is shown not only by the numerous publications of the last eight or ten years, but also by contributions to the new journal, *The Annals of Botany*, just published by the Oxford Clarendon Press, and by the activity and discussions of the botanists at the recent brilliant meeting of the British Association in Manchester.

The salient features of the new course of structural botany were the thorough study of leading types of the vegetable kingdom by means of material dissected and prepared by the students themselves, and the stress laid on the rule that the students should carefully draw what they saw, and thus gather their ideas at first hand. The method was similar to that employed by Prof. Huxley in his course on animal morphology.

It is evident that the little hand-book of practical botany now under review is the outcome of experience gained in continuing this important method of instruction. Part I. of the present work was published two years ago, and dealt with selected types of the Vascular plants. Part II. is now before us, and completes the scheme. It comprises studies of the chief types of lower Cryptogams, from the moss downwards.

We have already pointed out that the essential features of the new teaching are the exact and thorough study of types. Nothing is assumed; but the students are urged to see everything for themselves, and to draw all they see. These important points decide the plan of the work under review. It will be found an excellent and trustworthy guide to any who use it with the types at hand: it will be all but useless to the mere crammer, for there are no illustrations to take the place of actual objects in producing impressions on the student's mind; no lengthy descriptions to interfere with the directness or clearness of the impressions; and no classified "tips" to vitiate and confuse the teaching. We regard it as an excellent sign of the progress of botany in this country that an English work of this description should be forthcoming, and students are greatly indebted to Dr. Vines and Dr. Bower, and those who have contributed to this admirable little monument of practical teaching.

As special features in the book we may commend the selection of types; they are good, for the most part easily obtained, and well known. The treatment of the types in the book is clear, concise, and yet sufficient. The usage of bolder lettering for the chief word in the para-

graph is an admirable device for fixing the student's attention on one thing at a time, and is aided by the numbering and lettering of the paragraphs. The division into sections dealing with groups of characters, leading the student on from the more obvious features to those less easily investigated, also stamps the work as that of experienced teachers, and is eminently English.

The only part of the plan to which objection is likely to be raised by teachers is what may perhaps be termed the reversal of the order of the types. We are ourselves inclined to the opinion that it would have been better to begin with the more lowly organized types, and work upwards to those in which the anatomy and histology become more complex. There is much to be said in favour of the method adopted, but we think that the following two objections to it alone outweigh all we have heard in its favour.

(1) The types are obviously selected as illustrating the chief structural peculiarities of plants, and it might be better to at least indicate the relations of these structures in an order more in accordance with their probable development in the vegetable kingdom.

(2) The plan of teaching which marks the book is the educational one, *i.e.* the observer is led on from simple to less simple ideas. This is only carried out consistently, however, within the individual sections of the book: why should it not be followed throughout the work?

Of course the objection may be anticipated that the sections really lead the student on from the macroscopic to the microscopic characters, and that Algæ and Fungi, for instance, are less easy to investigate, and therefore come last, because they involve the use of the higher powers of the microscope so much. We do not admit that this latter is a difficulty, however, and in reply would simply propose for psychological study the mental attitude of a tyro struggling with his first transverse section of a sunflower-stem. The cutting, preparation, mounting, and finally the involved pattern of cell-walls which he has to unravel, at once plunge him into difficulties at least as great as those met with on the threshold of the study of the Algæ. Moreover, there is evidence in the work that the student is supposed to be acquainted with the use of the microscope, such as would be obtained from a proper course of elementary biology.

Of course, however, it is always open to a teacher to reverse the order of the types in the book; and it only remains for us to say a few words regarding some of those employed in the second volume. *Polytrichum* is selected as the chief type of the mosses, and we think Dr. Bower has done well to illustrate the details of structure by this complex form, surmounting the difficulty presented by its peculiar sporogonium by a comparative study of that of *Funaria*—itself an excellent type. We are glad to see *Marchantia* treated in detail. It is, of course, an out-of-the-way form, and is peculiar even in its own group, but it is an instructive plant, and one that has earned a reputation from the physiological lessons it teaches us. *Polysiphonia* serves as the chief type of the red sea-weeds, and although it has many peculiarities, it has the advantage of being common: the structures of several other Florideæ are shortly compared with that of *Polysiphonia*. *Fucus serratus* forms an excellent type for the study of the brown sea-weeds, and as it is easily obtained, it should be employed in every laboratory course: the details of the actual process of fertilization still offer an interesting problem to any intelligent student, but the chief stages in the process are not difficult to observe.

Passing to the green Algæ, *Œdogonium* seems to us a type well worth thorough study in the laboratory; it is by no means uncommon, and an effort should be made to introduce a definite species of the larger forms as a type. Short studies of *Coleochæte* and of *Ulothrix* are also given. *Vaucheria sessilis* is offered as an example of the Siphoneæ, and we are glad to see it brought well into the foreground; this again is a plant of increasing importance as an instructive plant. Most of the details of the process of fertilization in this Alga offer less difficulties than is commonly supposed, and students should be encouraged to spend some time on their study. A short *résumé* of the main points is given. No doubt the presence of *Protococcus* or *Hæmatococcus* in schedules of elementary biology explains its omission from the present work: *Pleurococcus* and *Volvox* are given, however, and they illustrate several points of importance. In spite of—or perhaps on account of—its very marked peculiarities, we look upon *Spirogyra* as one of the most instructive types that the student can examine, and Dr. Bower has done well to give it a prominent position. It deserves more attention, however, and we would strongly urge an exhaustive treatment of its life-history, germination, and some of the physiological lessons it teaches.

Passing over less important forms, we may now say a few words respecting the Fungi given as types for study. The first section is devoted to *Agaricus*, and a capital study of the structure and histology of the common mushroom is given. Then follows an equally good account of the Æcidiomycetes. Of Ascomycetes, we have *Peziza*, *Parmelia*, *Claviceps*, and *Eurotium*, as types of the chief great groups. A word as to *Peziza*. It is an excellent type, and certain forms can be cultivated, and we hope that in a second edition the author will see his way to introducing a fuller account of some one species. At the same time we are not sure that *Ascobolus* is not a better form for the present purpose: it can be easily cultivated, and its small size is an advantage, since perfect sections can be obtained across the whole plant. Another excellent type is also introduced in the study of the Peronosporeæ. English students are only now becoming aware of the theoretical importance of this group, and we are very glad to see Dr. Bower's section on *Pythium de Baryanum*: it cannot be too well understood that *Pythium* is one of the few parasitic Fungi which may be easily cultivated and followed through all its phases of development in the laboratory. For our own part, we regard it as the best of all Fungi for study, and its life-history and simple structure ought to be thoroughly investigated in every botanical course. It has the additional advantage of being also a saprophyte, and can be cultivated on dead organisms. The Mucorineæ are exemplified by *Mucor* and *Sporodinia*. No type of the Ustilagineæ is given.

Enough has been said to show that the volume pretends to no more than it can fairly claim, and we regard it with confidence as a praiseworthy and successful attempt to record for the benefit of a wide class of

students the methods of teaching so well introduced and so thoroughly carried out in the laboratory at South Kensington, the birth-place of the modern English school of morphological botany. It now remains for one of our competent younger botanists to prepare a course of practical instruction in the physiology of plants, introducing the experiments employed in our best laboratories; and there are signs that such a volume will meet with a hearty welcome from students of botany in this country. The importance of the subject needs no comment.

OUR BOOK SHELF.

A Chapter in the History of Meteorites. By the late Walter Flight, D.Sc., F.R.S. (London: Dulau and Co., 1887.)

THIS work, though left incomplete by the early death of its author, will be found of great service by all who are interested in meteoric studies. The first 144 pages were printed off twelve years ago, and were thus safely beyond revision. The rest of the work has been revised, and the whole has been prepared for press, by editors who, perhaps wisely, have chosen to be anonymous: their part of the task we may dismiss with the remark that it appears to have been executed with at least ordinary care. The task of the author has been to give a brief summary of the memoirs which have been published relative to meteorites since the year 1868, and thus to furnish an appendix to the work of Buchner. To collectors of meteorites such a convenient summary of memoirs, themselves scattered over a wide range of periodicals, chiefly foreign, is invaluable. There are seven plates and six woodcuts: the frontispiece is an excellent engraving of Chladni, who did so much to compel men of science to recognize the reality of meteoric falls. There is also a hand-painted picture of the wonderful meteorite of Busti, in which two minerals new to terrestrial mineralogy were discovered by Maskelyne. In an introduction there is a short sketch of the life and work of the author. Only 240 copies have been printed; the proceeds of their sale are to be added to the Flight Memorial Fund, which at present amounts to £317.

A Hand-book for Steam Users. By M. Powis Bale, M.I.M.E., A.M.I.C.E. (London: Longmans, 1887.)

MR. BALE'S little hand-book supplies a want long felt by steam users. Its contents are entirely of a practical nature, and the technical terms used are very properly those of the ordinary mechanic. The book embraces the whole of the many duties of the engine-driver and fireman, and explains to them what to do, and what not to do, under varying circumstances. The arrangement of the information is simple and effective, the writer evidently knowing how to get at the understanding of those for whom the book is written.

The information and rules given are eminently practical, and will prove very useful to those steam users who do not pretend to be engineers. In the preface we are told that the author has for many years urged the necessity of a compulsory system of boiler inspection, and of granting certificates of competency to those having boilers under their charge. In this we entirely agree, and we trust the time is not far distant when Parliament will establish a system of examination similar to that of marine engineers, under the control of the Board of Trade for all who have charge of stationary boilers and engines, as well as locomotives. Michael Reynolds, the author of several books on the practical working of steam-engines, has long advocated the introduction of certificates of competency for locomotive drivers and firemen. Their duties are as arduous and responsible as those of the marine engineer, and yet this fine class of men is entirely recruited from the lower grades employed in the locomotive running sheds and works, and their promotion generally depends on years of service on the footplate.

Students of steam and mechanical engineering will here find information which, although not generally taught in the lecture-rooms, will indicate some of the many points an ordinary engine-driver has to be thoroughly acquainted with. N. J. L.

The Encyclopædic Dictionary. Vol. VI. (Part II.) (London: Cassell and Co., 1887.)

THE special characteristic of this work is that the compilers have tried to make it combine some of the advantages of an encyclopædia with all the advantages of a dictionary. The result, upon the whole, is very satisfactory. The information given in the encyclopædic part of the work is not, of course, sufficient for students; but it will meet the wants of readers who may wish to obtain a concise and trustworthy account of any subject in which they happen to be interested. Special attention is devoted to the various branches of science, and scientific terms are very carefully defined and explained. So far as we have been able to test the volume of which this is the second part, we have found it in all respects equal to the preceding volumes.

A Treatise on the Principle of Sufficient Reason: a Psychological Theory of Reasoning, showing the Relativity of Thought to the Thinker, of Recognition to Cognition, the Identity of Presentation and Representation, of Perception and Apperception. By Mrs. P. F. Fitzgerald. (London: Thomas Laurie, 1887.)

THIS is neither a treatise nor has it anything particularly to do with the principle of the sufficient reason, or with the philosophical views mentioned in the second title. It is rather a kaleidoscope of phrases, original and otherwise, that have apparently from time to time touched the author's fancy, and are now vaguely but gratefully remembered to have once possessed a meaning for her. Quotations from Ouida, Plato, Lord Dundreary, and other philosophical authorities, are tossed together impartially, without apparent purpose except to fill 400 pages; and though some reference is made occasionally to opinions said to be held by the author, such reference is nearly always too vague to show what the opinions really are. Only the hard-hearted can find even amusement in the book.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

"Infusorial Earth."

THE following letter, addressed to the Secretary of the Royal Society, has been forwarded to us for publication:—

Foreign Office, October 27, 1887.

SIR,—I am directed by the Marquess of Salisbury to state to you, for the information of the President and Fellows of the

Royal Society, that Her Majesty's Consul-General at Christiania has reported that a considerable number of pits of "infusorial earth" containing 85 to 95 per cent. of silica are said to have been discovered in the neigbourhood of Stavanger.

Capital is being sought for the purpose of working the deposits, which are estimated to be capable of yielding 400,000 cubic metres of that rare product. It is affirmed that whilst the similar deposits at Lüneburg, in Hanover, are mixed with sand and gravel, those now discovered are so pure in quality as to be available for most purposes merely after desiccation.

As this discovery may possibly have a scientific interest as well as a commercial value, I am directed to convey the above information to the Royal Society.

I am, Sir, your most obedient humble servant,

T. V. LISTER.

The Secretary, Royal Society, Burlington House.

The Electrical Condition of the Peak of Teneriffe.

THE limited number of observations on atmospheric electricity which have been already made all point, with one exception, to a normal positive difference of potential between a point some few feet above the earth and the ground itself. The only notable exception to this law was found in some observations which were made on the Peak of Teneriffe about thirty years ago. Then it appeared that the condition of the Peak was constantly resinous or negative. These observations were, however, taken with a gold-leaf electrometer, and some doubt has been expressed as to whether the sign of the electricity was correctly obtained.

I therefore thought, when taking a short trip to Teneriffe, that it would be useful to examine this question by means of the improved electrical instruments now available.

Through the courtesy of the Meteorological Office I obtained the loan of a Thomson's portable electrometer, and, through the kindness of Mr. Whipple, received at Kew all necessary instruction in the use of the instrument, and special caution as to the possible difficulty of getting a good "earth" on sun-burnt lava. Any success the observations may have had is entirely due to his care and forethought.

I was only able to stay about a fortnight on the island, but the results obtained were so uniform that there can be no doubt as to their accuracy.

The height of the electrometer fuse was always about 5 feet 6 inches above the ground. At the Port of Orotava, at the base of the Peak, and about 50 feet above sea-level, the mean of eight sets of observations—each set usually consisting of six determinations—gave a potential of 138 volts. The highest was 193, and the lowest 98 volts. These, and all I obtained in Teneriffe, were uniformly positive.

One day I took a skirmishing expedition to the rock of Gayga, a portion of the rim of the old crater, 7100 feet above the sea. On the way up, while on the pretty uniform slope of the mountain, at 3800 feet, the potential was only + 99 volts, while on the rock itself, tension rose to 257 volts. The rock is a long sharp, narrow edge, perhaps half a mile long, with a precipitous cliff of 500 feet on one side. The rock was composed of dry lava, and I thought a little damp, but still the earth observations were not quite so accordant as usual.

A few days later, therefore, when starting for the top of the Peak, I took, as suggested by Mr. Whipple, an ordinary 66-foot iron surveyor's chain to be laid along the ground and connected with the instrument. The readings at different heights, on the way up, were as follows:—

At 5600 feet, on the slope of the mountain, 111 volts.

On the Cañadas, or rough flattish ground that forms the bottom of the old crater, at 5800 feet, 139 volts. The ground here was pumice and pumice dust, so I tried running out the chain to see if the earth-readings would be altered. There was not however the slightest change, and to show the character of the observations five out of the six earth-readings gave the same number.

At the *Estancia de los Ingleses*, 10,500 feet, situated on the slope of the main peak, the potential fell to 118 volts. The sun was setting, and dew falling so fast that the top of the electrometer box was covered with wet. There could be no doubt then of obtaining a good earth.

On the top of the Peak, 12,200 feet, the potential actually rose to no less than 549 volts. This was at 8 o'clock in the morning of October 24. The wind was blowing at the rate of about 10 miles an hour from the north-east, while the dry and wet bulb thermometers marked 31° and 26° respectively. There was a little white frost on the ground, and the earth-readings, without the chain, were remarkably uniform, only differing by the 11-100th of a turn of the screw.

The results of all the observations points unmistakably to the conclusion that during this month of October *the electrical condition of the Peak of Teneriffe were the same as in every other part of the world. The potential was moderately positive at the same distance from the ground even at considerable altitudes, but the tension rose enormously round a sharp point, and a projecting edge of rock.*

It is well known that there are very few thunderstorms in Teneriffe, though one passed near us at Orotava without affecting the indications of the electrometer. Would it not be interesting to measure the potential on the summit of a mountain like Kina Balu in Borneo, which is about the same height as the Peak of Teneriffe, but situated in the heart of the equatorial zone of the constant electrical discharge?

We had one day of very heavy rain, when possibly some negative indications might have been obtained; but I did not think it expedient to let the instrument get drenched.

But, besides obtaining these decisive electrical results, I was also very fortunate in some other observations during the short stay in Teneriffe.

We saw from the *Estancia* the shadow of the Peak at sunset gradually creep along the land and surrounding sea, and then stand up in the air like another peak rising above the horizon. This is what is so often seen from Adam's Peak in Ceylon, and from Pike's Peak in Colorado.

Then our observations confirmed not only the important discovery made by P. Smyth, that cloud is not formed at the junction of a south-west current flowing over a north-east trade, but the even more important fact that there is no such thing as the supposed simple return current from the equator. At Teneriffe, as in every other part of the world I have ever visited, the general circulation of the air is on a complicated screw system, the practical effect of which is that as you ascend, the wind always comes more and more from your left hand as you stand with your back to the wind. You do not come abruptly to a south-west wind over a north-east trade, but pass successively as you rise from the surface from north-east through south to south-west, and then probably to west, or even north-west.

I also made some very important observations on the local formation of halo-forming sky, and got an excellent photograph of the genesis of a cirrus cloud from a moist current rising over the Peak, but space will not allow me to explain the results in this place.

RALPH ABERCROMBY.

21 Chapel Street, London, November 7.

"Toeing" and "Heeling" at Golf.

I FEAR that "P. G. T.'s" reply to my letter on the above subject has left us very much in the same position as before. This is regrettable, as I hoped that further light would have been shed on this interesting mechanical problem. Before complying with the invitation to "think over the result of the impulsive rotation of the club-head," I considered it would be well to get some trustworthy observations on which to reason. With this object our professional, Mr. David Lowe, made twenty-seven tee shots with the driver, while I noted the effect. My instructions to him were, whether striking off the toe or the heel, to drive as truly as he could in the direction of an object selected for that purpose. The effects were as follows:—When the ball went off the heel of the club, the ball in its flight curved to the right, even though its direction commenced obliquely to the left; to this there was no exception. The opposite curve, or to the left, with only one exception, was produced by hitting off the toe. Care was taken to ascertain in each case the point of impact of the ball on the club-face.

I now instructed him to try and curve the ball to the left, striking with the heel of the club, or to "toe it off the heel," in "P. G. T.'s" words. This feat he was unable to perform, and he gave it as his opinion that it could not be done.

Now for my explanation of "toeing" and "heeling" in reply to the invitation of "P. G. T."

Everyone who has played golf is aware that the ball when cleanly struck leaves a round mark upon the face of a new club of about five-eighths of an inch in diameter. This is the measure of the elastic distortion that takes place in the ball by the

impact. The ball is *flattened* against the club-face, and is for the time prevented from revolving. To assist the grip of the club on the ball, lines are scored over the surface of the ball. Now consider the effect of the rotation of the club-head round the centre of percussion when the ball goes off the heel or the toe.

The following diagrams of a "toed" ball will best explain my meaning—

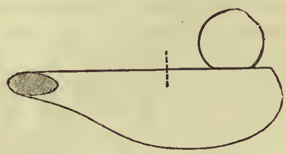


Fig. 1.

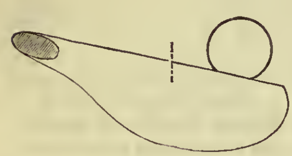


Fig. 2.

Fig. 3.

Fig. 1 shows the club-head and ball on first meeting.
Fig. 2 shows the backward revolution of the club-head due to the impact of the ball on the "toe" of the club.
Fig. 3 shows the club-head on the recovery before the ball leaves the club-face.

It is quite evident that during the movement from the position shown in Fig. 2 to that in Fig. 3 the ball, though adherent to the club-face, is revolving to the left on its own axis at the same rate as the club-head on its axis. This is the direction of spin that curves the ball to the left, or "toes" it. The opposite happens in a "heeled" ball. This rotary movement is necessarily intensified by the involuntary reaction of the wrists, which brings the club-head further round than the elastic recovery of the shaft alone would do.

I venture to think that this is the true explanation of "heeling" and "toeing." The same effects can be produced in other ways; "heeling" may be imitated by "slicing," but that does none the more make it "heeling," nor must we generalize from what happens in bad play, for then—as I know to my cost—all things are possible. T. Mellard Reade.

Park Corner, Blundellsands, October 22.

The Ffynnon Beuno and Cae Gwyn Caves.

The letter from Mr. Worthington G. Smith in the last number of Nature (p. 7) affords a remarkable instance of rushing into print and giving an opinion on a subject with which the writer is unacquainted. Speaking of the deposits in the caves, he states that all he knows about the matter has been derived from reading a very short abstract of a paper read by Dr. Hicks at the recent meeting of the British Association, in which the caves are referred to. Now, so much has been written and published on the Ffynnon Beuno and Cae Gwyn Caves in Nature and other scientific publications, that it is extraordinary that anyone should venture to offer an opinion without previously reading up the literature of the subject. Mr. Worthington G. Smith states that he has visited the caves, and is fairly well acquainted with the Glacial deposits of North Wales and with Palæolithic implements in general, and that his "unbiased opinion is, and will so remain—unless" he gets "very convincing proof to the contrary—that the drift at the caves has been without doubt relaid." We may be thankful for Mr. Smith's opinion, but unfortunately it is not worth anything, as his letter conclusively proves. Although his opinion is of no consequence, I think it should not pass unnoticed, and it affords me an opportunity of stating that during the last month the drift about the entrance of the Cae Gwyn Cave has been again carefully examined, and that the Reports of the British Association Committee have been fully confirmed. G. H. Morton.

Liverpool.

THE VICTORIA UNIVERSITY.

We are glad to observe that the application of the Yorkshire College for admission to the Victoria University has been successful. Doubt was expressed by some members of the Court as to whether the Faculty of Arts in the Leeds institution was strong enough to justify its claim to share in the privileges enjoyed by Manchester and Liverpool. This doubt was overruled. The Charter requires that the provision for teaching both arts and sciences in a College must be "reasonably sufficient" before it can be admitted to the University. It is not, however, intended that it must be equally developed in both directions. The Yorkshire College is no doubt stronger on the scientific side, and was indeed originally called the "Yorkshire College of Science." The name was changed, and the limitation it implied removed, two years after its foundation, when the Council formally took over the classes in literature and history previously carried on by the Cambridge University Extension.

The subjects of a curriculum in Arts are now taught, though the number of Professors engaged in the task is less than could be wished. The Professor of Classics is Principal, and representative of his scientific as of his Arts colleagues on the Council. There is a Professor of English Literature and History, and there are Lecturers in French, German, Italian, and some Oriental languages. An institution which provides a staff competent to teach these subjects, and places its Professor of Classics at its head, cannot be accused of an undue preference for science, and is, we think, fully qualified under the terms of the Charter.

One of the advantages of the federation of local Colleges in a University is that members of their governing bodies will be brought together in its management, and will thus learn practically what is being done in other institutions. Leeds will no doubt be stimulated to attempt to bring its Arts Faculty to the level attained by Manchester. Manchester may learn that combined classes for both sexes are practicable, and that the addition of a Faculty of Technology to those of Arts and Science may be of advantage to all concerned.

The Victoria University is now fairly started on its career, and its constituent Colleges have their future in their own hands. Manchester, Liverpool, and Leeds can confer degrees on students in their principal educational institutions untrammelled by the requirements of any external authority. We believe that this experiment is more promising than an attempt to subordinate local Colleges to our older Universities. Oxford and Cambridge have traditions and peculiarities which those who know them best would wish to survive amid the changes which are from time to time necessary to bring them into harmony with the spirit of the age. Had a close union been formed between these Universities and the local Colleges, it is probable that the Colleges would gradually have destroyed much that in its place in the Universities is useful, or that the Universities would have checked the growth of the Colleges by insisting on the attempt to fulfil conditions which in a manufacturing town are unattainable.

However this may be, it is certain that the most successful provincial Colleges have achieved success without direct connection with Oxford or Cambridge, though from the fact that graduates of these Universities are always to be found on the Professorial Staff they have exercised an indirect and no doubt useful influence.

If the Victoria University succeeds in combining the love of knowledge for its own sake with a readiness to meet the practical requirements of an age in which success in commerce and in learning are closely related, it may acquire a prestige and an authority second to that of no other educational institution in the country.

THERMO-MAGNETIC MACHINES.

IT would seem that at the present time there is being developed in the United States a new kind of engine, capable, at least in theory, of turning, by a magnetic method, the latent energy contained in fuel either into mechanical work or into the energy of electric currents. In this kind of machine the variations produced in the magnetic power of metals, such as iron and nickel, by heating and cooling them, are made the means of generating in the one case electric currents, in the other mechanical motion. The latter application was the earliest to be suggested. In the columns of NATURE (vol. xix. p. 397) will be found a note, extracted from the Journal of the Franklin Institute, upon a thermo-magnetic motor devised by Prof. E. J. Houston and Prof. E. Thomson, of Philadelphia. In this curious apparatus a disk or ring of thin steel is mounted on a vertical axis so as to be quite free to move, with its edges opposite the poles of a horse-shoe magnet. This wheel becomes of course magnetized by induction. When, however, heat is applied at a point on the circumference, the change thereby produced in the magnetic susceptibility of that part causes the disk to move round so as always to bring into line with the poles those portions of the disk which are for the time being the most susceptible to magnetization. Hence if the heating is continuous there will be a continuous rotation; the parts of the disk cooling as they leave the source of heat, and again becoming heated as they pass through the place where heat is being applied. The very same kind of thermo-magnetic motor was re-invented, in 1886, by Prof. Schwedoff, of Odessa, who, in a paper in the *Journal de Physique*, pointed out that this was a genuine case of conversion of heat into work, and gave the theory of the transformation and the cycle of operations from the thermo-dynamic point of view.

The next stage of invention in point of time, though it has only just been made public, is the suggestion by Mr. E. Berliner, of Washington, to use these thermo-magnetic variations in iron for the purpose of generating electric currents. In June 1885 Mr. Berliner filed an application for a patent for an "electric furnace generator," of which the following are the underlying principles:—"If," he says, "I take a magnet and provide it with a coil around its pole or poles, and place before this magnet and in proximity to the coil a piece of iron heated to bright red, nothing will occur to disturb the magnetic field; but the instant the iron cools down to a dull red, the magnetism becomes excited, and a momentary current of electricity is produced in the coil. I may go a step further and have a series of such magnet coils and iron armatures, and by connecting the coils into the same circuit, and cooling the armatures in rotation one after the other, a number of electrical impulses will be produced, which, when they follow one another rapidly, will approximate to a continuous electric current. . . . The current thereby produced might be utilized to charge another coil surrounding the magnet and reinforce the field; and in that case the magnet might be substituted by a tubular core of iron, . . . or a series of coils and magnets might be placed toward one larger armature disk, forming a common armature, heated by one furnace."

The most recent suggestions in this line come from Mr. T. A. Edison, who, independently of Mr. Berliner, has devised an almost identical generator, to which he has given the name of a "pyro-magnetic dynamo." At the recent meeting of the American Association of Science, a paper by Mr. Edison, giving an account of his machines, was read, and has been largely noticed in the non-technical press, as though it were an absolutely new departure in electric science. The famous inventor may certainly lay claim to having worked out in greater detail the practical problems of construction. In the generator there are eight double-pole electro-magnets arranged radially. At the top the eight poles converge toward a central space; and about a foot below the other eight poles converge toward a second central space. In these central spaces lie two soft iron disks, forming the cheeks of the armature, and pierced with eight large holes, each to receive eight vertical armature cores, each of which consists of a roll of corrugated sheet-iron surrounded with a coil of wire insulated with asbestos. The eight wire coils are connected up together and joined to a commutator, just like the coils in the armature of Niaudet's dynamo. This armature stands over a furnace, the heated gases of which are led up through the interstices of the eight rolls of sheet-iron. By the use, however, of a revolving screen of fire-clay the ascending hot gases are cut off successively from some of these tubular cores so that they are alternately heated and cooled, giving rise to electric currents in the coils, which currents are collected above by the action of the commutator. The arrangement appears to have been constructed with Mr. Edison's well-known ingenuity. The inventor has also constructed a pyro-magnetic motor, which consists essentially of a powerful field-magnet (independently excited) having between its poles as a rotating armature a bundle of small vertical tubes of very thin iron, which are packed in a convenient drum-like form and mounted on a vertical spindle. From a furnace underneath rise currents of heated air, and pass through the iron tubes; but, by a screen placed in a suitable position below, the heated air is prevented from rising through some of the tubes, and instead thereof a blast of cool air is blown down these: the cooled tubes, becoming more highly magnetic, are more powerfully attracted by the poles of the field-magnet, and move forward, only to be afresh heated, whilst a new set of tubes comes into position to be cooled and attracted. Mr. Edison states that already a speed of 120 revolutions per minute is practicable; and he is building one of these motors calculated to work at 3 horse-power. Whether the sanguine hopes which he expresses as to the economic working of such motors and generators, as compared with existing engines and dynamos, will be fulfilled in the future is as yet a matter of speculation. But the practical problem, even though it is surrounded by many obvious difficulties, is of so tempting a nature, and the attempt to solve it is so daring, that we must wish to our Transatlantic friends the utmost success in their efforts to supersede the present wasteful methods of utilizing the latent energy of fuel.

NOTES.

THE following is the list of names recommended by the President and Council of the Royal Society for election into the Council for the year 1888, at the forthcoming anniversary meeting on the 30th inst.:—President: Prof. George Gabriel Stokes. Treasurer: Dr. John Evans. Secretaries: Prof. Michael Foster, the Lord Rayleigh. Foreign Secretary: Prof. Alexander William Williamson. Other members of the Council: Sir William Bowman, Bart., Henry Bowman Brady, Prof. Arthur Cayley, W. T. Thiselton Dyer, Prof. David Ferrier, Dr. Edward Frankland, Dr. Arthur Gamgee, Prof. Joseph Henry Gilbert, Prof. John W. Judd, Prof. Herbert McLeod, Dr. William Pole, William Henry Preece, Admiral Sir George Henry Richards, K.C.B., Prof. Arthur William Rücker, the Earl of Rosse, and Sir Bernhard Samuelson, Bart.

MR. F. J. JACKSON has presented to the Natural History Museum an interesting set of animals collected by him during his three years' residence in East Africa. The birds are particularly valuable, and contain many species new to the Museum collection. Mr. Jackson resided for some time in the Kilima N'jaro district, and procured several rare species hitherto only known from the late Dr. Fischer's collections in the Berlin

Museum. From Lamu and Manda Island the additions to the Museum collection are numerous, and supply many desiderata to the series of bird-skins.

THE remains of the great naturalist, Audubon, lie in an obscure and little-visited portion of Trinity Cemetery, New York City, and his tomb is not marked by any distinguishing monument. A movement has been started for the erection of a suitable monument. At the first autumn meeting of the New York Academy of Sciences a Committee was appointed to collect funds and make all necessary arrangements. This Committee, of which Dr. Britten is Secretary and Treasurer, is now ready to receive subscriptions, which will be properly acknowledged. It is estimated that from 6000 to 10,000 dollars will be required. While confident that this amount might be collected in America, the Committee are anxious that interest should be taken in the project by men of science in all departments in all parts of the world.

MR. EDWIN LEES, who died lately at Worcester at the age of eighty-seven, had a considerable reputation as a naturalist. Among his writings are "The Botany of the Malvern Hills," "Pictures of Nature among the Malvern Hills and Vale of Severn," "The Botany of Worcestershire," and "The Forest and Chace of Malvern." He was one of the founders of the Worcestershire Natural History Society, and of the Worcestershire Naturalists' Field Club.

MR. THOMAS BOLTON, of the Microscopists' and Naturalists' Studio, Birmingham, died on Monday. He was in his fifty-seventh year. About a year ago a Civil List pension of £50 per annum was granted to Mr. Bolton in recognition of his services as a naturalist and microscopist. The memorial setting forth his claims, discoveries, and special circumstances was signed by Sir J. W. Dawson and many other eminent men of science.

A CONFERENCE on Technical Education, in which working men took a prominent part, was held last Saturday evening at the Finsbury Technical College. There was a large attendance of students and others. Mr. James Rowlands, M.P., occupied the chair, and Prof. Silvanus P. Thompson read an address on "The Present Position of the Technical Instruction Question." Prof. Thompson urged that the most essential of all the conditions for the organization of an adequate system of technical instruction is the creation of "a real Education Department under a real Minister of Education."

AT the annual meeting of the delegates of the Union of Lancashire and Cheshire Institutes at Crewe on Monday, Lord Derby delivered an excellent address on education. In the course of his remarks he pointed out that the "Institutes of fifty years ago for the most part failed because of the want of good primary schools to feed them. "You have the schools now," he continued, "and what we have to do is to provide the means of carrying on the instruction of those who are willing to learn after the time when they are clear of school, and free to follow their own devices when the day's work is over." Speaking of technical instruction, Lord Derby said :—" We are fighting for the markets of the world ; we have held our own hitherto, but the struggle is sharper than ever, and we cannot afford to throw away any advantage which is possessed by other countries. It may be that, as often happens, we shall find out that we have overrated the benefit of technical teaching, that it can do less for us than we now expect ; but we are not the less bound to try, and to deserve success, whether we get it or not."

SOME electric balloon signalling experiments were carried on at Berchem, in the fortifications outside Antwerp on Wednesday evening, October 26. The system used was known as the Bruce system, and the inventor, Mr. Eric Stuart Bruce, himself superintended the experiments at the invitation of the Belgian War Minister. The balloon used, which had just been purchased of Mr. Bruce by the Belgian Government, was a small one, being only 15 feet in diameter. It had been designed for hydrogen, but though it was filled with very dense coal-gas it lifted 500 feet of electric cable besides its captive rope. This special cable was an improvement on what was formerly used by Mr. Bruce, being now lighter though of the same current capacity. The Bruce key also, which gave great satisfaction, has been lately considerably modified, and can carry any current, the contacts being of carbon, which can easily be renewed on wearing away. The Minister of War, General Pontus, General Wauwermans, Inspector-General of Fortifications at Antwerp, and various other distinguished officers were present, including special delegates from Russia, Holland, &c., &c. The first telegram sent was : "Porte d'Herenthals de Berchem. Voyez vous distinctement signaux Bruce, répétez la dépêche par téléphone. (Signé) Général Wauwermans." This was distinctly read and telephoned back. Also the second, sent by the Minister of War : "Envoyez un bataillon au fort I. (Signé) Ministre Guerre Pontus." A third telegram sent to the Caserne of Telegraphists was equally successful. There were six lights in the balloon giving about twenty candle-power each. The telephonic stations of Rehls were comparatively near, being only at a distance of from 4 to 5 kilometres ; the object that night being to test at once the distinctness of the signals, by placing the observing-stations on the existing telephonic circuits ; but the night was an ideal one for signalling, and it is understood that the balloon was seen to an enormous distance. A company was also on the look-out at the top of the tower of Notre Dame, at Antwerp (4 kilometres), and they distinctly read all the messages sent.

CONSIDERABLE uncertainty has, up to the present time, existed as to the number and composition of the compounds of gold with sulphur. For years it was supposed that there were three sulphides of gold—Au_2S, Au_2S_2, and Au_2S_3 ; but Schrötter and Pruvoznik, in 1874, came to the conclusion that no sulphides of gold were to be obtained in a pure state, thus leaving the subject in greater darkness than ever. Happily, however, this deplorable uncertainty has at length been completely dispelled by Drs. Hoffmann and Krüss, of Munich, who have abandoned the methods of Berzelius, Levol, and Schrötter and Pruvoznik, for more fruitful ones of their own. The lowest sulphide of gold, Au_2S, was obtained by the addition of hydrochloric acid to a solution of the double cyanide of gold and potassium saturated with sulphuretted hydrogen. The last traces of admixed sulphur were removed by washing with sulphuretted hydrogen solution, alcohol, ether, carbon disulphide, and finally again with ether. After drying over phosphoric oxide, pure Au_2S was obtained as a dark-brown powder, yielding theoretical numbers on analysis. When freshly precipitated it is remarkably soluble in water, indicating a close relationship to the metals of the alkalies, whose sulphides are also soluble in water, and thus asserting its position in the first vertical series of the periodic system. With polysulphides of the alkalies it forms green sulpho-salts. It decomposes at 240°, leaving a residue of pure gold, and, if warmed in a stream of oxygen, takes fire, forming SO_2, and again leaving its gold in the metallic state. In a second communication in the current number of the *Berichte*, Hoffmann and Krüss describe how they have succeeded in preparing Au_2S_2. A cold neutral solution of gold chloride was precipitated by sulphuretted hydrogen until the supernatant liquid became colourless. Admixed sulphur was removed from the precipitate in a manner similar to that employed in case of Au_2S, and finally pure Au_2S_2 isolated as a deep black substance, decomposed by heat similarly to Au_2S. It is distinguished from the latter sulphide by being decomposed by caustic potash with formation of potassium oxy- and sulpho-salts and separation of a little metallic gold. Au_2S_3 of Berzelius was found not to

exist, being merely a mixture of Au_2S_2 and sulphur, for the former substance was completely extracted by a solution of potassium cyanide, leaving an emulsion of finely-divided sulphur.

THE November Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, is the first of a series of papers in which information will be given as to the capabilities of our colonies to grow and export fruit. The authorities of Kew have little doubt that, if proper arrangements were made for packing and shipping, large quantities of fruit might be exported from Cape Colony, Natal, the Australian colonies, and New Zealand. It is thought that much of this, arriving in England during the winter and early spring months, would be readily bought to supply the wants of the community, and that the prices paid for such fruit as an article of luxury would be sufficiently high to cover the cost of bringing it from the southern hemisphere. Much interest was taken in the fruit shown from all parts of the Empire at the late Colonial and Indian Exhibition. An effort, therefore, has been made at Kew to collect information on the subject, and excellent service, no doubt, will be done by the publication of the facts which have been brought together. In the present Bulletin a full account is given of Canadian fruits.

THE fifth part (just issued) of the Transactions of the Leicester Literary and Philosophical Society contains an interesting paper, by Mr. F. T. Mott, on foreign fruits available for acclimatization in England. Among the plants to which he calls attention is the *Zizyphus vulgaris*, which produces a yellow fruit of pleasant flavour, the size of a small gooseberry. These fruits are usually dried and sold under the name of jujubes, the gelatine jujubes of our shops being named after them. "It is probable," says Mr. Mott, "that no species of *Zizyphus* in its present condition would ripen its fruit in English gardens, but the art of cultivation consists in so modifying the natural habits of plants as to adapt them to man's needs in various climates. This is accomplished by selection, propagation by seed, changes of soil, and gradual exposure. The first step would probably be to obtain a hardy variety of the *Zizyphus vulgaris* regardless of the quality of the fruit. A tree should be selected in the most elevated and exposed situation in which it naturally ripens its fruit. Seeds from this tree should be grown in a slightly colder climate, and if any of them can be got to ripen fruit, the seeds of these should be again reared still further north. In this manner the tree might gradually be acclimatized in our southern counties. Having once obtained a sufficiently hardy stock, the next process would be to improve the fruit. This would be done by selection of seed with reference to the fruit rather than to the hardiness of the plant, by crossing with Indian or Chinese species, and by careful study of soil and general treatment. The process of thus producing a new hardy fruit would be tedious, because fruiting trees can scarcely be brought to such a state of maturity as to show their true characters in less than eight or ten years from the sowing of the seed, and five or six generations at least might be required to produce any useful result. But the experiment would be interesting in all its stages, and the object if ultimately attained would be of great value."

WE have received the General Report on the operations of the Survey of India Department, administered under the Government of India, during 1885–86. The Report has been prepared under the direction of Colonel H. R. Thuillier, R.E., officiating Surveyor-General of India. It is divided into three parts. Part I. is introductory; Part II. contains a summary of the operations of the trigonometrical, topographical, and revenue survey parties; in Part III. there is an account of the operations of the several head-quarter offices. Extracts from narrative reports are presented in an appendix. Among the "general remarks" in Part I. there is a paragraph in which some dissatisfaction is expressed with existing arrangements. "The large demands," says the writer, "that have been made on the Survey Department for officers required to accompany political missions and military expeditions, and for other special work, combined with the circumstance of a larger percentage than usual being absent on medical leave, has rendered the efficient prosecution of the regular work of the Department peculiarly difficult. This has been the subject of remark in the Annual Reports for the past two years, and during the year under review the paucity of officers has been still more seriously felt. There has been absolutely no reserve of trained officers, and the administration of the Department has consequently been a task of considerable anxiety. It is necessary to record that the working machinery of the Department has been limited to a dangerous extent."

IT is stated that the Government of the Straits Settlements are about to undertake a systematic survey, on the Indian plan, of their territory and of that of the neighbouring "protected" States. Colonel Burrow, of the Indian Survey Department, was recently appointed to advise the Colonial Government on the subject, with the result here stated.

THE Asiatic Society of Japan has, we are glad to observe, published a General Index to its Transactions. There are now thirty-six parts, or fifteen volumes, of the latter, and as almost every foreign scholar in Japan has been a contributor to the Society's Proceedings for fourteen or fifteen years past, it was necessary that an index should be published. About two years ago we noticed the publication of an index to the Proceedings of the Society's friendly rival, the German Society.

MR. HENRY SEEBOHM is about to issue a work on the Geographical Distribution of the *Charadriidæ* (Plovers, Sandpipers, and Snipes, &c.). The unrivalled collection of Wading Birds in Mr. Seebohm's possession supplies the material for this work, and the volume will undoubtedly be one of great interest to ornithologists. Mr. Seebohm's ideas on nomenclature, the influence of the Glacial epoch on the migration of birds, and other kindred subjects, are always original, and this new work of his will open, according to the prospectus, with an introduction setting forth his latest opinions. There is also to be given "a complete synonymy from 1776 to the present time," a rather appalling announcement, and one involving a vast change in ornithological nomenclature, as it will preclude the use of Linnean names.

A TRANSLATION, by Miss Margaret K. Smith, of Seidel's "Industrial Instruction" is about to be issued in America by Messrs. D. C. Heath and Co. The author presents an exposition of "the principles underlying the claims of hand labour to a place on the school programme."

"THE Shell-Collector's Hand-book for the Field," by Dr. J. W. Williams, the editor of *The Naturalist's Monthly*, will be published immediately by Messrs. Roper and Drowley. It will give full directions as to the collecting and preserving of British land and fresh-water shells, and will describe the habitat of each. Every genus, species, and variety known to the Conchological Society up to date of publication will be noted.

MR. T. A. WALKER'S "History of the Making of the Severn Tunnel" is, we understand, likely to appear about Christmas. In addition to portraits on steel of some of the more prominent engineers concerned in the enterprise, there will be numerous plans and sections showing the gradual progress of the work, and diagrams of the large pumping-engines, &c. Messrs. Bentley and Son will be the publishers.

SIR JAMES PAGET'S address to the medical students at Owens College, delivered at the opening of the session 1887–88, has been published. The subject is, the utility of scientific work in the practice of medicine and surgery.

THE new number of the Journal of the Anthropological Institute contains a striking paper by Dr. George Harley, in which he attempts to show that the tendency of civilization is decidedly to lower the bodily recuperative powers of human beings. Another interesting paper—by Mr. G. L. Gomme—is on the evidence for Mr. McLennan's theory of the primitive human horde.

MESSRS. S. WIGG AND SON, Adelaide, are issuing a work on "Common Native Insects of South Australia," by Mr. J. G. O. Tepper. It is intended to serve as a popular guide to South Australian entomology. Part I. relates to Coleoptera.

A PAPER containing a list of the mammals of Manitoba, by Mr. Ernest E. Thompson, has been reprinted from the Transactions of the Manitoba Scientific and Historical Society. It consists chiefly of the author's field notes.

A CORRESPONDENT writes from St. Petersburg that tigers have been encountered this autumn in parts of Asiatic and European Russia where they have never been found before. Some time ago one of these beasts was captured near Wladiwostock, in Siberia, and another in the government of the Caucasus, close to the Black Sea. Both animals have been conveyed to St. Petersburg alive.

A NORWEGIAN astronomer has collected seventeen reports from various parts of Norway respecting the great meteor seen in that country on the evening of September 18, no doubt the largest meteor seen in Norway in recent times. These reports show that the meteor was seen as far north as Hamar, in Central Norway, and as far south as the towns of Fredrikshald and Skien, on opposite sides of the Christiania Fjord, the capital and neighbourhood appearing to be in the centre of its track. Its light was everywhere magnificent, having the appearance of a sudden blaze of electric light. The reports from Drammen and neighbourhood, as well as those from the province of Smaalenene, on the opposite side of the Christiania Fjord, maintain that the bursting of the meteor, which took place within this area, was accompanied by a loud report; but the astronomer in question is of opinion that this belief is due to some freak of the imagination, as the track of the meteor, covering such a vast area of land, must have lain too high in the atmosphere for any sound to be heard. He calculates from the reports to hand that the bursting of the meteor occurred at an altitude of about 6000 feet, and he thinks that even this figure may be safely doubled, as no doubt the meteor was seen far north and south of the places whence reports have been received.

THE Aino idea of an eclipse is described by the well-known student of Aino language and manners, Mr. Bachelor, in a recent number of the *Japan Weekly Mail.* Mr. Bachelor specially observed the conduct of the Ainos during the recent eclipse. The Aino, he says, is a very matter-of-fact person, and is not usually carried away by the imagination. On being shown the eclipse through a smoked glass, the Aino cried out that the sun was fainting away and dying. A silence then ensued, and from time to time an exclamation of surprise or fear was to be heard; it was evident the fear was that the sun would die away and never revive. They brought water and sprinkled it upwards towards the sun (as they would do if a human being were expiring), crying at the same time, "O god, we revive thee! O god, we revive thee!" Some squirted the water upwards with their mouths, others threw it with their hands, others, again, used the common besom or willow-branches, the latter being supposed to be specially efficacious. A few, especially women and girls, sat down with their heads hidden between their knees, as if silently expecting some dreadful calamity to suddenly befall them. They have no theories with regard to eclipses, but their traditions run like this:—"When my father was a child, he heard his old grandfather say that his grandfather saw a total eclipse of the sun. The earth became quite dark, and shadows could not be seen; the birds went to roost, and the dogs began to howl. The black, dead sun shot out tongues of fire and lightning from its sides, and the stars shone brightly. Then the sun began to return to life, and the faces of the people wore an aspect of death; and, as the sun gradually came to life, then men began to live again."

THE first meeting of the one hundred and thirty-fourth session of the Society of Arts will be held on Wednesday, November 16, when the opening address will be delivered by Sir Douglas Galton, Chairman of the Council. Previous to Christmas there will be four ordinary meetings, in addition to the opening meeting. For these meetings the following arrangements have been made:—November 23, Prof. Silvanus P. Thompson, "The Mercurial Air-pump;" November 30, Mr. J. B. Hannay, "Economical Illumination from Waste Oils;" December 7, Mr. P. L. Simmonds, "The Chemistry, Commerce, and Uses of Eggs of Various Kinds;" December 14, Sir Philip Magnus, "Commercial Education." During the session there will be six courses of Cantor Lectures—"The Elements of Architectural Design," by Mr. H. H. Statham; "Yeast, its Morphology and Culture," by Mr. A. Gordon Salamon; "The Modern Microscope" (being a continuation of the recent course of Cantor Lectures on the "Microscope"), by Mr. John Mayall, Jun.; "Alloys," by Prof. W. Chandler Roberts-Austen, F.R.S.; "Milk Supply and Butter and Cheese Making," by Mr. Richard Bannister; "The Decoration and Illustration of Books," by Mr. Walter Crane. Two juvenile lectures on "The Application of Electricity to Lighting and Working," by Mr. William Henry Preece, F.R.S., will be given during the Christmas holidays.

THE additions to the Zoological Society's Gardens during the past week include a Campbell's Monkey (*Cercopithecus campbelli*) from West Africa, presented by Mr. E. B. Mitford; a Weeper Capuchin (*Cebus capucinus*) from Brazil, presented by Mr. C. N. Skeffington; a Raccoon-like Dog (*Canis procynides*) from China, presented by Mr. W. T. Manger; an Indian Antelope (*Antilope cervicapra*) from India, presented by Mrs. M. V. Charrington; a Leopard (*Felis pardus*) from Ceylon, presented by the Dissawe of Tamankadua Dulewa Adijur; a Common Squirrel (*Sciurus vulgaris*), British, presented by Mr. A. Townsend; a Naked-footed Owlet (*Athene noctua*), European, presented by Mr. R. E. Holding; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. G. E. Frodsham; two Larger Hill-Mynahs (*Gracula intermedia*) from Northern India, presented respectively by Mr. J. M. Cook and Mrs. J. S. Beale; a Gray-headed Porphyrio (*Porphyrio poliocephalus*) from India, presented by Lady Morshed; a West African Python (*Python sebæ*) from West Africa, a Common Boa (*Boa constrictor*) from South America, two Testaceous Snakes (*Ptyas testacea*), an Alleghany Snake (*Coluber alleghaniensis*) from North America, deposited; six Mocassin Snakes (*Tripidonotus fasciatus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE VARIABLE STAR U OPHIUCHI.—Mr. S. C. Chandler, Jun., who first determined the true period of this star, of all variables the one with shortest period and most rapid fluctuations of light, has brought together, in No. 161 of *Gould's Astronomical Journal*, all the observations of magnitude of this star available. Of these, one made by Schjellerup on June 7, 1863, is of special value, as it was evidently made near the time of minimum, and at an interval of nearly 8000 periods from the principal epoch; whilst a series made at Cordoba in 1871 and 1872 has proved of very high importance. The discussion of these various observations show that it is exceedingly probable that the period has undergone a slight shortening; all the data being well reconciled by the assumption that each period is

shorter than the preceding one by 0·0004s. The corrected elements of the star will therefore be as follows:—

1884 January 1, 0h. 54m. 43·6s. Paris M.T. + 20h. 7m. 41·6s. (E – 1070) – 0·0002s. E.

THE NEW ALGOL-VARIABLE, Y CYGNI.—In the same number of *Gould's Astronomical Journal* Mr. Sawyer states that he has obtained observations of this star which render it probable that the true period is 1d. 12h. ±, or half the period which Mr. Chandler had adopted for it (see NATURE, vol. xxxvi. p. 377).

OLBERS' COMET, 1887.—The following ephemeris is in continuation of that given in NATURE, vol. xxxvi. p. 588:—

Ephemeris for Berlin Midnight.

1887.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Nov. 11	14 24 15	13 59·0 N.	0·1152	0·3037	1·20
13	14 31 8	13 15·0			
15	14 37 51	12 31·6	0·1232	0·3098	1·12
17	14 44 25	11 48·9			
19	14 50 51	11 6·7	0·1317	0·3162	1·05
21	14 57 9	10 25·3			
23	15 3 18	9 44·7	0·1406	0·3226	0·98
25	15 9 20	9 50·0			
27	15 15 12	8 26·0 N.	0·1499	0·3291	0·91

The brightness on August 27 is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 NOVEMBER 13–19.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 13

Sun rises, 7h. 16m.; souths, 11h. 44m. 23·6s.; sets, 16h. 12m.: right asc. on meridian, 15h. 13·5m.; decl. 17° 58′ S. Sidereal Time at Sunset, 19h. 42m.

Moon (New on November 15, 8h.) rises, 4h. 13m.; souths, 10h. 6m.; sets, 15h. 46m.: right asc. on meridian, 13h. 34·4m.; decl. 4° 48′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	8 16	12 23	16 30	15 52·4	21 13 S.
Venus.....	2 56	8 52	14 48	12 20·9	1 39 S.
Mars	1 12	7 48	14 24	11 16·3	6 30 N.
Jupiter....	6 52	11 31	16 10	15 0·5	16 10 S.
Saturn....	21 21*	5 8	12 55	8 36·0	18 59 N.
Uranus ...	3 52	9 28	15 4	12 56·8	5 23 S.
Neptune..	16 40*	0 21	8 2	3 48·3	18 13 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich).

Nov.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. °	°
18	33 Sagittarii	5	16 23	16 50	38	1
18	ξ^2 Sagittarii	4	18 2	19 5	103	330

Nov.	h.	
17	19	Mercury in inferior conjunction with the Sun.
18	7	Saturn stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·3	81 16 N.	Nov. 17,	2 8 *m*
Algol	3 0·8	40 31 N.	,, 16,	1 3 *m*
			,, 18,	21 52 *m*
S Cancri	8 37·5	19 26 N.	,, 14,	1 26 *m*
R Virginis	12 32·8	7 37 N.	,, 14,	*M*
U Ophiuchi	17 10·8	1 20 N.	,, 13,	0 3 *m*
			and at intervals of	20 8
β Lyræ	18 45·9	33 14 N.	Nov. 15,	19 0 m_2
			,, 19,	0 0 *M*
R Lyræ	18 51·9	43 48 N.	,, 16,	*m*
δ Cephei	22 25·0	57 50 N.	,, 13,	21 0 *m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers.

	R.A.	Decl.	
From Lynx	125	40 N.	Swift; streaks.
Near κ Leonis	142	27 N.	Very swift.
Near θ Ursæ Majoris.	143	49 N.	Very swift.
The Leonids	149	22 N.	Swift; streaks.
Near ξ Ursæ Majoris.	166	32 N.	Swift; streaks.

GEOGRAPHICAL NOTES.

THE November number of the *Scottish Geographical Magazine* contains an admirable paper by Mr. John Murray, on "Some Recent Deep-sea Observations in the Indian Ocean." Mr. W. W. Blair, C.E., contributes a useful paper on the "Cold Lakes of New Zealand." Prof. Mohn sends a list of the highest peaks in Northern Europe, with their heights from the latest determinations. They are, with heights in feet:—Galdhoppigen, South Norway, 8399; Glitter Tind, 8379; Snehætten, 7566; Oræfajökull, 6427; Sulitelma, Northern Norway, 6178; Petermann's Spitze, East Greenland, 11,418; Beerenberg, Jan Mayen, 8350; Mount Misery, Bear Island, 1785; Hornsund Tind, Spitzbergen, 4560; Richthofen Mount, Franz Josef Land, 5184. Of these mountains two are volcanic, Oræfajökull and Beerenberg.

THE new number (9) of the *Mittheilungen* of the Vienna Geographical Society contains a summary of our knowledge of the physical geography of the East Asiatic waters (the Western Pacific and its offshoots)—currents, temperatures, &c.—by Lieut. Adolf Glockner.

IN the September number of the *Bulletin* of the American Geographical Society, Mr. R. E. Peary gives a detailed account of his journey, in the summer and autumn of last year, into the interior of Greenland. He entered in the neighbourhood of Disco Island, considerably further north than the starting-point chosen by Nordenskjöld for his expedition. Mr. Peary's experiences were somewhat similar to those of Nordenskjöld. His course throughout the journey was due east. He only reached 100 miles from the edge of the ice-blink or interior ice, his highest elevation being 7525 feet. Mr. Peary sums up his observations of the character of the interior ice. The coast-line shows a great diversity of features, dependent upon the altitude, the season, and the elevation and configuration of the adjacent mountains. Whenever the ice projects down a valley in a long tongue or stream, the edges contract and shrink away from the warmer rocks on each side, leaving a deep cañon between, usually occupied by a glacier; and the upper surfaces, disintegrated by the reflected heat from the mountains above, and shattered by the daily change of temperature more perhaps than by the forward flow, presents a chaotic labyrinth of crevasses, gullies, and rugged pinnacles, increasing in magnitude in direct proportion to the length of the tongue and its approach to the sea-level. As to the features of the interior beyond the coast-line, the surface of the "ice-blink" near the margin is a succession of rounded hummocks, steepest and highest on their landward sides, which are sometimes precipitous. Further in these hummocks merge into long flat swells, which in turn decrease in height towards the interior, until at last a flat gently rising plain is reached, which doubtless becomes ultimately level. In passing from the margin of the ice-blink to the remote interior, from one to five distinct zones may be noted, the number and width varying with the season, the latitude, and the elevation. In winter the entire surface is undoubtedly covered with a deep unbroken layer of fine dry snow. Late in the spring the warmth of the sun at midday softens the surface of the snow, along the land borders of the ice, and this freezes at night, forming a light crust. Gradually this crust extends up the interior, and with the advance of the season the snow along the border of the "ice-blink" becomes saturated with water. A little later the zone of slush follows the zone of crust into the interior, the snow along the border of the ice-blink melts entirely, forming pools in the depressions, and streams which cut deep gullies in the ice; water-cavities form; old crevasses open, and new ones appear. This zone rapidly widens, and extends into the interior in the footsteps of the others, and behind it the immediate border of the ice gets ragged and soiled; pebbles, boulders, and moraines crop out of its melting surface, and by the end of the Arctic summer it is disintegrated and shattered by the heat, and eroded by the streams, into impassable roughness. Mr. Peary

gives some useful hints as to the best modes of travel over the ice, which, if followed, he believes would without any difficulty take the explorer to the east coast.

IN Heft 3 of this year's *Deutsche Geographische Blätter* will be found the first part of a detailed study of the Schwarzwald by Prof. Platz, of Carlsruhe. It deals with the orography and geology.

THE Portuguese explorer, Josè Anchieta, is at present in the Quinsumbo region of the Portuguese West African territory, on his way to Bihé. He intends to investigate the flora of the region, which has never been adequately studied.

IN the Danish Budget for 1888–89 a sum of 68,000 kroner has been allotted for research in Icelandic waters. Several large fjords of great commercial importance are entirely unexplored, and are therefore full of danger to navigation. The fishery grounds around the various islands will also be investigated. This exploration will have great interest for science, as it is likely to accumulate much valuable information in oceanography, as well as zoology and meteorology. The work will be carried on freely from May to August, and it is hoped will be completed in five or six years.

THE Roman Catholic missionaries on Yule Island have been exploring the region of New Guinea opposite their station. They found that the Ethel and Helida are insignificant streams; but they discovered a new river, the St. Joseph, which rises at the foot of Mount Yule in 8° 15′ S. lat. and 146° 40′ E., and which flows in a southerly direction. The land on both sides is highly fertile and the natives peaceful. They visited fifteen villages, several with a population of over 2000.

IN a paper in the last-issued *Bulletin* (vol. ii. No. 6) of the Californian Academy of Sciences, Mr. George Davidson gives some interesting information on submarine valleys off the Pacific coast of the United States. He points out that within 40 or 50 miles of the coast to the south of Cape Mendocino the plateau of the Pacific reaches a depth of 2000 to 2400 fathoms. Generally there is a marginal plateau for 10 miles out to the 100-fathom curve, and then the descent is sharp to 500 or 600 fathoms. In this marginal plateau there has been discovered by the Coast Survey several remarkable submarine valleys. Notably that in Monterey Bay, heading to the low lands at the great bend of Salinas River; and that off Point Hueneme at the eastern entrance of the Santa Barbara Channel, and heading into the low coast at the wide opening of the Santa Clara Valley. Then there are one or two off the southern point of Carmel Bay, while the deepest one enters far into the bay. The latest discovered submarine valleys are near the high bold coast under Cape Mendocino. Just north of a submarine ridge extending from Point Delgada to Shelter Cove is a deep valley which breaks through the marginal plateau and runs sharply into the immediate coast-line under the culminating point of the crest-line of mountains. The head of this submarine valley is 100 fathoms deep at 1¼ mile from shore; when it breaks through the 100-fathom line of the marginal plateau it reaches a depth of 400 fathoms. The slopes of the valley are very steep. Midway between this and Point Garda there is another valley 300 to 150 fathoms deep. The opening of this valley through the outer edge of the 100-fathom plateau is 520 fathoms deep. Between Point Garda and Cape Mendocino is another valley, which, 6½ miles south-west by south from the cape, is 450 fathoms deep. This is a wide valley, the bottom of which is green mud, though in two places, at depths of 320 fathoms, broken shells were brought up with gravel.

BY the latest communication from Mr. Stanley's expedition it is evident that, unless some unexpected disaster has happened, he reached Emin Pasha some time in August. He found the Mabodi country, through which the Aruwimi flows, densely inhabited, while that river on the borders of the Mabodi country bends south, and again becomes navigable. This seems clearly to show that the Aruwimi can have no connection with the Wellé system.

THE last number of the *Izvestia* of the Russian Geographical Society (1887, 3rd fascicule) will be most welcome to geographers. It contains a preliminary map (70 miles to an inch) of the eastern parts of East Turkestan, Tsaidam, and the upper parts of the Yellow and Blue Rivers, embodying the results of the fourth journey of General Przewalski in Central Asia. The most interesting feature of the map is that it shows that the depression of the Lob-nor must not be confounded with the Eastern Gobi. This last is more elevated, and falls by a steep terrace towards the depression of the Lob nor, which has in the east of the lake a width of only 80 miles, and terminates at Lake Tchin-jen-he, where the desert reaches altitudes of 3700 and 4800 feet above the sea. The Tarim depression is thus well limited in the east, and the doubts which arose among geographers as to the possibility of embodying the Eastern Gobi and the Tarim depression under the same denomination of Hang-hai, as proposed by Richthofen, are thus settled. The well-known difference of characters of the two regions depends upon the differences of their orographical structures, and the Tarim region appears as a depression of the high plateau of East Asia, limited in the east as well as in the north, the west, and the south. Geographers will find on the map the series of chains named after Colombus, Marco Polo, Humboldt, and Ritter, discovered by General Przewalski; the high range to which the Russian Geographical Society gave the name of its Russian discoverer; the Burkhanbuda range; the lakes Jarin and Orin, 14,000 feet high, of the upper Hoang-ho; and all those minor features which, when mentioned in M. Przewalski's letters, excited so much interest among geographers. A list of sixteen places, the latitudes and partly the longitudes of which have been determined, and a list of ninety-five altitudes, accompany the map.

IN a short note accompanying the above map, General Przewalski mentions certain facts brought to light during the last three months of his journey. The Khotan-daria of East Turkestan does not make a bend towards the west, as shown on several recent maps. It flows due north through a sandy desert, and its course on Klaproth's and D'Anville's maps was more in accordance with reality than the indications on more modern maps. Its water reaches the Tarim only during the summer. A new oasis, Tavek-kel, grew up some fifty years ago on the Yurun-kash; its population numbers about 500 families. The lake Yashil-kul does not exist where it is shown on our maps. The most important statement is, however, the following. By the beginning of October 1885—that is, at low water—the Tarim had, at the confluence of the Yarkand and Khotan Rivers, a depth of 3 to 5 feet, and a width of about 185 yards. In the summer, according to information obtained from the natives, and confirmed by the state of the river-bed, the depth and width of the Tarim are thrice the above. Taking into consideration the fact that the lower Tarim, followed by M. Przewalski in 1876 and 1877, has throughout a depth of no less than 14 feet, it may be maintained, M. Przewalski writes, that the Tarim is navigable for steamers on its whole length from the above junction to the Lob-nor. It seems probable also that steamers may be able to ascend a short distance up the Aksu River and further up the Yarkand-daria.

THE same number of the *Izvestia* contains an elaborate paper by M. A. Eliséeff embodying the ethnological results of his journeys in Asia Minor since 1881. In this paper there are able descriptions of the various populations of Asia Minor—the Turks, the Armenians, the Kurds, the Kurmanjis, the Greeks, the Arabs, the Chaldæans, the Tsiganes, and the Jews. The numerous anthropological measurements and other observations which the author made during his journeys in the interior of the country will be published separately in full. Two papers, on the Manych and the steppes of Northern Caucasus, by D. Ivanoff, and on the vegetation and geology of the same, by W. Fausek, are valuable contributions towards a better knowledge of the nature of this interesting region.

METEOROLOGICAL NOTES.

Symons's Monthly Meteorological Magazine for October contains a fifth annual table of the climate of the British Empire, giving a summary of the daily observations at sixteen stations, distributed over the globe, for the year 1886. The extremes show some very interesting facts, from which we select the following:—Adelaide has the highest maximum temperature in the shade, viz. 112°·4; the highest temperature in the sun, 174°·5; the least rainfall, 14·42 inches; and the lowest humidity, 56 per cent. Winnipeg has the lowest shade temperature, −44°·6; the greatest annual range, 147°·6; and the lowest mean daily temperature, 33°·2. Colombo (Ceylon) has the highest mean daily temperature, 81°·0. Bombay has the greatest rainfall, 99·74 inches. London occupies the unenviable position of the dampest station, 80 per cent. The same magazine contains a discussion of the severe thunderstorm which visited London on

August 17. The greatest rainfall on this occasion was 2·08 inches at Wimbledon, and the least at Hackney, 0·27 inch. In connection with the climatology of the British Empire, it may not be generally known that the Annual Reports of the Army Medical Department contain meteorological summaries for a number of stations mostly in the northern hemisphere, *e.g.* the Mediterranean, Africa (including Egypt), and the East and West Indies. The last Report published is for the year 1885, and contains the results of observations and the extremes from nineteen stations.

IT is stated in the *Meteorologische Zeitschrift* for October that a new edition of Prof. H. Mohn's "Grundzüge der Meteorologie" has just been published by Reimer and Co., of Berlin. The fact that the work has reached a fourth edition in twelve years shows the favour with which it has been generally received. The plan remains the same as before, but both the text and the plates have been corrected to correspond to the recent progress of the science.

MR. H. ALLEN HAZEN has contributed an article to the *American Journal of Science* for October on the relation between wind-velocity and pressure, giving a summary of the better class of experiments, the methods employed, and the results arrived at, from those of Borda, in 1763, to the present time. The methods of investigation generally adopted are (1) carrying a plate either in a straight line or in a circle; and (2) allowing a current of air to impinge normally upon the plate. The results of Borda's observations are expressed in the formula—

$$p = (\cdot0031 + \cdot00035c)sv^2,$$

in which p = pressure in pounds; c = contour of plate in feet; s = surface in square feet; and v = velocity in miles per hour. In some careful experiments made at Washington in 1866, the formula obtained, viz.

$$p = (\cdot0032 + \cdot00034c)sv^2,$$

shows a remarkable and unexpected coincidence with Borda's results, with an entirely different apparatus. By far the most careful experiments with a whirling machine were those of Hagen, in 1873, with plates varying in size from 4 to 40 square inches in area. His formula is—

$$p = (\cdot0029 + \cdot00014c)sv^2;$$

and these results have been used by Prof. W. Ferrel in his recent discussion of this question. Various other experiments are discussed, including those lately made in France on a train running at increasing velocities, which give the formula—

$$p = \cdot000535sv^2.$$

The author expresses the opinion that further experiments are much needed, with larger plates than 2 feet square, and with high velocities with a straight-line motion. In connection with this subject it may be mentioned that the Royal Meteorological Society have appointed a Wind-Force Committee to consider the relation existing between velocity and pressure, together with other anemometrical questions, and a preliminary report was read in the spring of this year.

THE publications of the Swedish Meteorological Office are somewhat in arrear, the volume recently published being for the year 1882. It contains observations *in extenso* from eighteen stations of the second order, and monthly and yearly results of 117 stations, among which are seventy-nine for temperature only and several that have been established in the interest of forestry. The Central Office has no station of the first order, but publishes the observations of the Upsala Observatory, which is an independent institution. From this Observatory we have very complete observations from 1855 to 1886, in addition to very valuable works on the classification of clouds and the movements of cirrus clouds, by Dr. Hildebrandsson. The Central Office publishes, however, a monthly weather report, in the service of agriculture, which is brought out to date. The Swedish network of stations was established in 1856, by the Royal Academy of Sciences of Stockholm, and in 1873 the present Office was founded, with Dr. R. Rubenson as Director. The Office for Marine Meteorology, established in 1877, is also an independent institution; the logs used are those of the English Meteorological Office, with the addition of the headings in Swedish. By mutual agreement, Sweden deals specially with the Baltic, while Norway takes the North Sea, the data collected being exchanged by the respective countries.

THE Report of the Meteorological Service of the Dominion of Canada for the year 1884, just issued by Mr. Carpmael, shows satisfactory progress in the various departments. Several new stations have been added, and the number for which monthly and yearly averages are given amounts to 136. Eighty-three per cent. of the storm warnings issued during the year have been verified; weather predictions have also been disseminated throughout portions of the country by means of large disks attached to the railway cars. These disks have the image of the sun, representing fine weather, the crescent moon, for showery weather, and a star, for wet weather, painted on them, in addition to words. The percentage of verification of these predictions is also very satisfactory. The climatological tables show that the highest mean annual temperature was 47°·81 at Windsor (Ontario), and the lowest at Fort Chipewan (North-West Territory), 26°·65. The records for Hudson's Bay Territory are not complete, but would probably have shown a lower mean. The maximum shade temperature was 100° at Chaplin (North-West Territory) in June, and the lowest at St. Andrews (Manitoba), – 53°·3, in January; with one slight exception this station had also the largest mean daily range, viz. 24°·75. Sunshine-recorders are erected in five provinces only; in these Winnipeg has the maximum sunshine, 45 per cent., and Pembroke (Ontario) the least, 30 per cent., of the possible amount. The greatest mean rainfall in any whole province was 48·46 inches in Newfoundland, and the least, 9·90 inches, in North-West Territory on 48·6 days. The greatest average of rainy days was 151·5 in Prince Edward's Island. The distribution of rainfall in Ontario is also represented by maps for each quarter and for the year. With a view to enhancing the value of the tables, we suggest the desirability of arranging them according to the international scheme, instead of in the present form; or at least of printing the extreme values in thick type, as is usually done in other countries.

THE chief feature of the United States *Monthly Weather Review* for July last is the unusually high mean temperature over the central and northern parts of the country; in some portions it averaged from 4° to 7° above the normal values, and was the warmest that has occurred since the establishment of the Signal Service stations. This fact is interesting in connection with the weather experienced in some parts of this country, where there was an excess of 2° to 5° in all districts. Descriptions of the storms which occurred over the North Atlantic are given; the average number of areas of low pressure for July during the last fourteen years is nine, for July 1887 the paths of seven such areas are traced, being two less than the average. The storm of the 26th is the one in which the high wave struck the s.s. *Umbria* (see NATURE, vol. xxxvi. p. 508). This depression was first charted in N. 55°, W. 25°, on the 25th, and its presence was indicated northwards of the British Isles during the 27th and 28th. The *Review* also contains a discussion of the North Atlantic storms during 1885; of sixty storms which advanced over the ocean from the American continent, twenty-eight were traced to European waters. Fifty-nine storms first appeared over the ocean, of which about 65 per cent. were traced to the west coast of Europe. A table is given showing the positions of centres of areas of mean high and low barometer for each month, and explains why in March and October the storm areas moved northward before reaching European waters, and that in August the depressions did not move eastward owing to unusually high pressure along the middle latitudes. Attention is drawn to the fact that, as a rule, the storms which do traverse the ocean leave the coast north of the fortieth parallel; only a very small number of the storms which advance from southern latitudes cross to the northward of the trans-Atlantic ship routes.

A SERIES of very interesting articles, from the pen of Dr. Oscar Doering, on the inter-diurnal variability of temperature at places in the Argentine Republic and South America generally, are being published in the *Boletin de la Academia Nacional de Ciencias* of Córdoba. Investigations of this kind have been very seldom undertaken, although Dr. Hann and Dr. Supan have pointed out that the variability of temperature is a factor of eminent importance, affecting the habits and character of mankind, and also partially the distribution of plants. Dr. Hann, in his elaborate paper upon this subject presented to the Vienna Academy on April 15, 1875, and based upon such observations as were then available, defines the variability of temperature as the differences of temperature of two immediately succeeding intervals of time which do not belong to the daily and yearly

period; or, in other words, as the differences of temperature between two short intervals that lie within the daily or yearly period, *minus* the amount of the periodical (or normal) variation. In part 4, vol. ix., of the above-mentioned Bulletin, Dr. Doering has calculated the variability for Concordia (lat. 31° 25′ S., long. 58° 4′ W.), but for three years only. The month of October has the maximum value, 4°·6, and April the minimum, 2°·8. The variability during spring is greatest, viz. 3°·9, and least during autumn, viz. 3°·0, and the mean for the year is 3°·6, or about 0°·4 above that for Buenos Ayres. The hourly observations published by the Meteorological Council, with the daily means ready calculated, afford excellent materials for similar investigations. The preceding number of the Bulletin contains the meteorological observations made at Córdoba during the year 1885. The absolute maximum shade temperature was 100°·9 in December, and the minimum 14°·9 in June, giving an annual range of 86°·0. The maximum solar temperature was 147°·4, in February. The mean relative humidity ranged between 81·7 per cent. in March and 61·1 per cent. in August. The rainfall amounted to 24·26 inches; the wettest month was March, 5·96 inches, and the driest, May, 0·04 inch. Rain fell on 71 days, and snow on one day. The times of rain at the moment of observation, an element much recommended by Dr. Köppen, are also quoted.

THE WORK OF THE INTERNATIONAL CONGRESS OF GEOLOGISTS.[1]

II.

MY only remaining subject is the representation of terranes on maps by means of colours. At present no two organizations and scarcely two individuals use colours in the same way; and it is probably true that every organization and individual publishing many geologic maps has at different times employed the same colour for different terranes, and different colours for the same terrane. It results that the map user can gain no information from the distribution of colours until he has studied the legend; before he can read a new atlas he must learn a new alphabet. The advantage to be gained by substituting a universal language for this confusion of tongues is manifest and great, and has justified the application of much time and attention by the Congress and its Committees. By a series of resolutions a partial scheme has been selected, one colour at a time, and the completion of the plan has been left to the Committee on the Map of Europe. That Committee has prepared a colour legend which is accessible to American geologists in the volume of information published by the American Committee. It is understood in a general way that the Congress reserves final action, and the published legend not only belongs specifically to the map of Europe, but is provisional; still, as this map, if generally approved, will unquestionably be declared by the Congress an authoritative pattern for the guidance of map makers, the plan should be freely criticized at its present stage. The selection of uniform colours is a far more delicate and important matter than the arrangement of taxonomic terms; for while ill-chosen words may quickly fit themselves to new uses, the adoption of an ill-arranged colour scheme must entail continual loss.

In my judgment the scheme provisionally chosen is defective in several particulars, to which I shall presently call attention; but it is necessary to introduce the discussion by a statement of the conditions to be satisfied by a standard colour scheme and a statement of the practical means available. The following are the principal conditions, arranged in an order embodying my estimate of their relative importance:—

(1) The map must be clearly and easily legible. Each colour must be so distinct from each other colour that it can be identified, whatever its surroundings; and all other conventions must be readily discriminated.

(2) The cartographic scheme must be adjustable to the geologic facts; it must not require that the facts be adjusted to it.

(3) The same scheme should serve both for general maps—as, for example, those representing only systems—and for detail maps, representing numerous smaller divisions.

(4) Undue expense should be avoided. The amount and consequent utility of colour cartography is largely limited by its cost.

(5) It should be easily fixed and retained in the mind. This is best accomplished by making it orderly.

(6) Other considerations permitting, the map should please the eye. Since the arrangement of colour areas cannot be foretold, this can only be accomplished by admitting a certain range of choice. If allowed sufficient latitude in the selection of tones, an expert colourist can ameliorate an offensive combination of hues.

(7) Other considerations permitting, the establishment of a universal system should involve the least possible inconvenience. But as the inconvenience of change is temporary, while the inconvenience of a bad system is lasting, this consideration should yield to every other.

The art of mapping geologic terranes by means of colour is well developed, and its methods, viewed from the geologist's stand-point, admit of easy characterization. Colour may be varied in two distinct ways—in hue and in tone. Hues differ in quality, as yellowish-green and bluish-green. Tones differ in strength, as pale green and dark green. A colour is printed either solid or broken; it is said to be broken when applied in a pattern, as in lines or dots, or when it is interrupted by a pattern. The difference between solid and broken colours is a difference of texture. The primary discriminations in mapping are through hue, tone, and texture.

The map engraver produces texture in three ways. In the first way a single impression is made with the broken colour. The white of the paper, displayed where the colour is interrupted, combines with the colour in the general effect, producing a paler tone of the same hue. In the second way two impressions are made, one with solid colour, the other with broken, and the two impressions have the same hue; they may or may not differ in tone. This is monochromatic over-printing, and its general effect agrees in hue with the single impression, but differs in tone, being darker. In the third way two impressions are made, one solid, one broken, and their colours differ in hue. This is bichromatic over-printing, and its general effect differs in hue as well as tone from each of the colours combined in it. The first and second ways produce texture monochromatically, and do not yield a new hue; the third way produces texture bichromatically, and yields a new hue. It is practically impossible to obtain a texture effect without modifying the original tone.

The natural gradation from hue to hue is absolutely continuous, and the number of hues is infinite; the number of tones of each hue is likewise infinite. The number of hues and tones the eye can discriminate is finite, but very great; it is stated that 1000 hues have been distinguished in the solar spectrum. But the number of hues and tones that can be combined in a map is small. As a matter of perception, every colour is modified by the colours adjacent to it. The same hue affords different sensations when differently surrounded, and different hues may afford the same sensation. The same is true of tones; and there is a certain interdependence of hues and tones in this respect. In a geologic map each colour is liable to fall into various combinations, and two colours little differentiated occasion confusion. There is therefore a somewhat narrow limit to the employment of hues and tones. The matter has not been fully worked out, but it is probable that twenty is as large a number of hues as can safely be employed in connection with tones. Texture admits of very great variation. The various colour schemes submitted to the Congress and printed in the report of the Bologna meeting afford, with their manifest permutations, about 200 distinct textures, and I am satisfied from a study of these and others that as many as 100 can be chosen that are not subject to confusion. It follows that a map or atlas expressing few distinctions need use only hues, or only hues and tones, but where numerous distinctions are to be made, recourse must be had to textures.

The printing of a large number of textures of the same hue produces a greater number of tones than can be discriminated, and its effect is to confuse and nullify any distinctions (within the range of that hue) based purely on tone. The printing of a large number of bichromatic textures causes the same result, and it also produces a greater number of hues than can be discriminated. Its effect is to confuse and nullify distinctions based purely on tone, or on hue, or on tone and hue together.

In the colour scheme prepared for the map of Europe, thirty-eight distinctions are made. There are twenty-four hues, and

[1] Vice-Presidential Address read to Section E of the American Association for the Advancement of Science, August 10, 1887, by Mr. G. K. Gilbert. Continued from p. 22.

the remaining fourteen distinctions are accomplished by variations of tone. While it may be possible to select twenty-four hues available for indiscriminate combination, there can be no question that those provisionally printed by the Committee will fail to maintain their distinctness when variously combined upon a map. Under the influence of such chromatic environments as are sure to be encountered, the four yellow hues of the Tertiary cannot be discriminated, and the same difficulty will arise with the two hues of gray assigned to the Carboniferous, and with the hues of gray and brown assigned respectively to the Permian and the Devonian. Some of the tones likewise are not sufficiently distinguished. Two of the blues of the Jurassic, two of the browns of the Devonian, two of the rose tones of the Archæan, and the two violets of the Trias, are open to this criticism. A certain amount of adjustment can be made in the final selection of inks, and probably all the defects from tone can be thus remedied, but the confusion of hues is more difficult to eliminate, for the great number of the hues interferes with the separation of those that are too approximate. To strengthen one contrast is to weaken another.

In order to judge of the availability of the scheme for the production of detail maps, it is necessary to consider the resolutions of the Congress as well as the printed legend. A resolution provides that the subdivisions of a system shall be represented by shades of the colour adopted for the system, or by broken colour or other texture devices; and it is further provided that the shades, whether produced by solid colour or by texture, shall be so arranged that the darkest or strongest represent the lower divisions of the system. The resolution is in French, and the word I have translated shade (*nuance*) is one which applies popularly to either hue or tone, while in the scientific terminology of chromatics it applies to hue only. The Committee on the map has taken it in its popular sense, and has represented some subdivisions by hues, and others by tones; for example, Pliocene and Miocene are assigned two tones of the same hue, while Oligocene and Eocene have each a separate hue. The Upper Cretaceous and part of the Lower Cretaceous are assigned a green hue in two tones, while the Gault and the Wealden, classed as subdivisions of the Lower Cretaceous, have independent hues of green. Of the six reds assigned to volcanic rocks, two agree in hue and differ in tone, while the remainder have distinct hues. As the legend stands, both major and minor distinctions—that is to say, the discrimination of groups, the discrimination of systems, and the discrimination of divisions smaller than systems—are all accomplished by differences of hue; while the discrimination of minor divisions is accomplished indifferently by variation of hue and by variation of tone. The same means performs several functions, and the same function is performed by several means.

It is stating the same thing from another point of view to say that the Congress and its Committees have used the term colour in its popular rather than its scientific sense. Scientifically, a colour is a particular tone of a particular hue, and the number of colours is infinite. Popularly, a colour is an assemblage of contiguous hues and their tones, to which a name has been given. Each hue and tone within the range covered by the name is a shade of the colour. It is in this popular sense that the resolutions assign a colour to each system, and assign shades of the system-colour to the subdivisions of the system.

Now, if in the variation of a system-colour, by textures or otherwise, a single hue is adhered to, the system-colour remains distinct from other system-colours throughout all its modifications and their modifications; but if hues as well as tones are varied, the inevitable result is confusion, for some of the hues of one system-colour will approach too near to hues of other system-colours. With a multiplicity of minor distinctions the main distinction of system from system will be lost.

Another difficulty lies in the fact that the Quaternary and Devonian colours, while strongly contrasted in tone, are nearly identical in hue. This does not affect their use in a general map, but in a detail map the stronger tones of the Quaternary gray will approach too closely the paler tones of the Devonian brown.

These criticisms apply to those features of the scheme which affect its adoption for general and detail maps of European countries. There is one of equal or greater importance affecting its application in other continents. It is adjusted to the rock systems of Europe exclusively, and makes no provision whatever for the systems of other parts of the earth. The geologists of Wisconsin, for example, cannot use it without calling the Keweenawan either Cambrian or Archæan. If they were in doubt which division should hold it, but inclined a little one way or the other, they could express their qualified opinion in the notation provided by the Map Committee; but having attained an unqualified opinion that the terrane belongs to neither of these two categories, they find no means for expressing their conclusions. The scheme cannot be applied to the geology of India, of New Zealand, or of Australia, without misrepresentation. It is not universal but local, and this because it is founded on the fallacy of a world-wide unity of geologic systems.

So far as the geology of the world is concerned, it would be better to adopt no convention at all as regards map colours than to adopt one carrying with it and promulgating a vicious classification. Uniformity is not worth purchasing at the price of falsification. If the members of the Congress cannot agree upon a plan having the flexibility demanded by the geologic facts, it will be best to limit its action to the local problems involved in the map of Europe. I believe, however, that the necessary flexibility is attainable; and before proceeding to further criticism of the Committee's scheme, I will give the outlines of a plan which appears to me to combine the advantage of flexibility with a number of other desirable qualities.

The plan is founded on the universality of geologic time and the diversity of local geologic histories as expressed in rock systems. Geologic periods are arranged in linear order. Each one adjoins the next, and together they constitute continuous geologic time, which we may conceive as represented by a straight line. The stratigraphic systems of a country have likewise an order of succession, and their arrangement is linear. They are not always continuous one with another, but the history recorded by the systems and the breaks between them is continuous, and may be represented by a straight line, equal and parallel to that of geologic time. And so for each country. A colour scale which shall represent each and all of these parallel lines must be itself linear and continuous, and fortunately we have such a scale furnished us in the prismatic spectrum.

I propose, first, that the continuous prismatic spectrum be adopted as the standard universal scale for continuous geologic time. I propose, second, that the conventional time scale, based on the geologic history of Europe, be complemented by a colour scale, prismatic but discontinuous. I would assign to each period, not a certain portion or area of the spectrum, but a specific colour defined by its position in the spectrum. This colour scale will also apply to the geology of Europe. I propose, third, that the students of each geologic district shall assign to the stratigraphic systems of that district a set of prismatic colours so selected from the spectrum as to properly represent the relation of each system to the time scale, provided that relation is approximately known. Under this rule a system corresponding partly with the Cretaceous and partly with the Jurassic will receive a prismatic colour intermediate between those assigned to the Cretaceal and Jural divisions of the time scale. I propose, fourth, that systems whose relations to the standard time scale are not even approximately known be given tentative positions in the time scale and assigned the corresponding colours; and that such provisional colours be distinguished by a special device.

Of this device I will speak later, but before we leave this part of the subject the capability of the plan to express the facts should be more clearly characterized. Continuous geologic time being equated with the continuous spectral band of light, each period is theoretically equated with a segment of that band including all the hues between certain limits. But practically the period is represented in the colour scale only by the central hue of the segment, and there is nothing in the nature of this hue to indicate the length of the segment. Similarly each local system is represented only by the hue corresponding to the middle of the equivalent period, considered as a part of the continuous time scale, and this hue gives no information as to the magnitude of the system or the duration of the corresponding period. When a non-European system is represented on a map with the Devonal colour, all that is expressed is that the middle of its period coincides with the middle of the Devonal period; the whole period may equal the Devonal or may be shorter or may be longer. With this limitation the scheme is able to express the exact facts, or the exact state of opinion, in regard to correlation.

I propose, fifth, that the subdivisions of systems be represented, if their number is small, by distinct tones of the hue assigned to the system, and if their number is great, by monochromatic textures. It having been provided that systems shall be distinguished by means of hues, it is now provided that hues

shall have no other function. This secures the integrity of the distinction between systems, whatever the minuteness of subdivision.

The idea of using the spectral colours in their proper order is not novel. It has entered into half the plans submitted to the Congress, but each author has introduced other colours also, or else has undertaken to use the spectrum colours more than once, under the impression that they do not afford the necessary range or variety. This impression is based largely upon the popular meaning of the word colour. It is indeed true that if we limit ourselves to those parts of the spectral series which have univocal names, we have only six or seven distinctions; and it is further true that if we have recourse to binomial designations, such as yellowish green and greenish yellow, we obtain rather indefinite conceptions; but to men of science there are better resources than those afforded by the language of every-day life. The spectrum has been elaborately studied, and the relations of its dark lines to its colours have been determined. Its wave-lengths have, moreover, been measured, and by such means as these we are furnished with three different scales, any one of which is adequate to the precise definition of any hue of the continuous series. What needs to be done is this. When the divisions of the time scale have been decided on, the spectrum must be studied to ascertain the best selection of hues. Their number must, of course, be that of the number of divisions of the time scale, and they must be so chosen that the degree of separateness of adjacent colours shall be everywhere the same, as judged by the normal human eye. Then define each hue by its wave-length or its position in the Kirchhoff scale, and define it also in terms of the best combination of pigments with which it can approximately be reproduced for practical use. It is, of course, impossible to copy the prismatic colours with accuracy, because the colours of pigments are impure, but this difficulty will not seriously interfere with the employment of the prismatic colours as a standard.

The practical question whether the spectrum will give a sufficient number of hues so far separated from each other as to be distinguishable in all the arrangements occurring on maps has received such consideration as I have been able to give it, and it is my judgment that the maximum number of hues that can safely be used falls somewhere between fifteen and twenty. There will certainly be no difficulty in thus constructing a standard colour scale with about a dozen terms.

The employment of the spectral colours in this manner leaves three groups of colours unassigned—the purples, the browns, and the grays. If the spectral colours be arranged on the circumference of a circle so that each diameter of the circle connects hues that are complementary, it is found that they occupy the greater part, but not quite all, of the circumference, and the colour needed to fill the vacant arc is purple. The hues of purple might then, if deemed necessary, be added to one end or the other of the spectrum, thus increasing the range from which to select colours for the time scale.

My sixth proposition is to assign the browns to volcanic rocks. I would leave the grays unassigned.

It will be observed that no intimation has been given as to whether the violet end of the spectrum should apply to the newest system of strata or the oldest. It must of course be definitely assigned to one or the other, but the particular assignment is a matter of indifference.

The main features of the proposed prismatic scheme have now been set forth, and you are fairly entitled to exemption from the minor features, but there is one detail that can hardly be omitted. In one of the main propositions it was provided that some special device should distinguish colours assigned to uncorrelated systems, and I feel it incumbent to show that a suitable device can be found. Of a number that have occurred to me as about equally available, I will mention but a single one—the over-printing, in small dots, widely separated, of the complementary colour. The complementary colour is selected because it does not disturb the relation of the system-colour to the colours of adjacent systems. Bichromatic over-printing produces a hue intermediate between the two hues combined, but the hue mid-way between a system-colour and its complementary colour is white or gray, and if only a small amount of the complementary colour is added, the system-colour merely becomes paler or duller, when viewed from such a distance that the colours blend.

The prismatic colour scheme, having been constructed for the express purpose of securing a degree of flexibility that will fit it for universal use, need not be further compared in that regard with the scheme published by the European Map Committee. Enough has also been said to show that its superior perspicuity is claimed both for general and for detail maps. A few words will suffice to compare the two systems in other respects.

As regards the expense incurred in the production of general maps, neither has any notable advantage, and they are not yet sufficiently developed to permit a comparison as regards the cost of detail maps. Their capability for the production of pleasant colour effects can best be judged when maps have been actually made, but it may be said in a general way that the Committee's scheme will afford more strong contrasts between adjacent colour areas than the prismatic. The maps coloured by the former will be relatively lively, those coloured by the latter relatively quiet. It is provided by the Committee that the volcanic colours shall be not merely red but strong. On a general map volcanic areas cover comparatively small spaces, and strong reds thus disposed will ordinarily add brilliancy; but the detail map of a volcanic district, thus coloured, will be disquietingly suggestive of active eruption.

The alphabet of colours for the prismatic scale will be the more easily learned of the two, because it is orderly, and because its order is already familiar in the spectrum. The Committee's scheme, however, has some old-fashioned mnemonic features which the prismatic lacks. The green of the Cretaceous is connected with greensand, the red of volcanic rocks with fire, and the rose of the Archæan with feldspar; and the gray of the Carboniferous mildly suggests the blackness of coal.

In respect to facility of introduction the Committee's scheme, being essentially a compromise of existing colour scales, has the advantage that to most users it is not entirely novel. The prismatic scheme on the other hand has the advantage of being orderly. It scientifically differentiates the functions of hues and tones, and though each one of its colours may be different from what the individual geologist has previously employed for the indication of the same system, the order of the colours is already familiar to him in another way.

This closes my review of the various works undertaken by the Congress. Some of these have been favoured, others opposed, and reasons have been given. But there is a general consideration or criterion applicable to all, which has nearly escaped mention, although it is of pre-eminent importance. When a matter is proposed for regulation by the Congress, the first question which should be asked is whether it falls within the legitimate purview of a convention of geologists. It manifestly does not if it belongs to some other science rather than to geology, and objection has on this ground been made against the regulation by our Geologic Congress of the nomenclatures of palæontology and mineralogy. But not all geologic matters even are properly subject to settlement by convention. This is peculiarly the case with geologic facts. Science is distinguished from the earlier philosophies of mankind by the peculiarity that it establishes its fundamental data by observation. The old philosophies were founded largely upon assumptions, and it was not deemed illogical—perhaps it was not illogical—to appeal to the authority of an assemblage of experts for the establishment of fundamental assumptions. But for science it is not merely illogical, it is suicidal, to establish facts in any other way than by observation. No vote of the most august scientific body can possibly establish a fact, and no vote can have any weight against a good observation.

Now the entire science of geology, using the phrase in a strict sense, is constituted by the aggregation and arrangement of facts, and none of its results can be rendered more true or be more firmly established, or be prevented from yielding to contradictory facts, by conventional agreement. A classification, if it has any value whatever, is merely a generalized expression of the facts of observation, and is outside the domain of the voter. If it comprises all the essential facts, its sufficiency will eventually be recognized, whether its authority is individual or collective. If it does not comprise them, it will inevitably be superseded, by whatever authority it may have been instituted. For this reason I am opposed to the classification by the Congress of the sedimentary formations, and likewise to the classification of volcanic rocks, and I also regard it as ill-advised that the Congress undertook the preparation of a map of Europe, for that—if more than a work of compilation—is a work of classification.

If we examine the other undertakings of the Congress—the definition and gradation of taxonomic terms, the systematization of terminations, the selection of a scale of colours for geologic maps, and the selection of other conventional signs for the

graphic expression of geologic phenomena—we find that they all belong to the means of intercommunication of geologists. They affect only the verbal and graphic technical language of the science. Of the same nature is the arbitrary time scale whose preparation I favour—a conventional terminology for the facts of correlation. So we may say, in general, that the proper function of the Congress is the establishment of common means of expressing the facts of geology. It should not meddle with the facts themselves. It may regulate the art of the geologist, but it must not attempt to regulate his science. Its proper field of work lies in the determination of questions of technology; it is a trespasser if it undertakes the determination of questions of science. It may decree terms, but it must not decree opinions.

TECHNICAL EDUCATION.[1]

THE present century has witnessed a vast and almost incredible change in the great industries of the world, and in the progress of the arts and manufactures. The causes of this great change are various, though mutually dependent upon each other, such as the cessation of the great wars that had for so long ravaged the continent of Europe, which enabled many of the most vigorous minds to be turned to the arts of peace; the rapid growth of population, which rendered the wants of mankind more pressingly felt; and the more general spread of education, which caused the great discoveries that have enriched this period to be eagerly taken advantage of and adopted.

Among the many results which have ensued, is one which must be carefully studied, affecting as it does in a peculiar degree our own country at this time.

Since the latter half of the last century, when by the disappearance of forests in the iron-producing districts, resulting from the use of timber as fuel, maternal Necessity had brought forth an invention in the shape of the process of smelting iron ore with coal, progress in machinery and manufactures had steadily been made. The great natural advantages arising from the conjunction, not only of coal and iron in the same locality, but also their immediate proximity to the limestone required in iron-smelting operations, had greatly contributed to this advance, until this country, instead of importing four-fifths of the whole iron used from Sweden, as was the case in 1750, had become the greatest iron-producing country of the world. The invention of the steam-engine in conjunction with the power-loom and other important machines, greatly contributed to the growth of the factory system, the establishment of the cotton, linen, and woollen industries, and the rapid increase of manufactories in general. Owing to the insular position of Great Britain, and the prohibitive laws in force, until fifty years ago the nature of the machinery used in all these manufactures, as well as the technical knowledge and skill of the workman, was prevented from being carried abroad. Thus, as stated in the recent Report of the Commissioners on Technical Education:—

"When, less than half a century ago, Continental countries began to construct railways, and to erect modern mills and mechanical workshops, they found themselves face to face with a full-grown industrial organization in this country, which was almost a sealed book to those who could not obtain access to our factories."

This artificial state of things was not destined to last, for, on the one hand, these countries were keenly alive to the importance of possessing such manufactures, and were determined to obtain them at all costs; and, on the other, it was greatly to the immediate advantage of our manufacturers to sell freely in such a market as began to be opened to them. At the same time skilled artisans were easily found who were willing to accompany abroad machinery which had been constructed in this country, and thus to become the means of disseminating technical education of the most practical type amongst those who were quite as industrious and frequently better educated than the workmen at home.

The efforts of foreign nations to establish mills and workshops of their own did not cease here; for, recognizing the necessity of specially spreading technical knowledge by all possible means, technical schools, instituted and supported by the State, at which instruction could be obtained free, or at almost nominal cost, were established in numerous places all over the Continent. The larger number of these schools have been institutions at which the scientific principles underlying industrial and manufacturing operations, rather than the actual operations themselves, were taught, although there are also in lesser number special technical schools, such as the weaving schools of Chemnitz in Saxony, of Crefeld in Rhenish Prussia, of Basle in Switzerland. From these various schools, numbers of highly educated men have been sent out year by year, prepared, when becoming foremen, managers, or employers of labour, to take advantage of the latest discoveries and improvements in various branches of industry, and keenly alive to the fact that "knowledge is power."

Notwithstanding all this, an enormous increase of trade and prosperity was enjoyed by this country for many years, and notably was this the case after the first International Exhibition in Hyde Park, in 1851, which Exhibition revealed to visitors from all parts of the world much (some persons of the old school are to be found, who assert *too* much), concerning the perfection of our machinery and processes of manufacture which had been scarcely realized before, even by ourselves. This prosperity apparently reached a climax from ten to fifteen years ago, and, since then, trade has assumed a very different aspect. At first the change was felt in relation to countries whose resources were in some respects comparable with our own, and afterwards with others less favourably situated, and in place of supplying them with manufactured articles and machinery, they began to enter into competition, and in many cases successful competition, with this country, even in markets hitherto considered all our own. Indeed, a positive reflex action has actually occurred in some important branches of industry and foreign iron, machines, hardware, and textile goods are imported for home use. The result of this competition has been keenly felt, and the consequent struggle which has taken place in these times of peace has been, and now is, almost as determined and often as bitter as in an open war. That rather doubtful compliment once paid by a great general to the British soldier, that he never knew when he was beaten, could scarcely be applied to the British manufacturer, since there is a very speedy way of settling this point in a commercial transaction; but the question upon which knowledge has often been wanting and information sometimes too tardily sought, is rather as to the cause and its remedy. In some cases the cause is obviously due to the lower wages and longer hours for which foreign workmen will toil, and it may be mentioned, as pointing to what may be sometimes possible in this case, that in the neighbouring industry of wire-drawing at Warrington, which was threatened with extinction, the German competition was entirely met and overcome by the wire-workers voluntarily accepting a reduction in wages of 10 per cent., after four of their delegates had visited the Black Forest and ascertained for themselves full particulars as to the wire industry of that district.

But, on the other hand, there are branches of manufacture in which the state of foreign workmen and workwomen is so pitiable that no right-thinking person would desire to have increased trade in this country at such a price to our own people, though happily there is not much fear of this, since the movement is rather in the other direction. But the question of wages is only one of many causes, for it has been asserted by excellent authorities that it is not in those branches of industry in which foreign wages are lowest and hours longest that competition presses most heavily upon us. Thus, according to the recently published Consular Reports, we have still something to learn in several directions in the matter of finding out fresh markets and accommodating our productions to native wants, instead of trying to force goods of our own pattern and design where they are either not in accordance with native views and prejudices, or are unsuitable to the locality. Again, it is not only the Germans who stamp the words "best Sheffield steel" upon cast-iron axes and knife-blades: neither in the matter of shoddy-manufacturers can this country afford to throw stones at our foreign rivals.

It is not, however, the object of this address to enter into a discussion of the various causes of trade depression, and still less to presume to say how such an undesirable state of things may be met and overcome, but to consider a subject which has recently been very vigorously brought forward in connexion with this matter under the title of "Technical Education." No branch of education has of late attracted so much attention as this. It has formed not only the text of the Presidential Address of the British Association in 1885, and part of that at the recent

[1] Part of Inaugural Address of session of University College delivered at St. George's Hall, Liverpool, on October 1, 1887, by Prof. Hele Shaw, M.Inst.C.E., of the University College, Liverpool.

address at Manchester, but of innumerable other speeches, pamphlets, papers, and even books, one of the very earliest and most brilliant of which was a treatise from the pen of the late Mr. Scott Russell. It has been quite recently the subject of a special Government Bill, which was considered sufficiently urgent to be carried through almost to the last stage when other Bills were being dropped right and left, and then disappeared only with the full assurance of a revival in more vigorous form at an early period of next session; while only in last July there was formed "A National Association for the Promotion of Technical Education," which numbers as its President, Vice-Presidents, and Committee, many of the most able politicians, experienced men of business, and well-known men of science.

These facts are quite sufficient to show that there is now a very prevalent and wide-spread belief that the subject of technical education has become one of pressing national importance. There are, indeed, already not wanting persons who connect the subject with the terms "foreign competition" and "commercial depression," by a train of reasoning apparently somewhat as simple as the following:—

1st proposition.—Bad trade is the result of foreign competition.

2nd proposition.—Foreign competition derives its strength from superior foreign technical education.

Conclusion.—Therefore bad trade at home is due to superior technical education abroad.

This mode of reasoning is brief and conclusive enough to satisfy even the most superficial, it is easily portable, and has the advantage of admitting of illustration in certain special cases in which both propositions and conclusion are true, but it is nevertheless a striking example of the danger of arguing from the special to the general. Without, however, accepting such a sweeping generalization, it may be safely said that foreign countries have derived great benefit from their systematic encouraging of technical teaching, and we may proceed to consider briefly what progress we ourselves have made in this direction.

In the first place it may be well to ask what the term "technical education" really means? Most people have, no doubt, a general idea on the subject, but there are a great many who freely discuss the question, who would be woefully at a loss if asked for an exact definition; and if anyone doubts the truth of this, let him try the experiment on a few friends. The answer which will generally be given, with some hesitation, will probably have some not very distinct reference to instruction in the use of tools, backed by allusion to carpentering by way of illustration, or will, perhaps, be some mention of chemistry, or other branch of science, or, as a final resort, "something to meet the German competition." Now the fact is that the first of these may not be really technical instruction at all, but only manual training as part of a general education, as, for instance, is now given out of school hours in the working of wood to the boys at most of our Colleges, partly to keep them out of mischief and partly to train the hand and eye, but in which case there is not the slightest intention or idea that any of the boys shall actually become a carpenter. The last answer, however grotesque it may seem, is much nearer the truth, as it connects technical instruction with a special *object* in view. Now that this is really the idea of those who have thought most carefully over the subject is made clear by the terse and excellent statement of the aims of the Association for the Promotion of Technical Education, one of which is "to effect such reforms in our educational system as will develop in the best way the intelligence of those of all classes upon whom our industries depend," the Association itself being formed because of "the general expression of opinion throughout the country as to the necessity of a reform in our system of national education, with the object of giving it a more practical direction." Thus we may accept the following definition of a writer on the subject, that "by technical education is meant special instruction in some scientific, artistic, or mechanical process or handicraft as distinguished from purely literary instruction"; or that by another writer, who defines it as "special training for an industrial pursuit as distinguished from a general preparation for any calling hereafter to be chosen." Thus technical education will comprise a very wide range of subjects, not those merely taught with a view to manufacturing, mechanical, and artistic pursuits, but will comprise the instruction given in a medical school, in an agricultural college, and even commercial education, which last now forms a distinct feature of our own College, and the reform of which branch of education is one of the special objects of the Association above alluded to. It is therefore at first surprising to the uninitiated that we find the following definition in the recent Bill for Technical Education: "The expression technical instruction means instruction in the branches of science and art with respect to which grants are for the time being made by the Science and Art Department, or in any other subject which may for the time being be sanctioned by the Department." This definition is no doubt quite satisfactory to the authorities of the Department, although it savours strongly of the opinion attributed in a well-known series of rhymes to a certain eminent University don, who is made to assert—

> "I am the Master of this College,
> And what isn't taught here isn't knowledge."

And though this definition happens at present to exclude manual and workshop instruction, concerning which the mover of the Bill, Sir Wm. Hart Dyke, expatiated somewhat eloquently and at considerable length when moving the second reading of the Bill—but this is a trifling matter, as no doubt when the Science and Art Department has had time to go into the matter, and to study the subject, and has made arrangements for teaching and examining it, it will be "sanctioned" with the rest, and become technical instruction. It must, however, be recognized that the Science and Art Department is the most important institution in this country for the promotion and encouragement of technical education, and has done a work, especially in the direction of evening-class teaching to the artisan class, which must have proved of incalculable benefit, and it will be well to study the progress made in science instruction, as affording some index of our general progress in technical education. The following table gives the result of work during the last ten years, showing in three columns: (1) the amount of the grants given to teachers for successful candidates on the system of payment by examinational results; (2) the actual number of students under instruction in science classes fulfilling conditions which would enable a grant to be claimed; (3) the number of papers actually worked in different science subjects. The three columns are independent of each other in a certain sense, since a registered student may either take several papers, or may, on the other hand, possibly not come up for examination at all, or, coming up, may fail to secure a grant.

TABLE I.—*Results of Science and Art Department during the last Ten Years.*

Year.	Grants. (1)	Students under Instruction. (2)	Examination Papers actually Worked. (3)
	£		
1878	39,073	59,705	66,365
1879	41,036	56,752	70,248
1880	43,863	60,041	72,428
1881	47,231	61,180	75,735
1882	49,700	67,315	79,786
1883	50,967	71,164	83,387
1884	61,638	77,519	90,825
1885	69,113	81,491	101,275
1886	79,000	97,664	118,241
1887	88,000	103,362	131,896

The results are striking, but in order to reveal their significance more closely, the diagram, Fig. 1, is reproduced from a recent memorandum of expenditure and estimates of the Department, in which the height of the lines in each year from the base line gives the value of grant, number of students, or of worked paper.

The three curves represent at once to the eye the rapid progress which is being made. Indeed, the rate of increase is twice as great during the last two years as during previous years, and, so far from there being any want of appreciation of technical instruction, the results are such as might possibly cause the taxpayer some concern; on this point, however, the memorandum states:—"There is no reason to suppose that the expenditure will rise at the present rate; on the contrary, even without look-

ing at the increased rate of rise of the last few years, as a sudden augmentation due to special causes, it is obvious that as the limit is approached the rate of rise must rapidly diminish. This limit, as far as it can be arrived at by calculation from population, &c., probably about 200,000 persons under instruction in science—there were last session 110,000 under instruction. Continuing the curve for science as it may reasonably be expected to run, we should arrive at about 110,000 in 1896." In any case there is no real cause for alarm, because the standard of work required to secure a grant can always be raised, and, as a matter of fact, appears to be steadily rising year by year, and, after all, the sum of even £101,175, which is the estimated expenditure for the current year in aid of science instruction, is a remarkably small annual expense for the instruction of 103,362 students all over the country.

There has been for several years at work another central agency, which promotes technical instruction in the same manner as the Science and Art Department, viz. by payment upon the results of examination. This body is known as the City and Guilds Institute of London. These examinations carried on by this body were originally established in 1873 by the Society of Arts—the subject that year being cotton manufacture, steel, and carriage building, the number of candidates being respectively one, two, and three, making a grand total of six. The next year, gas manufacture and agriculture were added, and the total rose to thirty-six. Subjects continued to be added, and the

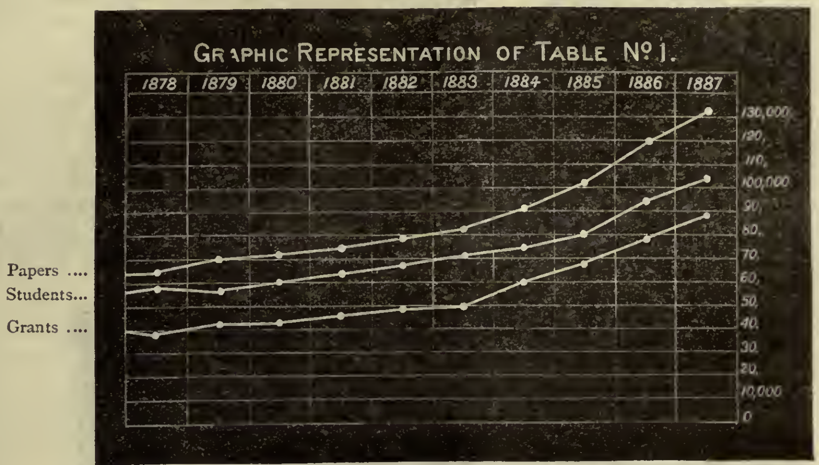


FIG. 1.—Science and Art Department.

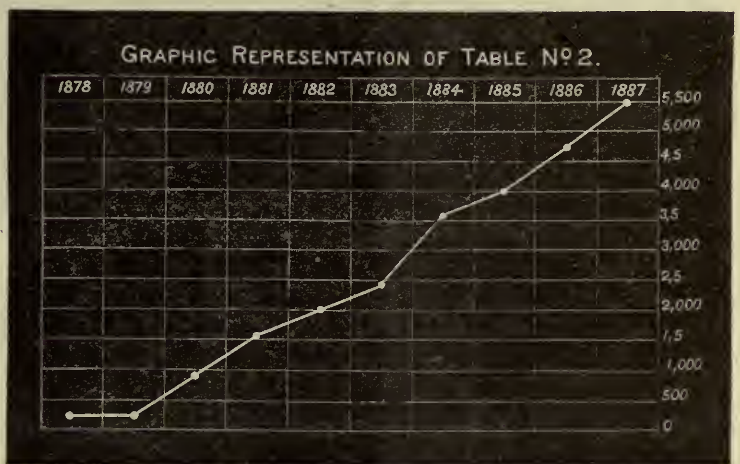


FIG. 2.—City and Guilds of London.

numbers to rise year by year, until ten years ago the latter had reached 184, since when the following table shows the progress made, the City and Guilds taking over the whole responsibility of the work in 1881.

TABLE II.—*Society of Arts and City and Guilds Examinations.*

Years.	1878	1879	1880	1881	1882	1883	1884	1885	1886	1887
Number of Candidates.	184	202	803	1563	1972	2397	3635	3968	4764	5508

I have plotted the above results in a similar manner to those of the Science and Art Department, and it will be seen (Fig. 2) that the rate of growth is far more rapid; and rich as are the worthy livery companies of grocers, fishmongers, tanners, spectacle makers, and others, who form the City and Guilds Institute, they too must have reason to confess that technical education, towards which they have recently contributed not less than a quarter of a million of money, is not quite at a standstill, for at the present rate of growth the number of candidates, large as it now is, will have doubled in the next seven years, though even this, with an assured income of £33,000 a year, may not give them cause for alarm. It may be well to explain that the examinational work of the City and Guilds, and that of the Science and Art Department, not only do not clash, but bear an important and valuable relation to each other. Thus the former is more distinctly technical, dealing with special details of trades

and manufactures, and the term "technological examination" is always employed to emphasize this point, and before a full technological certificate is granted by the City and Guilds Institute in any subject, certificates in the elementary stage of certain specified theoretical examinations of the Science and Art Department must be produced. A comparison of a few of the subjects will at once make this clear, the numbers appended to the subjects in the following list being those attached to them in the syllabus of each examining body.

SCIENCE AND ART DEPARTMENT.	CITY AND GUILDS.
Subject II.—Machine Construction and Drawing.	(27) Tools. (*a*) Wood Working. (*b*) Metal Working.
Subject III.—Building Construction.	(34) Carpentry and Joinery. (35) Brickwork and Masonry.
Subject VI.—Theoretical Mechanics. Subject VII.—Applied Mechanics.	(28) Mechanical Engineering.
Subject IX.—Magnetism and Electricity.	(22) Electrical Engineering. (*a*) Telegraphy. (*b*) Electric Lighting. (*c*) Electrical Instrument Making.
Subject X.—Inorganic Chemistry. Subject XI.—Organic Chemistry.	(1) Alkali and Allied Branches. (4) Coal Tar Products. (7) Oils, Varnishes. (8) Oils and Fats. (9) Gas Manufacture.
Subject XIX.—Metallurgy.	(10) Iron and Steel Manufacture.

Practical examinations are held by the City and Guilds in weaving and pattern designing, in metal plate work, in carpentry and joinery, and in mine surveying, while last year, for the first time, an examination was held in typography. This latter was conducted in several printing works placed at the disposal of the Institute, and thirty-two out of the seventy-seven candidates succeeded in composing and printing the difficult manuscript supplied to them—sufficiently well to obtain a certificate.

There is nothing at all approaching our own system of payment by results in any country in Europe, and eminent foreign educationalists have frequently deplored the absence of such in their own respective countries. This system has given particular vitality to that most valuable kind of education—evening class instruction; and as an examiner for both the bodies above alluded to, and after an experience—not a very enjoyable experience, and not the experience that a rich man would continue to indulge in—of upwards of 6000 examination papers, I may be permitted to testify to the valuable nature of the work done by the students, and the possibility of almost complete prevention of "cram" when proper precautions are taken. Thus, though large numbers of technical night classes exist all over the Continent, it is very doubtful if the results obtained by them are superior or even equal to our own.

When it is considered what splendid technical training the workshops and manufactories of this country have afforded, there will, perhaps, appear to be very good reasons why, originally, technical schools were not so extensively instituted at home as abroad, where almost all foreign States have established and maintained technical schools, the École Centrale at Paris being almost the solitary exception to this rule. When, however

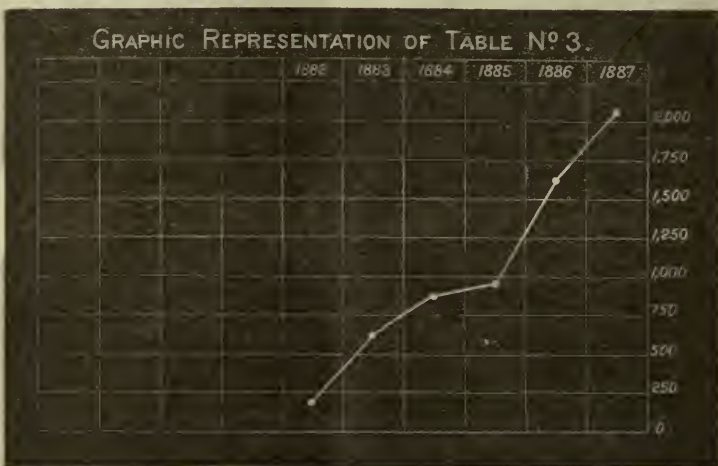


FIG. 3.—University College, Liverpool.

their need was felt, it was not left to the slowly-moving wheels of State to bring such schools on the scene. At first these schools took the form of lectureships and chairs in constructive science, for which the term "engineering" was conveniently adopted, the first of these being at London, Glasgow, and Manchester, and founded in connexion with the local Colleges. These have been gradually followed by Engineering Schools in the Colleges at Leeds, Sheffield, Nottingham, Dundee, Newcastle, Bristol, Birmingham, and last, but it is hoped not least, at Liverpool. These schools give instruction not merely in engineering subjects in a narrow sense of the word, but include in their courses of teaching the wide range of subjects necessary for laying a firm foundation for a successful career in any one of the constructive professions, and prepare a student to enter any of the particular branches into which engineering has become differentiated, and compare in this way with chemical teaching, which is given in places like this city—which may be specialized with a view to particular local industries. Besides these schools, others have arisen of a more special nature, due to liberal-minded men and public-spirited bodies, such as the Weaving and Dyeing Schools at Leeds, upon which the Worshipful Company of Clothworkers have spent between £20,000 and £30,000; the Technical Schools at Huddersfield, Bradford, Glasgow, Manchester, and other towns, some of which will bear comparison with the celebrated foreign schools of Chemnitz, Mulhouse, Verviers, Crefeld, and Vienna. The benefit of such schools has already been felt, for it is most encouraging to find amongst many similar opinions the testimony of the Technical Education Commissioners that—"In those textile manufactures in which other nations have hitherto excelled us, as in soft all-wool goods, we are gaining ground. We saw, at Bradford, merinos manufactured and finished in this country, which would bear comparison in texture and colour with the best of those of the French looms and dye-houses, and in the delicate fabrics of Nottingham and Macclesfield (thanks in great measure to their local Schools of Art) we no longer rely upon France for design."

The address, after pointing out that this country was, taken as a whole, after all not in such a deplorable state with regard to technical education, asserted that such education was of two kinds—general and special. General technical education may be said to be that necessary in all large centres of population, being the preparation for such callings as engineering, architecture, medical science, and other professions, which at any rate a certain percentage of the inhabitants will always follow, besides training of another kind suitable to the artisan class. Special technical education is that necessary in a locality where there are special industries, instances of which have already been given, and others will readily occur to the mind.

The remainder of the address was devoted to considering the educational work of Liverpool and its special technical require-

ments. A brief reference was made to the progress of University College, as shown by the following table of attendances in the day classes since its foundation in 1882.

TABLE III.—*Entries in Day Classes, University College.*

1882.	1883.	1884.	1885	1886.	1887.
189	625	883	944	1642	2063

These results are better shown graphically in Fig. 3.

During the last few months no less than £30,000 has been contributed to the Engineering Department alone, but the other professorships are all provided for upon an equally sound basis, and Prof. Hele Shaw thus concludes:—

"Hence, side by side with teaching, directed—sometimes perhaps only directed—to the practical purposes of life, we shall, thanks to the liberal endowment of chairs of language, of literature, and of art, always have the more liberal studies, and, as their exponents, scholars of the highest culture. Thus every individual professor thinking, as he ought to think, his subject to be the most important of all (a feeling I must, in common with the rest, confess to having myself), and so led to work for its due recognition, the happy mean will doubtless be maintained between mere idealism on the one hand, and mere routine on the other. Yet one word more. There is another motto prominent upon the College crest ('*Fiat lux*'), in the spirit of which work will always be true to the highest ideal. Our national life depends upon our national progress, and when we cease to advance, decay will speedily follow. Just as surely our College life, vigorous because growing, depends for its vitality upon the reality of the effort we make to carry forward the light of truth, and should never suffer because we strive to keep in touch with the requirements of practical life. Scientific investigation and philosophic research must have their proper place and support, and if allowed fair scope for development, will exercise the needful influence, and one that will be of untold value upon such narrowing tendencies as there may be in our various schemes of technical education."

SCIENTIFIC SERIALS.

American Journal of Science, October.—The relations between wind velocity and pressure, by H. Allen Hazen. A comparative study is made of the experiments carried out by Borda, Hagen, Piobert, Didion, Morin, and more recently at Washington, showing the great necessity there is for further research before absolutely trustworthy results can be obtained. Experiments are much needed, especially with larger plates than 2 feet square, with bodies of other forms than those hitherto employed and with high velocities by a straight-line motion.—Is there a Huronian Group? (continued), by R. D. Irving. After establishing the existence of a true Huronian Group, the author proceeds to define its character, showing that many formations even in the Lakes Superior and Huron regions, have been wrongly referred to this type. The presence is clearly demonstrated of two entirely distinct and mutually discordant series in the Marquette, Penokee, and Menominee districts. In all these regions there are great discordances between a lower set of gneisses and other crystalline schists, intruded by granite, and an upper set of detrital rocks carrying iron. The so-called Animiké series is then considered, and referred with the older Penokee formations to the Huronian system.—Oxygen in the sun; contributions from the Physical Laboratory of Harvard University, by John Trowbridge and C. C. Hutchins. The experiments here described have been carried out in order to test the soundness of the conclusion generally drawn from Dr. Henry Draper's discovery of bright spaces in the solar spectrum apparently coincident with the bright lines of the spectrum of oxygen. This conclusion is shown to be at least premature, and in the numerous photographs taken of the solar spectrum by them the authors have failed to discover any line that could with certainty be pronounced brighter than its neighbours. The bright bands of Dr. H. Draper's spectrum are found to be occupied by numerous dark lines of various degrees of intensity; but the hypothesis of Prof. J. C. Draper that these are the true representatives of the oxygen lines is rendered untenable by the lack of any systematic connection between the two.—Bismutosphærite from Willimantic and Portland, Connecticut, by H. L. Wells. An analysis of two specimens of basic bismuth carbonate shows them to be apparently identical with Weisbach's bismutosphærite, the composition of which had been considered somewhat doubtful.—Note on some remarkable crystals of pyroxene from Orange County, New York, by George H. Williams. The lower back part of some of these specimens is exactly like the lower front quarter, but in a reversed position, so that the lower half is a twin as represented by Von Rath, while the upper half is apparently simple and of the usual habit.—The flow of solids, or liquefaction by pressure, by William Hallock. The experiments here described point at the conclusion that pressure alone cannot truly liquefy a solid—that is, diminish its rigidity; consequently neither can chemical or mineralogical changes be produced by pressure alone without a rise of temperature.—Analysis of some natural borates and borosilicates, by J. Edward Whitfield. The series of analyses here described have been undertaken to verify, if possible, the given formulæ, and correct errors caused by defective analytical methods of estimating the boric acid of natural borates. The percentages of boric acid as here determined by direct analysis do not differ greatly from the results of Stromeyer's and Marignac's methods.—The Texas section of the American Cretaceous, by Robert T. Hill. In this paper the author studies the true character of the deep marine Cretaceous strata already determined by him in Texas, at the same time explaining some new features of it, which throw much light on the various American chalk systems.—Notice of new fossil mammals, by O. C. Marsh. Descriptions are given of some new species of *Bison alticornis*, *Aceratherium acutum*, *Brontops robustus*, *Menops varians*, *Titanops elatus*, and *Allops serotinus*, recently received at the Yale Museum from the West.

Rivista Scientifico-Industriale, September 15.—On the pressure of mixtures of gases and vapours, and on Dalton's law, by Prof. G. Guglielmo and V. Musina. Regnault, while admitting that Dalton's law on the tensions of vapours in gases is not strictly verified, and that the maximum tensions are less in gases than in vacuum, concluded that the law was theoretically exact, and would even be verified in practice in a receptacle whose walls were formed of the liquid generating the vapour. The experiments here described have been carried out for the purpose of testing the accuracy of this view, with the result that the attraction of the walls for the vapour is far from sufficing to explain the discrepancies of the Daltonian law. Consequently this law is not even theoretically correct, at least so far as can be concluded from these researches, which, however, will require to be repeated with apparatus insuring greater precision than those here employed.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 31.—M. Janssen in the chair.—Observations of the minor planets made with the great meridian of the Paris Observatory during the second quarter of the year 1887, by M. Mouchez. The right ascension and Polar distance, with correction of the ephemerides, are given for Belisane, Athor, Asterope, Nausicaa, Vesta, Antiope, Amphitrite, Polana, Bellona, Hecuba, and Arethusa.—On the Observatory of Nice, by M. Faye. In connection with the Geodetic Conference just concluded at Nice, the author announced that the magnificent Observatory of that place, due to the munificence of M. Bischoffsheim, is now completely finished. This institution, he added, is entirely at the service of the astronomers of all nations who may wish to avail themselves of its exceptional advantages in the prosecution of their researches.—New fluorescences with well-defined spectral rays, by M. Lecoq de Boisbaudran. The results are described of spectral researches made with gallina and samarine ($Ga_2O_3 + \frac{1}{50}Sm_2O_3$) moderately calcined; the same very highly calcined; gallina and the earth $Z\alpha_2O_3$; gallina and the earth $Z\beta_2O_3$; and alumina with a small portion of the oxide of praseodyme (Pr_2O_3) highly calcined.—Observations of the new planet, Peters (270), made at the Observatory of Algiers with the 0·50m. telescope, by MM. Rambaud and Sy. The observations cover the period from October 14 to October 17.—Observations of the new planet, Knorre (271), made at the same Observatory by the same astronomers during the period from October 20 to October 24.—Magnetic declinations and inclinations observed in Tunis by the Hydrographic Mission of 1884–86, communicated by M. Bouquet de la Grye. The results of these observations are tabulated for twenty-one places, whose latitudes and longitudes are also accurately determined.—On the

phosphites of ammonia, by M. L. Amat. The process is described by means of which the author has obtained the salt (PhO_3HO) NH_4O,HO, which has not hitherto been studied. It may be prepared very easily in beautiful crystals and in a perfectly pure state, which is rarely the case with phosphites.—On the production of the double carbonate of silver and potassium, by M. A. de Schulten. The carbonate of silver obtained by the action of an alkaline carbonate on the nitrate of silver is found to be sometimes yellow, sometimes white, while in most cases the white precipitate takes the yellow colour when washed with water. The experiments here described show that, as anticipated by the author, the white colour of the precipitates is due to a combination of the carbonate of silver with the alkaline carbonate, this combination being transformed by the water into a yellow carbonate by eliminating the alkaline carbonate.—On some salts of aniline, by M. A. Ditte. The salts here described are formed by metallic acids almost insoluble in water, or by energetic oxidants, and have been obtained by the process of double decomposition. They comprise a molybdate, a tungstate, a vanadate, an iodate, a chlorate, and a borate.—Formation of normal amylic alcohol in the fermentation of glycerine set up by *Bacillus butylicus*, by M. Ed. Charles Morin. Fitz has shown that, under certain conditions of temperature and environment, this Bacillus transforms glycerine into alcohols, glycol, and acids. To the normal ethylic and propylic alcohols determined in the products of the fermentation must now be added normal amylic alcohol, which may be easily extracted by distillation.—On a remarkable variety of mineral wax, by MM. G. Dollfus and Stanislas Meunier. The specimens here described came from Sloboda Rungorska, near Kolomea, in Austrian Galicia, where petroleum wells have recently been sunk. A rough analysis yields $H = 15, C = 85$, corresponding to the formula CH, with density 0·60.

BERLIN.

Physical Society, October 28.—Prof. von Helmholtz, President, in the chair.—The President gave a heart-felt address in memory of the late Prof. Kirchhoff, who was Vice-President of the Society.—Dr. Robert von Helmholtz showed and explained before the Society the experiments on vapour currents, of which he has recently given an account in *Weidemann's Annalen*. In his earlier experiments on the formation of mist he arrived at the same results that had been obtained by Aitken—namely, that the condensation of supersaturated aqueous vapour, as it forms a mist, takes place only at some nucleus which is provided ordinarily by the particles of dust in the air. His observations on vapour currents have, however, now shown that other conditions have an influence on the condensation. When a platinum wire heated red-hot by an electric current is brought near a current of vapour, the colour of the latter changes owing to an increased condensation. A similar result was obtained when the following agents were employed instead of the red-hot platinum wire, viz. the gases evolved from a hydrogen flame; the gases which rise from a glowing wire gauze; a metallic point from which electricity is making its exit; an electric spark; the vapours which rise from sulphuric acid; sal-ammoniac when formed in the current of vapour by the interaction of hydrochloric acid gas and ammonia. In all these last-named cases, where the condensation is facilitated, it is impossible to speak of any "nuclear" action. The speaker was of opinion that a supersaturated vapour, just as is the case with water cooled below its freezing-point, or a supersaturated solution of any salt, can be made to pass from its condition of unstable equilibrium by two means, either by some "nuclear" action or by a sudden vibration. Mist formation is the result of a "nuclear" action in those cases in which the atmospheric dust induces a condensation in the supersaturated vapour. The condensation must be regarded as the result of the sudden vibration in the other cases mentioned above. Although in these cases no truly mechanical vibration takes place, still the chemical processes involved in the production of the gases evolved by the flame, in the evaporation of the sulphuric acid, in the formation of the sal-ammoniac, at the point from which the electricity is making its exit, and in the electric spark, are to be regarded as so many sources of molecular tremors which upset the unstable equilibrium of the supersaturated vapour.—Dr. Dieterici gave an account of his experiments on the determination of the mechanical equivalent of heat by the indirect electrical method. He made this choice of method on account of the exactness with which electrical values can now be determined in absolute units. The speaker described the general arrangement of his experiments and gave a detailed account of the ice calorimeter which he used, as specially modified by himself. As the result of his series of measurements he obtained closely agreeing values for the mechanical equivalent of heat, namely 424·4 and 424·2 as the mean of each series, the highest and lowest values obtained differing but little from the mean of the determinations. When making his calculations the speaker took as the specific heat of water, the mean of the determinations made between 0° C. and 100° C. The statements which have been made respecting changes in the specific heat of water as dependent on changes of temperature differ so greatly with different observers that the mean values based on their results provide no constant factor; the speaker's determinations would have been considerably different had he taken as his basis any other value of the specific heat of water. He next compared the results of his experiments with those of earlier observers, and discussed the very marked differences in the values given for the specific heat of water at various temperatures. He thinks that the specific heat of water may best be determined by the electrical measurement of the mechanical equivalent of heat, and intends to investigate this question more fully at a later date.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Pen and Pencil in Asia Minor: W. Cochran (Low).—An Elementary Treatise on Light and Heat: Rev. F. W. Aveling (Relfe).—British and Irish Salmonidæ: F. Day (Williams and Norgate).—*Vega* Expeditionens, 2 vols.: A.E. Nordenskjöld (Beigers, Stockholm).—L'Atmosphère et Météorologie Populaire: C. Flammarion (Hachette, Paris).—Spezial Karte von Afrika, 2, 3, 4, 5 Lief. (Perthes, Gotha).—Guatemala; the Land of the Quetzal: W. T. Brigham (Unwin).—The Microscope in Theory and Practice, translated from the German of Prof. Carl Naegeli and Prof. S. Schwendener (Sonnenschein).—Reynolds's Experimental Chemistry, Part 4, Organic (Longmans).—Klima und Gestaltung der Erdoberfläche: Dr. J. Probst (Schweizerbart'sche, Stuttgart).—Beiträge zur Geophysik, i. Band: Prof. Dr. Georg Gerland (Schweizerbart'sche, Stuttgart).—Die Japanischen Seeigel, 1 Theil: Dr. L. Döderlein (Schweizerbart'sche, Stuttgart).—The Lake Age in Ohio: E. W. Claypole (Maclachlan and Stewart).—Gold-fields of Victoria, Reports of the Mining Registrars for Quarter ended June 30, 1887 (Melbourne).—Report on the Progress and Condition of the Government Botanical Gardens at Saharanpur and Mussoorie for Year ending March 31, 1887 (Allahabad).—Coleoptera; or, Beetles of South Australia: J. G. O. Tepper (Wigg, Adelaide).—The Answer to the Universal Question, What is an Earthquake? —Journal of Anatomy and Physiology, October (Williams and Norgate).—Journal of the National Fish-Culture Association, October.—Journal of the Chemical Society, November (Gurney and Jackson).

CONTENTS.

THURSDAY, NOVEMBER 17, 1887.

POLITICS AND THE PRESIDENCY OF THE ROYAL SOCIETY.

THE combination of vigorous intellect, profound knowledge, and scrupulous integrity, is not so common among our legislators, that a good citizen, whatever his political convictions, can have any feeling but one of satisfaction at the entrance into the House of Commons of the new member designate for the University of Cambridge. Prof. Stokes's foes (if indeed he have any foes), no less than his friends, will concur in attributing these qualifications to him. No man in the scientific world is, or deserves to be, more respected or more popular.

In that world many will doubtless find an additional source of congratulation in this public recognition of the merits of their colleague by the dominant political party in the University of Cambridge. And many will probably entertain the hope that the addition of another man of science to the three or four, who already occupy seats in the House of Commons, may do something towards the enlightenment and guidance of the House and of the Government, when scientific questions come under discussion.

In the minds of thoughtful men, more or less familiar with the realities of political and official life, however, it is probable that reflections of a less satisfactory nature may arise. They may regret that faculties which are so eminently fitted to serve science should inevitably be devoted to the interests of a party. Inevitably, because, with whatever high resolves the nominee of the Conservatives of Cambridge enters Parliament, he will find, before he has been there a week, that he is expected to do what the Whips bid him to do. And again such persons may think, not unreasonably, that Science is every day becoming more and more able to look after her own interests; and that, for her own honour and dignity, it is better that they should be neglected than that they should be promoted by back-stairs agencies. Moreover, experience may suggest that the deliberate judgment of the majority of scientific men, upon any question in which State intervention is called for, may be widely different from the view taken by this or that member of their body who happens to have a seat in Parliament; and that it is extremely undesirable that less legitimate methods of influencing a Minister should be substituted for the present fair and open mode of placing a case before him by responsible and authorized deputations.

But, whatever doubts may be entertained as to the service which has been, or can be, rendered to science by scientific members of Parliament, it is obviously within the right of every man to judge for himself whether he will become one or not. So far as Prof. Stokes is simply a very distinguished mathematician and physicist, it is for him, and for him alone, to decide between the claims of science, on the one hand, and those of political and ecclesiastical conviction on the other.

At the present moment, however, Prof. Stokes is something more than an eminent investigator and teacher: he is President of the Royal Society; and, as such, he enjoys all the prestige which is given by the fact that, in the eye of the public, he has the oldest, the strongest, and the most widely representative body of men of science in the country at his back. The President is the organ and mouth-piece of the Council of the Royal Society—a body which has frequent and important relations with the Government; and, as such, it may often be his business to represent to the Government the conclusions at which the Council arrives. It is therefore highly important that the freedom of the President's intercourse with Ministers should be in no way trammelled by his political relations.

It may be quite safely affirmed that Prof. Stokes's political and ecclesiastical views were not taken into consideration by those who placed him in the chair of the Royal Society. The last half-dozen of his predecessors, to go no further back, have sedulously abstained, during their occupancy of the chair, from holding office in any other Society, no less than taking part in any public, and especially political, action about which the opinions of the Fellows could be divided. Prof. Stokes has not followed this prudent example. Some little time ago he accepted the Presidency of a body of pronounced theological tendencies; and he now accepts the nomination of a no less pronounced political party, and, since our note upon his candidature appeared, he has issued an address in which he promises to devote himself to certain party objects.

It does not appear that Prof. Stokes has obtained, or, indeed, sought, the sanction of the Council or of the Society, at large, for this departure from precedent. For such it is, in spite of the fact that Sir Isaac Newton was a member of Parliament during his Presidency, and that many peers have occupied the chair. But it is obvious that a peer need not be a party politician; and, as regards the precedent of Sir Isaac Newton, it is enough to point out that the House of Commons of the end of the nineteenth century is a very different body from the House of Commons of the beginning of the eighteenth century. The position of an independent member has become impossible; and those who refer to Prof. Stokes's address will see that, whatever his first feelings may have been, he, now at any rate, does not propose to be anything but a staunch Conservative.

No doubt there are many staunch Conservatives in the Royal Society, but no doubt also there are many equally staunch Liberals and Radicals; and if it had entered into the imagination of the latter that Prof. Stokes would carry the prestige of the Presidency into the service of their political opponents, it may be doubted whether they would have voted for him. The same argument would apply with equal force if Prof. Stokes happened to be a Liberal. The question before us is one not of party, but of principle.

We are in the midst of a great political struggle, and it may be safely predicted that the force of party feeling will increase rather than diminish for years to come. If it is permissible that the President of the Royal Society may be a political personage, the minds of the Fellows on St. Andrew's Day will be divided between two sets of considerations. Not only will each ask, "Is A.B. the best man for the Presidency in the interests of science and of the Society?" which is the only question he ought to put; but he will ask, "Is A.B. of my politics, or the opposite?"

It is eminently true of political passion that a "little leaven leaveneth the whole lump"; once inoculate the Royal Society with that virus, and the poison will spread through the whole organism. The Council practically chooses the President: it will therefore be necessary to look to the politics of the Councillors. The Fellows elect the Council: have a care, therefore, to the politics of the new Fellows. We may yet see a politico-scientific caucus. Some years ago a most sagacious and experienced man of affairs in the United States was asked why, in drawing up the constitution of a new University, he had not given such persons as the Governor and Chief Justice of the State an *ex-officio* position on the governing body. "Ah," said he, with a shrewd smile, "if you only knew the trouble my colleagues and I have taken to render it impossible for any political person to have anything whatever to do with the administration of the University! We know to our cost that wherever politics enters corruption follows."

The records of the Royal Society tell us of more than two centuries of scientific life, fertile in good work and unstained by anything worse than an occasional outbreak of prejudice or jealousy. The only occasion on which it ever manifested a political bias was in the case of Priestley; and it has no reason to be proud of that episode.

The Society is now at the parting of the ways. Either it will continue its beneficent work for untold ages to come, untroubled by the transitory political and social storms raging around it; or, headed by politicians pledged to serve their party, it will gradually be dragged down into that miserable slough in which no capacity seems proof against the temptation to sophistical special pleading and no character strong enough to refuse degrading subserviency to party exigencies.

The occasion is grave and demands action. It is for the President, by the course which he may think fit to adopt, to determine what that action shall be.

THE STORAGE OF ELECTRICAL ENERGY.

The Storage of Electrical Energy. By Gaston Planté. (London: Whittaker, 1887.)

TO the author of this book we owe the use of lead plates instead of platinum plates in voltameters. His experiments showed that, after repeated charging and discharging of lead-plate voltameters, accumulators of energy were producible which might be employed in a great variety of useful ways. He showed that his accumulators might be charged in parallel by a few Bunsen or Daniell cells, and discharged in series. As his accumulators had small internal resistances, he was able to give to circuits either of small or great resistance very considerable supplies of electric power for short times, and as an experimenter he availed himself of this novel power in heating wires, melting beads of metal, and generally of observing effects produced by strong currents.

Many of the phenomena observed by him were new, and well worthy of being recorded, as they were recorded, in the proceedings of scientific Societies; and the present book, in addition to a fine portrait of the author, and many other engravings, and a dedication to the Emperor of Brazil, seems to be merely a collection of these papers of M. Planté, published between the years 1859 and 1879. In the first chapter of the book and part of the second we find an interesting account of experiments with various electrodes in voltameters, which led the author to use lead instead of platinum, and of the forms which the author gave to his cells, with directions for their formation, and speculations as to the chemical actions involved. The remaining twelve and a half chapters may be regarded as almost solely devoted to the "effects created by currents combining quantity with high tension"—to use the old-fashioned phraseology which Mr. Elwell, the translator, has thought fit to use upon the title-page—and to the author's speculations upon things in general.

The infancy of the electric accumulator lasted to 1879, its boyhood to 1883, and we may now be said to know it in its manhood. The advance since 1879, not only in our knowledge of the chemical and electrical actions going on in the accumulator, but also in our methods of applying this knowledge, has been quite as wonderful as the advance made in any other part of applied physics. Batteries of accumulators capable of driving boats 80 feet long, of driving numbers of tram-cars, of maintaining large installations of electric lights, are now in actual use. Plates of lead are now used as in 1879, but the salts of lead in contact with the metallic plates are attached mechanically, hundreds of devices having been tried and rejected or adopted in the last eight years for the purpose of obtaining great capacity and longevity. Of these great changes, the results of numerous, most costly, and carefully conducted experiments, made by scientific men, M. Planté tells us nothing. He was in charge of the accumulator in its infancy; it was taken away from him in 1879, and its subsequent history seems to be as unknown to him as the boyhood and early manhood of Harry Bertram were to Dominie Sampson.

The dominie looked upon his pupil, now grown to be a man, as if he were still a boy who was about to resume his childish studies, and in the same way it is probable that M. Planté regards the accumulator of 1887 as in no respect different from the laboratory toy with which he obtained such remarkable effects prior to 1879. M. Planté gives in this book what may be regarded as the history of the infancy of the electric accumulator; and it is obvious that if he had written it as charmingly as Mrs. Molesworth herself could have written it for the nursery, yet, with the misleading title which it possesses, he has given occasion to the ordinary reader to feel greatly disappointed. We are here assuming that M. Planté shares with Mr. Elwell the responsibility of publication, and also of change in the name of the book from that of the first edition—"Recherches sur l'Électricité"—published in 1879, which is the only French edition with which we are acquainted.

The technical terms used by the translator are not now so familiar to students as they used to be in the good old times when *strength*, *intensity*, *quantity*, and *power* of a current were synonymous with each other or with electromotive force.

It was this freedom in "the older electricity" which enabled statements like "The E.M.F. was thus found equal to 1·41, the current from the Bunsen cell being 1" (p. 17) to be enjoyed by readers. Other statements like

this: "We have found that the resistance of secondary cells of the various dimensions which we have used varied from 2 to 5 metres of a copper wire 1 millimetre in diameter" (p. 64), show that M. Planté sought for greater exactness in his measurements than many of his contemporaries during the infancy of the electric accumulator.

It was to be expected that in suggesting yet untried applications of secondary batteries the author should make statements which any student now knows to be erroneous. An example is to be found at p. 105, where it is suggested that, by using a secondary battery, two ordinary Bunsen cells might be enabled to work a continuous voltaic arc. As was also to be expected in such a republication of papers as this, many of which read like the contents of an inedited laboratory note-book, there are repetitions of the same facts and sentiments.

Unfortunately there is another resemblance to laboratory notes in much of the matter of this book which cannot be so readily forgiven. One often confides to one's note-book a speculation which is based on a very far-fetched resemblance between two phenomena. And it is quite possible to find in a note-book such a note as this (p. 198): "The experiment described above (158) in which a cloud of metallic oxide, torn from an electrode by the current, takes a spiral motion in the body of a liquid under the influence of a magnet, seemed of a nature to explain, in particular, the remarkable form of spiral nebulæ." Then follows a description of the nebulæ observed by "Lord Ross," and the further remarkable note: "In view of so striking a similarity, may it not be reasonably supposed that the nucleus of these nebulæ may be formed by a veritable electrical furnace; that their spiral form is probably caused by the presence of celestial bodies powerfully magnetized, and that the direction of the curve of the turns in the spiral must depend upon the nature of the magnetic pole turned towards the nebula."

This sort of thing may be found in the note-book of almost any laboratory worker, but it is astonishing to find that M. Planté has not only published it in the proceedings of a scientific Society, but actually publishes it again after he has had many years of leisure for reflection and for verification. These speculations occupy many chapters of the book. M. Planté describes some natural phenomenon, such as globular lightning, the formation of hail, water-spouts, cyclones, the aurora, atmospheric electricity, spiral nebulæ, or solar spots; he then begins to write on the vague analogy existing between this natural phenomenon and some isolated phenomenon observed by him in the laboratory, and after he has written some pages, the analogy becomes very indistinct; but he continues to write in the hope that if he writes long enough he may obtain clearer ideas. Of the same kind are his "views" of the nature of electricity. He finds that when successive intense currents are sent through fine wires, which are, of course, greatly heated, the wires lose their straightness in curious ways. It is very interesting to read about the observed phenomena, but unfortunately we have the author's speculations as well. He says (p. 247):—"The phenomena we have just described (313–20) are of a nature to throw some light on the mode of propagation of electricity. The molecular vibrations revealed by knots formed in a metallic wire, by the curious noise, and by a notable change in its cohesion under the influence of the passage of the dynamo-static current which we have just studied, must be produced in a lesser degree in conducting substances traversed by electric currents of very low tension. This vibration may be too feeble to be perceptible, but it is not the less real. We are then able to conclude that the electric movement must diffuse itself in substances after the manner of a purely mechanical motion, by a series of very rapid vibrations of the more or less elastic matter through which it passes."

He then goes on in his last chapter, without a thought of the possibility that very rapid heating of a not perfectly homogeneous conductor might explain his phenomena, to build up a theory of electricity from these isolated facts with the help of a few far-fetched analogies, and he publishes his theory without further verification. In spite of our great obligations to M. Planté, we feel that he has set the very worst example possible to the probable readers of his book, in publishing these vague speculations of his.

JOHN PERRY.

FRITSCH'S CRUSTACEAN FAUNA OF THE CHALK OF BOHEMIA.

Die Crustaceen der Böhmischen Kreideformation. Von Prof. Dr. Anton Fritsch und Jos. Kafka. Pp. 55. (Prague: Selbstverlag, in Commission von Fr. Řivnáč, 1887.)

THERE is probably no sedimentary deposit in the whole series of the stratified rocks with which one is more familiar than the Chalk. This is doubtless due to its peculiar whiteness, and to the fact of its occupying so large an area in our eastern and south-eastern counties, and its prominence in the coast-sections of Yorkshire, Kent, and Sussex, and the opposite coast of France; forming at Dover those white cliffs which gave to our shores their ancient name of Albion.

In the Cretaceous formation, however, we include a set of other beds, very dissimilar from the Chalk in appearance and composition, but which, on stratigraphical and palæontological grounds, seem to form a natural rock-system. These are known as the Upper Greensand, the Gault Clay, the Lower Greensand, and the Wealden Beds, comprising marls, sands, clays, and even fresh-water limestones. Without entering into details as regards the minor divisions, we may say that the major proportion of these deposits are marine, as shown by the organic remains contained in them. The Chalk itself, from its general purity, must have been formed in a deep and open sea; indeed, the researches which have been carried on in the North Atlantic Ocean show that the materials for a continuous bed of limestone with flint-nodules are now being deposited at depths of from 400 to 2000 fathoms, while many forms of life met with there are analogous to those of the Chalk.

That this old Cretaceous sea must have been of very wide extent is proved by the enormous area over which its sediments have been traced, as shown on our geological maps; whilst outliers and vast beds of

flint-gravel derived from the Chalk give evidence of a still wider region once covered by its waters, but whose deposits have since been removed by denudation.

The *white* Chalk, whence the name "Cretaceous" was taken, is almost wholly confined to the Anglo-Parisian area, where the system was first studied, but the formation, varying in lithological characters, may be followed from England into France, Belgium, Holland (Maestricht), Denmark (Faxoe), south of Sweden, Hanover, Brunswick, Saxony, Bavaria, Bohemia, Moravia, Switzerland, Austria, and the chain of the Alps, the Mediterranean Basin, including parts of Spain, south of France, Italy, Greece, Asia Minor, Sicily, and North Africa. This latter is the well-known "Hippurite Limestone" of the South of Europe, which stretches away to Persia and the Himalayas, and extends over the greater part of the continent of India. Cretaceous fossils have also been traced as far south in Africa as Natal.

The vastness of the Cretaceous system in North America far exceeds even our largest computation of its aggregate mass in the European area, being from 11,000 to 13,000 feet in thickness; whilst in our own hemisphere it probably does not exceed 7000 feet as a whole. It extends across the breadth of the North American continent, and over wide regions in South America, marked by many of the characteristic fossils of the Cretaceous rocks of Europe. But the evidence of contiguity to land in North America demonstrated by plant and animal remains far surpasses our own very limited records of shore and shallow-water conditions in Cretaceous times in Europe. Nevertheless we do possess at Aix-la-Chapelle, and in Saxony and Bohemia, Upper Cretaceous beds containing plant remains, such as leaves of *Acer*, *Alnus*, *Credneria*, *Cunninghamites*, and *Salix*, with Conifers akin to *Sequoia* and *Pandanus*, South African, and Cape *Proteaceæ*, and many Cryptogams, chiefly ferns, such as *Gleichenia*, *Lygodium*, *Asplenium*, &c. These have been dealt with elsewhere, as have also the Cephalopoda ("Cephalopoden der Böhmischen Kreideformation," von Dr. Anton Fritsch; Prague, 1872).

The present monograph presents us with descriptions and figures of seventy-two species of Crustacea obtained from eight localities and well-marked beds in the Cretaceous formation of Bohemia. These are divisible into Cirripedia (twenty-one species), Bivalved Entomostraca, Ostracoda (twenty-one species), Decapoda-Macroura (eighteen species), Decapoda-Brachyura (twelve species). The Cirripedia, with one exception, all belong to the stalked division (Lepadidæ), or "barnacles," eleven species being common to our own Chalk and Gault. In these are included two varieties of that most aberrant genus *Loricula*, first described by Sowerby from the English Chalk, and afterwards more fully by Charles Darwin. This pedunculated genus, by a retrograde development, no longer stands supported on its stalk, but lies prone, attached by one side to the surface of some shell, or other foreign body, its five rows of peduncular imbricating scales (over 100 in number) serving to form a dermal covering to the soft parts of the animal, which must have been distorted in its mode of growth somewhat as the flat-fishes (Pleuronectidæ) are modified as the result of their recumbent habits.

A *Balanus*, referred to a new genus (*Balanula* ?), is supposed to represent a sessile form of Cirripede. Such a form, *Pyrgoma cretacea*, was described from the Upper Chalk of Norfolk by H. Woodward in 1868 (see *Geol. Mag.*, vol. v. p. 258, pl. xiv. figs. 1-3), but the "acorn-shells," Sessile Cirripedes, mostly belong to the Tertiary and Recent periods, in which they attain a large development all over the globe.

The Ostracoda have been determined by Herr Joseph Kafka, Dr. Fritsch's assistant, in the Museum at Prague. Of the twenty species here treated of, five have been previously figured and described as new by Herr Kafka in the *Sitzungsb. K. böhm. Gesell. Wiss.*, Prag, 1885. The figures and woodcuts of the old species have been mostly taken from Prof. von Reuss's memoir on the Microzoa in Geinitz's "Elbithalgebirge," and the new species are here also figured in woodcuts, some of which leave much to be desired as to "finish" of characteristic features. Figs. 24 and 25 appear to belong to *Macrocypris*, and not to *Bairdia*. Fig. 26 has no relation to *Bairdia*, but may be a *Cytherella*. The representation of *Cythere reticulata*, Kf. (Fig. 32, *a*, *b*, *c*), has some peculiarities which better figures perhaps would clear up. Though not mentioned by Herr Kafka, ten of the species are found also in the English Chalk, and the others (excepting Fig. 32) have near allies in that formation in Western Europe. It is stated that in Bohemia the Ostracoda are mostly found in the Senonian stage. Only *Cytheridea perforata*, and four other species, come from the Turonian beds of Weissenberg.

Turning to the higher forms of Crustacea, the Decapoda (crabs and lobsters), only a single species, *Enoploclytia leachii*, Mantell, is recognized as being identical with our Chalk Crustacean fauna; but the genera *Hoploparia*, *Callianassa*, *Palæocorystes*, *Necrocarcinus*, *Etyus*, and *Astacus* are represented by corresponding species in the two areas. *Callianassa* is said to be represented by six species. This is a burrowing form, of which only the great chelate appendages are usually found fossil, or are brought up in the dredge from deep water, and it is extremely doubtful, judging from the author's figures, whether more than about three out of six of Fritsch's species can be maintained. One Greensand species occurs in Ireland, and the well-known *Callianassa faujasii* described eighty-eight years ago from the Uppermost Chalk of Maestricht. We have also a Tertiary form described from the upper marine series, Hempstead, Isle of Wight. All these species are very nearly related to each other.

Perhaps one of the most interesting forms described by Dr. Fritsch is his *Stenocheles esocinus*, the long slender-toothed chelæ of which agree closely with those of *Astacus* (?) *zaleucus*, W. Schm., a Crustacean dredged up in 1000 fathoms during the *Challenger* Expedition near St. Thomas in the West Indies.

The present work is illustrated by ten chromolithographic plates and seventy-two text figures.

This series of fine memoirs, which is being issued by Dr. Fritsch from the Royal Bohemian Museum, Prague, will certainly maintain the merit, and serve to enhance the reputation, of that great institution, which has, quite recently, been so well endowed by the magnificent bequest of the late Dr. Joachim Barrande, the historian and palæontologist of the Silurian system of Bohemia.

OUR BOOK SHELF.

Manual of Mineralogy and Petrography, containing the Elements of the Science of Minerals and Rocks. By James D. Dana. Fourth Edition, Revised and Enlarged. Illustrated by numerous Woodcuts. (New York: Wiley and Sons; London: Trübner and Co., 1887.)

THAT a new edition of this important and admirable manual has been issued will be good news to all interested in mineralogy, and especially to the teacher and student. The book, which now consists of 517 pages, is well arranged throughout, and contains, as all such books should do, a full index. The whole body of mineralogical science is here brought to focus, and the present edition, in that part of it relating to the description of minerals, is brought down to the year 1886, many new species described during the past six years being included. The chapter on rocks has been re-written, re-arranged, and enlarged, and many illustrations are new. We would suggest to the learned author that in the next edition a chapter on meteorites and their mineralogy would form an appropriate and much-valued addition.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

"A Conspiracy of Silence."

THE article which I contributed to the September number of the *Nineteenth Century*, on the Coral Islands of the Pacific, has done what I intended it to do. It has called wide attention to the influence of mere authority in establishing erroneous theories and in retarding the progress of scientific truth. The vehement assault made upon it in the current number of the same review by Prof. Huxley, and the article by Prof. Bonney in this journal, are to me gratifying evidences of success. But both of these writers are entirely wrong in the interpretation they put on a few expressions in my paper. They interpret these expressions as conveying imputations on the probity and honour of scientific men in the habitual and wilful suppression or discouragement of what they know to be truth. But there is nothing to justify this interpretation. I have made no such accusation, and if any one else were to make it I should join the two indignant Professors in repudiating it. Scientific men are not only as good as other men in this way, but generally a great deal better. Prof. Huxley has been irritated by some "anonymous sermon," which I have not seen and for which I am not responsible. He admits that it is in this anonymous production that the "slanders" against scientific men have taken the peculiarly offensive form; but he maintains that this unknown writer has been "inspired" by my article on Coral Islands. On the strength of this assumption—which may be true for aught I know—he goes on through some seven pages to dissect certain parts of my paper, and to read into it a great deal that is due to his own excitement and to nothing else.

I have no difficulty in expressing clearly and without any circumlocution exactly what I do mean, and what I have intended to say. Prof. Bonney interprets it very fairly, in abstract, when he says that the moral of my paper is, "Beware of idolatry." Some theory, hypothesis, or doctrine, is propounded by a great man. It becomes established, partly perhaps by certain inherent elements of strength, or at all events of attractiveness. But soon it stands unassailable, and unassailed, upon the vast foundations of general acceptance and admitted authority. It becomes what Prof. Huxley on a celebrated occasion, and with at least a momentary insight, called "a creed." The effect of such a position is tremendous. Some men who see cause to doubt are daunted. They keep silence. Others are prevented from even thinking on the subject. A few who do think, and who do doubt, and who do venture to express their doubts, are discouraged and discountenanced. A great many others take refuge in a suspended judgment, even after the production of evidence which, in the absence of a "creed" and of authority, would have been deemed conclusive. In all this there may be, and in general there is, nothing worse than timidity on the part of those who are the laggards, or the opponents, in some great advance. It is more difficult for some men than for others to face a prevalent opinion or an accepted doctrine. It is all very well to say, as Prof. Bonney says, that "to the man of science truth is a pearl of great price, to buy which he is ready to part with everything previously obtained." But scientific men are human. They are, I admit, immensely superior to the politicians, especially just now. But they have their failings, and everyone who knows the history of science must be able to call to mind not one instance only, but many instances, in which the progress of knowledge has been delayed for long periods of time by the powerful and repressive influences of authority, exerted in one or other of many ways.

My contention is that Darwin's theory on the origin of the Coral Islands is a case in point. I believed in it or accepted it, for many years, as others did. Prof. Bonney admits that I have described it not only fairly, but as forcibly as if I were still its advocate. This is exactly what I tried to do. I now hold that it has been disproved, and has been replaced by another theory quite as grand, and more in harmony with other natural laws which are of universal operation, but have been only lately recognized. I affirm, farther, that this new theory or explanation has been received with the timidity, the discouragement, the discountenance, and the obstruction which are characteristic in such cases. That Dr. Geikie has supported it, is most creditable to him. But his voice is not enough to disprove the truth of my contention. That Prof. Huxley and Prof. Bonney should be unable to make up their minds upon such evidence as has been before us now for several years is, in my opinion, a strong confirmation of the law which is operating upon them. There are some discoveries in science—some explanations of curious phenomena—which are self-luminous. They shine with their own light. The moment they are suggested, with a few cardinal and certain facts to illustrate them, they are their own proof. Everything that turns up speaks in support of them. My conviction is that such is the character of Mr. Murray's theory of the coral island formations in the Pacific.

Prof. Huxley challenges me to re-affirm with better proof the fact I allege—that Mr. Murray has met with discouragement. I respond at once to that challenge. I have seen the letter from Sir Wyville Thomson in which that naturalist urged and almost insisted that Mr. Murray should withdraw the reading of his papers on the subject from the Royal Society of Edinburgh. This was in February 1877. No special reason was assigned, but the terms of the letter indicate clearly that Sir Wyville dreaded some injury to the scientific reputation of the body of naturalists of whom he was the chief, and for whom, as connected with the *Challenger* Expedition, he was in some degree responsible. He had not himself at that time, I believe, fully accepted the new doctrine. But that would have been no sufficient reason for discouraging free discussion, if it were indeed as free as it ought to be. In my article I understated the delay which was thus occasioned. Three years, not two, elapsed

before Mr. Murray was at perfect liberty to advocate his views in the proper place, before a scientific body.

But the challenge of Prof. Huxley has brought to my knowledge a new bit of circumstantial evidence to the same effect, which is highly significant. Among the investigators of the Pacific corals no man has done better work than Dr. Guppy, Surgeon of H.M.S. *Lark*. Since my article was written, his volumes on the Solomon Group of islands have been published. The geological volume is an admirable memoir. It is the record of observations as patient, detailed, and conscientious as have ever been made on the great geological problem which is at issue. After his return home he was advised by Mr. Murray to offer a paper on his researches to the Geological Society of London. He did so in the spring of 1885. But his paper was refused—much to Dr. Guppy's disappointment. It was not orthodox. His facts effectually removed some difficulties in the way of Mr. Murray's theory—facts which in more than a corresponding degree were adverse to the theory of Darwin. As a consequence the Royal Society of Edinburgh has had the honour of receiving and publishing Dr. Guppy's most interesting memoir. As a Scotchman I am proud of this contrast. I make no accusation of wilful unfairness against the authorities of the Geological Society of London, of which my critic Prof. Bonney was, I believe, at that time the President. They did not consciously discourage truth. On the contrary, they probably smelt heresy. But if their minds had been free from this prepossession—if they had been alive to the breadth and sweep of the questions at issue, and open to receive with welcome the crucial evidence bearing upon them which is contained in Dr. Guppy's paper—the rejection of it would have been impossible.

As regards Darwin's own state of mind upon the subject, I can only say that my information was as good as that in the possession of Prof. Huxley. I am not struck by the perfect candour of his reference to Darwin's letter to Prof. Semper in October 1879. If he had quoted the very next sentence to that which he does quote, a very different impression would have been left on the reader's mind. But I attach no importance to this point. I prefer to believe that Darwin's mind was open to conviction, and to hope that others will follow his example.

ARGYLL.

The Theories of the Origin of Coral Reefs and Islands.

I WAS pleased to see Prof. Bonney's article on the Duke of Argyll's strictures on scientific men ("A Conspiracy of Silence," NATURE, November 10, p. 25). It is to be hoped that the rhetoric and methods of Parliamentary debate will not become common in scientific controversy. The Duke is, however, not the first who has tried to show "that if Darwin had lived he would with his well-known candour have been the first to admit the truth of Murray's theory," &c., &c. This I submit is a species of rhetoric which is out of place in scientific discussion.

It so happens that shortly after the appearance of Mr. Murray's paper "On the Structure and Origin of Coral Reefs and Islands," in NATURE, August 12, 1880 (p. 351), I had occasion to write to Dr. Darwin, and in my letter the following passage occurs, which I only quote to make Darwin's answer intelligible :—

"*September* 21, 1880.

"I think the theory Mr. Murray sets forth—that the cones or peaks, on which he considers atolls have been formed, have been levelled up by pelagic deposits, and thus brought within the limits of reef-building coral growth—a very far-fetched idea."

To which Darwin with his usual acumen replies :—

"*Beckenham, September* 22, 1880.

"I am not a fair judge, but I agree with you exactly that Murray's view is far-fetched. It is astonishing that there should be rapid dissolution of carbonate of lime at great depths and near the surface, but not at intermediate depths where he places his mountain peaks.

"Dear Sir, yours faithfully,
"CH. DARWIN."

As so far there appears to have been no *written* expression of Darwin's views published, this quotation may be of value.

T. MELLARD READE.

Park Corner, Blundellsands, November 11.

Earthquake at the Bahamas.

I AM instructed by the Meteorological Council to inclose copies of reports from the Resident Justice and Light-keepers of Inagua, Bahamas, relating to an earthquake on September 23 last, which you may think worthy of a place in NATURE.

ROBERT H. SCOTT, *Secretary.*

Meteorological Office,
116 Victoria Street, London, S.W.
November 11.

The Resident Justice at Inagua to the Colonial Secretary, Nassau. In re Earthquake at Inagua.

Resident Justice's Office, Inagua, September 27, 1887.

I HAVE to report that this island was visited by a severe shock of earthquake at 7 a.m. of the 23rd instant; the effect on the light tower, the keeper reports, was terrific, two nuts on the iron stauncheon of the smoke-stack were broken, and several cylinders. A portion of the stone wall around the Residency, and other private property, were thrown down in Mathew Town.

At 8.10 p.m. another shock was felt, no damage at the township; at the light station the cylinder on the lamp was broken, and the keepers were compelled to extinguish the light to prevent conflagration. A new cylinder having been placed in position, the light was again lit in about six or eight minutes after the accident.

At midnight another shock was felt, and the light-keeper reported next morning several cracks in mortar inside of the light tower; the light continued good.

Since the 23rd instant several light shocks have been felt, which keeps the people in a state of alarm.

We have had no arrival from Hayti and neighbouring islands, and it is feared that some of them have greatly suffered.

(Signed) G. R. McGREGOR, *Resident Justice.*

The Hon. Robt. Butler, Acting Colonial Secretary.

Principal and Assistant Light-keepers, Inagua, to the Inspector of Lighthouses, Nassau.

Inagua Light Station, September 29, 1887.

SIR,—I beg most respectfully to report for your information that this station and island was visited by several severe shocks of earthquakes on the 23rd, 24th, 25th, and 26th instants. The shock on the former date was felt at 7 a.m., which shook the tower and dwelling severely. Two nuts forming a part of fastening of iron rods in the upper part of lantern supporting upper barrel and smoke-pipe were wrenched off and smashed several cylinders.

The second shock, at 8.10 p.m., shook the tower very much, and smashed the cylinder on lamp. The light was then extinguished to prevent fire, which last about eight minutes [*sic*], when the light was again exhibited and kept burning bright and clear until daylight. There was another shock felt during the night, but not so severe. I noticed several cracks on the walls in the tower, which may be the mortar only. The latest shock was on the morning of the 26th at 1.3.

I am glad to say that the lamp and machinery are in good working order, but there will be slight repairs required.

The latest shock felt was at midnight of the 27th.

I also inclose the head of nut, the length of which is seven-eighths of an inch on inside.

I have, &c.,
(Signed) BYRON N. JONES, *Principal;*
CORNELIUS S. E. LOTMAN, *Assistant.*

The Inspector of Lighthouses.

RESEARCHES ON METEORITES.

I.

On October 4 I communicated to the Royal Society a preliminary note embodying some results I had obtained in observations on meteorites, undertaken with a view of obtaining additional information on some parts of the spectrum of the sun.

Some years ago I commenced a research on the spectra of carbon in connection with certain lines I had detected in my photographs (1874) of the solar spectrum. I have been going on with this work at intervals ever since; and certain conclusions to which it leads, emphasizing the vast difference between the chemical constitution of the sun and of some stars, recently suggested the desirability of obtaining observations of the spectra of meteorites and of the metallic elements at as low a temperature as possible.

I have latterly, therefore, been engaged on the last-named inquiries. The work already done, read in conjunction with the work on carbon, seems to afford evidence which amounts to demonstration on several important points.

The researches are still very far from complete, and the results must be given with great reserve, as the astronomical observations with which I have had to compare my laboratory work have been frequently made under conditions of very great difficulty.

A full report on the work, so far as it has gone, made to the Solar Physics Committee, which I have also communicated to the Royal Society, was read to-day, and I have received permission to publish part of it in this week's NATURE.

The general conclusions at which I have so far arrived may be stated as follows:—

I. All self-luminous bodies in the celestial spaces are composed of meteorites, or masses of meteoritic vapour produced by heat brought about by condensation of meteor-swarms due to gravity.

II. The spectra of all bodies depend upon the heat of the meteorites, produced by collisions, and the average space between the meteorites in the swarm, or in the case of consolidated swarms upon the time which has elapsed since complete vaporization.

III. The temperature of the vapours produced by collisions in nebulæ, stars without C and F but with other bright lines, and in comets away from perihelion, is about that of the bunsen burner.

IV. The temperature of the vapours produced by collisions in α Orionis and similar stars is about that of the Bessemer flame.

V. The line of increase of temperatures of the swarms of meteorites, and of subsequent cooling of the mass of vapour produced, and the accompanying phenomena, may be provisionally stated as follows:—

Sequences of Spacing and Temperatures (Provisional).

From Cold to Hot = Sparse to Dense Swarms.

	Spectrum of interspace. H.	C.	Spectrum of vapour of meteorite. *Radiation.*	*Absorption.*	Spectrum of meteorite. *Radiation.*
Nebulæ (without F)	Nil	Nil	Mg (500) ± 495	Nil?	Dimly continuous.
Comets 1866 and 1867	Nil	Nil	Mg (500)		
Nova Cygni after collision ...	Nil	Nil	Mg (500)... ...		
Stars with bright lines (without F)	Nil	Nil	Fe, Mn	Broad band 475	
Nebulæ (with F)	H	Nil	Mg (500) ± 495	Nil?	Continuous.
Stars with bright lines (with F).	H	Nil	Fe, Mn	D and *b* and other lines and bands	
Comets under mean conditions of collision	Nil	C	Mg(*b*)	Nil.	Continuous.
Comets at perihelion...	Nil	C	Meteorite lines.	(?)	
Stars, Class III.*a*	Nil	C		Meteorite flutings and lines	Vividly continuous.
Mixed swarms—					
R Geminorum	H	C	Meteorite lines.	Meteorite lines	
Nova Orionis at maximum.	H	C		Meteorite flutings and lines	
Condensation.					
Stars, Classes I. and II.			Continuous ...	High-temperature lines of substances present in meteorites	The radiation from individual meteorites now gives place to radiation from the interior vaporous and subsequently consolidated mass of the condensed swarm.
Subsequent Cooling.					
Stars, Class II. some stars, including sun ...			Continuous ...	K in excess	
Stars, Class III.*b*				Flutings of carbon ...	

VI. The brilliancy of these aggregations, at each (increasing) temperature, depends on the number of meteorites in the swarm—*i.e.* the difference depends upon the quantity, not the intensity, of the light.

VII. The existing distinction between stars, comets, and nebulæ rests on no physical basis.

VIII. The main factor in the various spectra produced is the ratio of the interspaces between the meteorites to their incandescent surface.

IX. When the interspace is very great, the tenuity of the gases given off by collisions will be so great that no luminous spectrum will be produced ("nebulæ" and "stars" without F bright). When the interspace is less, the tenuity of the gas will be reduced, and the vapours occupying the interspaces will give us bright lines or flutings ("nebulæ" and "stars" with F bright). When the interspace is relatively small, and the temperature of the individual meteorites therefore higher, the preponderance of the bright lines or flutings in the spectrum of the interspaces will diminish, and the incandescent vapour surrounding each meteorite will indicate its presence by absorbing the continuous-spectrum-giving light of the meteorites themselves.

X. The brighter lines in spiral nebulæ, and in those in which a rotation has been set up, are in all probability due to streams of meteorites with irregular motions out of the main streams, in which the collisions would be almost *nil.* It has already been suggested by Prof. G. Darwin[1]—using the gaseous hypothesis—that in such nebulæ "the great mass of the gas is non-luminous, the luminosity being an evidence of condensation along lines of low velocity according to a well-known hydrodynamical law. From this point of view the visible nebula may be regarded as a luminous diagram of its own stream-lines."

XI. New stars, whether seen in connection with nebulæ or not, are produced by the clash of meteor-swarms, the bright lines seen being low-temperature lines of elements the spectra of which are most brilliant at a low stage of heat.

XII. Most of the variable stars which have been observed belong to those classes of bodies which I now suggest are uncondensed meteor-swarms, or stars in which a central more or less solid condensed mass exists. In some of those having regular periods the variation would seem to be partly due to

[1] NATURE, vol. xxxi. p. 25.

swarms of meteorites moving around a bright or dark body, the maximum light occurring at periastron.

XIII. The spectrum of hydrogen seen in the case of the nebulæ seems to be due to low electrical excitation, as happens with the spectrum of carbon in the case of comets. Sudden changes from one spectrum to the other are seen in the glow of meteorites in vacuum tubes when a current is passing, and the change from H to C can always be brought about by increased heating of the meteorite.

XIV. Meteorites are formed by the condensation of vapours thrown off by collisions. The small particles increase by fusion brought about again by collisions, and this increase may go on until the meteorites may be large enough to be smashed by collisions, when the heat of impact is not sufficient to produce volatilization of the whole mass.

XV. Beginning with meteorites of average composition, the extreme forms, iron and stony, would in time be produced as a result of collisions.

XVI. In recorded time there has been no such thing as a "world on fire" or the collision of masses of matter as large as the earth, to say nothing of masses of matter as large as the sun; but the known distribution of meteorites throughout space indicates that such collisions may form an integral part of the economy of Nature. The number of bodies, however, subject to such collision is small, and must, it would appear, form but a small percentage of the celestial bodies, seeing that they must be consolidated.

XVII. *Special solar applications.*

α. The solar spectrum can be very fairly reproduced (in some parts of the spectrum almost line for line) by taking a composite photograph of the arc spectrum of several stony meteorites, chosen at random, between iron meteoric poles.

β. The carbon which originally formed part of the swarm the condensation of which produced the sun has been dissociated by the high temperature brought about by that condensation.

γ. The indications of carbon which I discovered in 1874 (Proc. R.S., vol. xxvii. p. 308) will go on increasing in intensity slowly until a stage is reached when, owing to the reduction of temperature of the most effective absorbing layer, the chief absorption will be that of carbon—a stage in which we now find the stars of Class III.*b* of Vogel's classification.

δ. At the present time it seems probable that among the chief changes going on in the solar spectrum are the widening of K and the thinning of the hydrogen lines.

Experiments upon which the Foregoing Conclusions depend.

A. Experiments upon carbon.

The main conclusions which may be stated here are that there are two systems of flutings which depend upon temperature only.

At low temperatures all compounds of carbon give a set of simple flutings, the brightest of which are at wave-lengths 4510, 4830, 5185, and 5610. At higher temperatures there is a series of compound flutings, the brightest edges of which are at wave-lengths 4380, 4738, 5165, and 5640. In the case of compounds of carbon with hydrogen there is an additional fluting at wave-length 4310, and this is the only criterion for the presence of hydrocarbon among the flutings shown on the map. (See Map 3.)

B. Experiments upon the luminous phenomena of the various metals volatilized in the bunsen burner and the oxy-coal-gas blow-pipe flame as compared with the phenomena seen at higher temperatures.

The main conclusions are that certain lines, bands, and flutings are seen in the bunsen burner, that a larger number is seen in the flame, and that the total number seen in the burner and flame is small.

The order of visibility in the bunsen is, roughly—

Lines ... { Mg, Na, Li, Tl, Sr, Ba, Ca, K, Mn, Bi

Bands { Ca, Sr, Ba

Flutings { Mg, Mn

All the observations both of bunsen and oxyhydrogen flame may be condensed as follows:—

In metals of the alkalies	Na K Li
,, ,, alkaline earths ...	Ca Sr Ba
In magnesian metals	Mg Zn Cd
In iron metals	Fe Ni Co Mn Cr
In metals which yield acids	Bi Ti W
In copper metals	Cu Tl
In noble metals	Ag Hg
In earthy metals	Ce

The following table shows the positions of the principal lines, bands, and flutings seen in the spectrum of each of the metals examined, arranged roughly in the order of their intensities.

It should here be stated that as some of the researches have had to deal with feeble illumination small dispersion has been of necessity employed, and to make the observations along the several lines comparable a one-prism spectroscope has been so far used throughout. Hence the wave-lengths given are in all cases only approximate. With this proviso the lines observed have been as follows:—

Lines {

In bunsen—

Mg	5183, 5172, 5167, 4586, 5201.
Na	5889, 5895.
Li	6705.
Tl	5349.
Sr	4607.
Ba	5534.
Ca	4226.
Mn	5395.
K	6950.
Bi	4722.

Seen on passing from the temperature of the bunsen to that of the oxy-coal-gas flame—

Fe	5268, 5327, 5371, 4383, 5790, 6024.
Cu	5105, 5781, 5700.
Cr	5202, 5203, 5207, 5410.
Zn	4810, 4911.
Cd	5085.
Ni	5476.
Ti	5128, 5338.
W	5490, 5511.
Ag	5208, 5464.
Hg	5460.
Ce	5273, 5160.

Bands {

In bunsen—

Ca	5535, 6250, 6500, 6000.
Sr	6050.
Ba	5150, 5250, 5330, 4860.

Seen on passing from the temperature of the bunsen to that of the oxy-coal-gas flame—

Co	4710, 4920, 5170, 5460.

Flutings	*In bunsen—*	
	Mg	5000
	Mn	5580, 5860, 6145, 5340
	Seen on passing from the temperature of the bunsen to that of the oxy-coal-gas flame—	
	Ba	6010, 6350, 6480
	Cr	5360, 5570, 5800, 6040
	Fe	6150
	Cu	6050, 6130
	Zn	5460, 5680, 4985, 5140, 5340

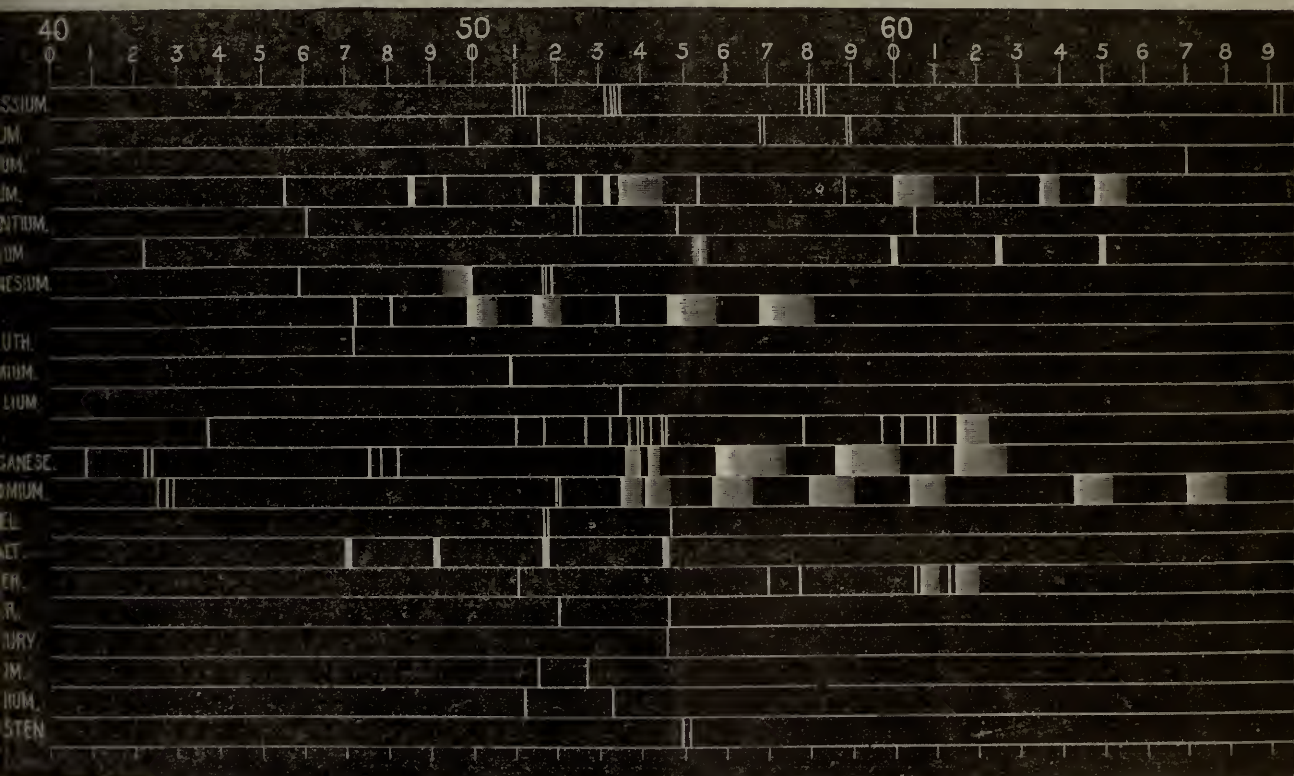
All the flutings, with the exception of magnesium, have their maxima towards the blue, and shade off towards the red end of the spectrum. (See Map 1.)

C. Experiments upon Mg at low temperatures.

I have again gone over the experiments already communicated to the Royal Society (Proceedings, vol. xxx. p. 27), and in addition have observed the spectrum of the metal burning in the centre of a large bunsen burner, in which case we get the line at 5201, and the fluting in the position of *b* without the fluting at 500. In the Bunsen as ordinarily employed the fluting at 500 far eclipses the other parts of the spectrum in brilliancy, and at this temperature, as already observed by Messrs. Liveing and Dewar (Proc. R.S. vol. xxxii. p. 202), the ultra-violet line visible is that at 373. Lecoq de Boisbaudran has observed the lines in the chloride at 4705 and 4483 ("Spectres Lumineux," p. 85).

D. Experiments upon the glow of Na and Mg in vacuum tubes.

A small piece of sodium, free from hydrocarbon, was placed in the lower limb of an end-on spectrum tube, and arrangements made for observing the spectrum of the gas evolved when the sodium was heated. Having first obtained as perfect a vacuum as possible, the sodium was gently heated, and the spectrum of the gas then gave nothing but the C and F lines of hydrogen. The pump being stopped and the sodium heated, a point was reached when C and F became very dim and were replaced by the structural spectrum of hydrogen.



Map 1.—Spectra of metals at the temperature of the oxy-coal-gas blowpipe.

In another experiment the sodium was replaced by a piece of magnesium along the end-on tube. The same process being gone through, similar phenomena were observed, but in the latter case there was a line at 500, in addition to the lines seen in the case of sodium.

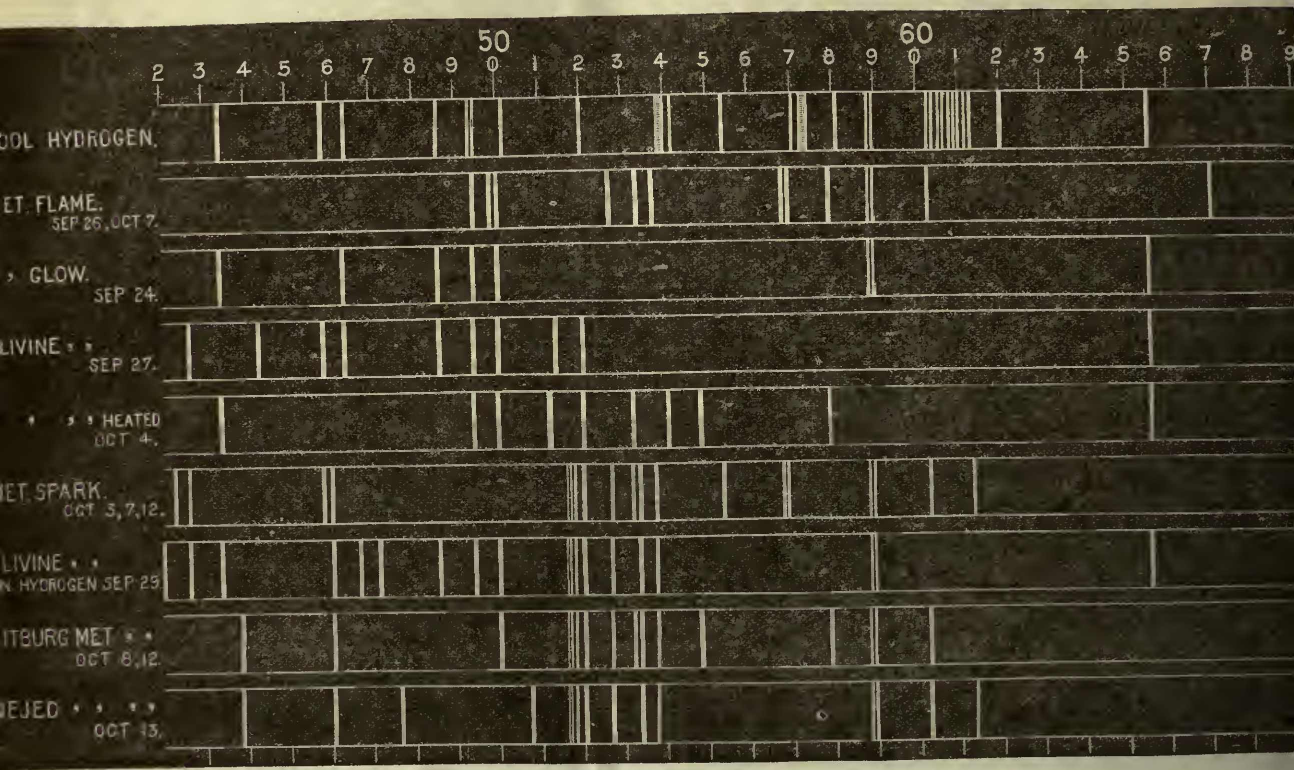
The important point, then, is the existence of a line at 500 in the spectrum when magnesium is heated, and the absence of such a line in the gas evolved from sodium under the conditions stated.

E. Experiments upon the conditions under which the C and F lines of hydrogen disappear from the spectrum.

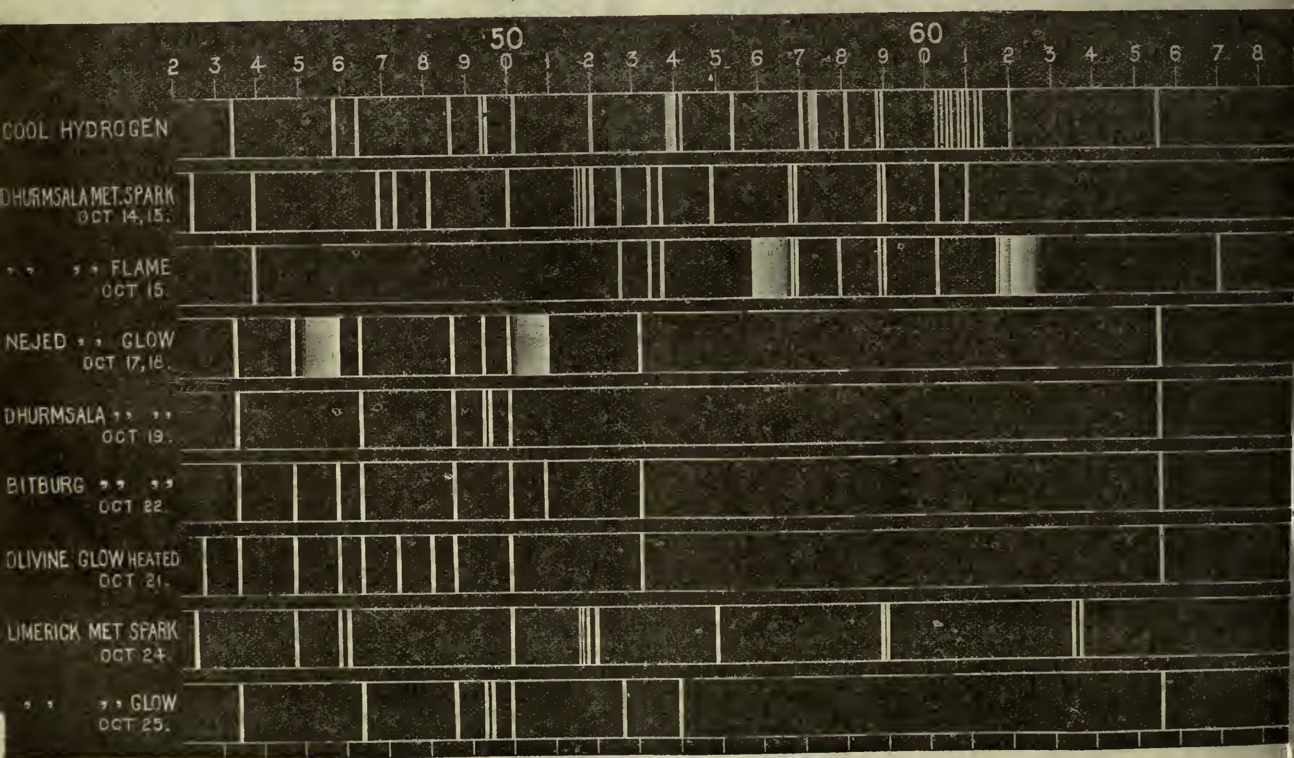
The association of the bright lines of hydrogen with nebulæ and many of the stars with bright lines and the so-called new stars points out at once that it is important to consider the various changes which hydrogen can undergo under various conditions of temperature and pressure. I pointed out many years ago that, when under certain conditions the spectrum of hydrogen is examined at the lowest possible temperature, the F line retains its brilliancy long after C disappears; and the fact that, after all the lines of hydrogen may be made to disappear from the spectral tube, the spectrum which remains visible, and is sometimes very brightly visible, is also due to hydrogen, has always been a matter of thorough belief in my mind, although so many observers, down even to M. Cornu not so very long ago, have been inclined to attribute it to the existence of "impurities."

I began to map the so-called structural spectrum at the College of Chemistry in 1869, but other matters supervened which prevented the accomplishment of this work. This, however, is a matter of small importance, because quite recently Dr. Hasselberg has communicated to the St. Petersburg Academy an admirable memoir on the subject, accompanied by a map (Mémoires de l'Académie Impériale, series vii. vol. xxx. No. 7, Hasselberg). The brightest portions of the structure spectrum are shown in Map 2.

The most convenient way of obtaining a supply of hydrogen for investigations of this kind is to use a little sodium which has never been in contact with hydrocarbon, or a piece of magnesium wire; to place them in the low end of a glass tube, one part of



MAP 2.—Spectra of olivine and meteorites under various conditions.



MAP 2A.—Spectra of olivine and meteorites under various conditions.

which can be used as an end-on tube, and then, after getting a vacuum so perfect that the spark will not pass, to slightly heat the metal. After a time the spectrum of hydrogen, sometimes accompanied by the low-temperature flutings of carbon, begins to be visible alike from the sodium and the magnesium.

If the vacuum has been very perfect to start with, the bright lines C and F will at first be visible without any trace of structure, and the hydrogen will be of a magnificent red colour. If now the action of the pump be stopped and the sodium be still more heated, a point will be reached at which the conductibility of the gas is at its maximum, and then, the jar not being in circuit, the structure-spectrum of the gas will be seen absolutely alone, without any trace of either C or F. The gradual disappearance of the F line is very striking, and when the bright line is out of the field the lines due to the structure seem to be enhanced in brilliancy.

The brightest part of the spectrum is then that near D; in the blue-green we have a line at 464 more refrangible than F, and then a doube line at 4930 and 4935; other less refrangible lines are seen. These are phenomena seen associated with sodium, but if we use the hydrogen produced from a piece of magnesium wire or from a crystal of olivine, under the same circumstances we find that so far as the lines of hydrogen go the phenomenon remains the same, but that there is then visible in the spectrum a line at 500, which has been recorded in the spectrum of magnesium under other conditions, not only by myself but by Dr. Copeland.[1]

F. Experiments upon the spectra of meteorites at low temperatures.

All the later observations recorded have been made on undoubted meteorites, fragments of which have been in the kindest manner placed at my disposal.

I. In the oxyhydrogen flame.

The observations gave in all only about ten or a dozen lines belonging to the metals magnesium, iron, sodium, lithium, and potassium, and two flutings, one of manganese, and one of iron.

II. With a quantity coil without jar.

The observations gave in all about twenty lines belonging to the metals magnesium, sodium, iron, strontium, barium, calcium, chromium, zinc, bismuth, and nickel, and four lines of unknown origin.

III. When heated in a vacuum tube when a current is passing along it.

A small piece of iron meteorite was inclosed in the middle of a horizontal tube, so that the spark might be made to pass through the tube and over the meteorite. After complete exhaustion has been obtained, the first spectrum observed when the tube, end on, is placed in front of the spectroscope, is a spectrum of hydrogen. The carbon flutings are only visible occasionally. If the meteorite then be very gently warmed by placing a Bunsen burner at some distance below the tube, the glow over the meteorite is seen to change its colour, and the line at 500 is constantly, and another line at 495, apparently exactly in the position of the second line of the spectrum of the nebulæ, is occasionally, seen. This line is more refrangible than the structure line of hydrogen in this region, which occupies the same position as the barium line. This, however, if the heating is continued, especially in the case of stony meteorites, is soon succeeded by a much more brilliant green glow, in which magnesium *b* and many other lines appear, now accompanied by the carbon flutings. The observations made under all the above conditions are shown in Maps 2 and 2A.

In these observations if a line in the meteorite spectrum were coincident with a metallic line, with the dispersion employed, in the absence of the brightest line of that metal, the line was regarded as originating from some other substance. Thus a line was sometimes seen at 5480, apparently coincident, with the dispersion employed, with the green lines of Sr and Ni; sometimes the brightest line of Sr at 4607 was absent, and it was then fair to assume that the presence of 5480 was due to Ni, but in the presence of 4607 it might be due to Sr.

[1] "To this table must be added 500·6 mmm. as the wave length of the first line in the great band of magnesium as determined by M. Lecoq de Boisbaudran from the spark-spectrum of the chloride of that metal, which evidently agrees with the flame-spectrum, in this region at least. It is worthy of note that this line almost absolutely coincides with the brightest line in the spectra of planetary nebulæ" (Dr. Copeland, *Copernicus*, vol. ii. p. 109).

Comparisons of the Foregoing Observations among themselves and with those made on various Orders of Celestial Bodies.

The discussions have taken, in the first instance, the form of comparisons of the different phenomena observed, and for this purpose all recorded observations of flutings and bright lines and dark lines in stars, comets, nebulæ, &c., have been carefully mapped in addition, all records having, when necessary, been brought to a common scale. Having these maps, I could then compare the totality of celestial observations with the laboratory work to which reference has already been made.

The following are among the comparisons already dealt with:—

I. The spectra of meteorites observed under the various conditions, chiefly considering magnesium, iron, and manganese, with the bright lines observed at low temperatures.

The main conclusions are:—

(1) That only the lowest temperature lines of Mg, Na, Fe, Cr, Mn, Sr, Ca, Ba, K, Zn, Bi, and Ni are seen in the meteorites under the various conditions. They are not all seen in one meteorite or under one particular condition; the details of individual observations are fully recorded in Maps 2 and 2A.

(2) That in the case of Mg the line most frequently seen is the remnant of the fluting at 500, while in a photograph the main ultra-violet line recorded is the one at 373, previously recorded under these conditions by Messrs. Liveing and Dewar. In the quantity spark other lines are seen, notably b_1, b_2, b_4, and 5201. The line at 500 was considerably brightened when the number of cells was reduced, thus showing it to be due to some molecule which can exist best at a low temperature.

(3) That in the case of Mn the only line visible at the temperature of the bunsen burner, 5395, is the only line seen in the meteorites.

(4) That the lines of iron seen in the meteorites are those which are brightest when wire gauze is burned in the flame. The chief of these are 5268, 4383, 5790, and 6024; it is possible, however, that the two latter are due to some substance, not iron, common to the gauze and the meteorites.

II. The spectra of meteorites generally, with the bright lines and flutings seen in luminous meteors, comets, and some "stars."

a. Luminous meteors.

With regard to the records of luminous meteors, it may be remarked that the observations, so far as they have gone, have given decided indications of magnesium, sodium, lithium, potassium, and of the carbon flutings seen in comets. The following quotations from Konkoly and Prof. Herschel are among the authorities which may be cited for the above statement.

"On August 12, 13, and 14, I observed a number of meteors with the spectroscope; amongst others, on the 12th, a yellow fireball with a fine train, which came directly from the Perseid radiant. In the head of this meteor the lines of lithium were clearly seen by the side of the sodium line. On August 13, at 10h. 46m. 10s., I observed in the north-east a magnificent fireball of emerald-green colour, as bright as Jupiter, with a very slow motion. The nucleus at the first moment only showed a very bright continuous spectrum with the sodium line; but a second after I perceived the magnesium line, and I think I am not mistaken in saying those of copper also. Besides that, the spectrum showed two very faint red lines."[1]

"A few of the green 'Leonid' streaks were noticed in November (1866) to be, to all appearances, monochromatic, or quite undispersed by vision through the refracting prisms; from which we may at least very probably infer (by later discoveries with the meteor-spectroscope) that the prominent green line of magnesium forms the principal constituent element of their greenish light."[2]

Again, later on in the same letter, Prof. Herschel mentions Konkoly's observation of the bright *b* line of magnesium in addition to the yellow sodium line in a meteor on July 26, 1873.

I again quote from Prof. Herschel:—

"On the morning of October 13 in the same year, Herr von Konkoly again observed with Browning's meteor-spectroscope the long-enduring streak of a large fireball, which was visible to

[1] Konkoly, *Observatory*, vol. iii. p. 157.
[2] Herschel, letter to NATURE, vol xxiv. p. 507.

the north-east of O'Gyalla. It exhibited the yellow sodium line and the green line of magnesium very finely, besides other spectral lines in the red and green. Examining these latter lines closely with a star-spectroscope attached to an equatorial telescope, Herr von Konkoly succeeded in identifying them by direct comparison with the lines in an electric Geissler-tube of marsh-gas. They were visible in the star-spectroscope for eleven minutes, after which the sodium and magnesium lines still continued to be very brightly observable through the meteor-spectroscope." [1]

The green line "*b*" of magnesium occurring as a bright line in luminous meteors indicates that their temperature when passing through our atmosphere is higher than that of the bunsen, and we may add of comets as generally observed, although some exhibit the *b* lines of magnesium and those of iron when at perihelion, as shown later on.

The two lines which Konkoly supposes are probably due to copper will, I expect, be found to be iron lines when other observations are made of the spectra of meteors.

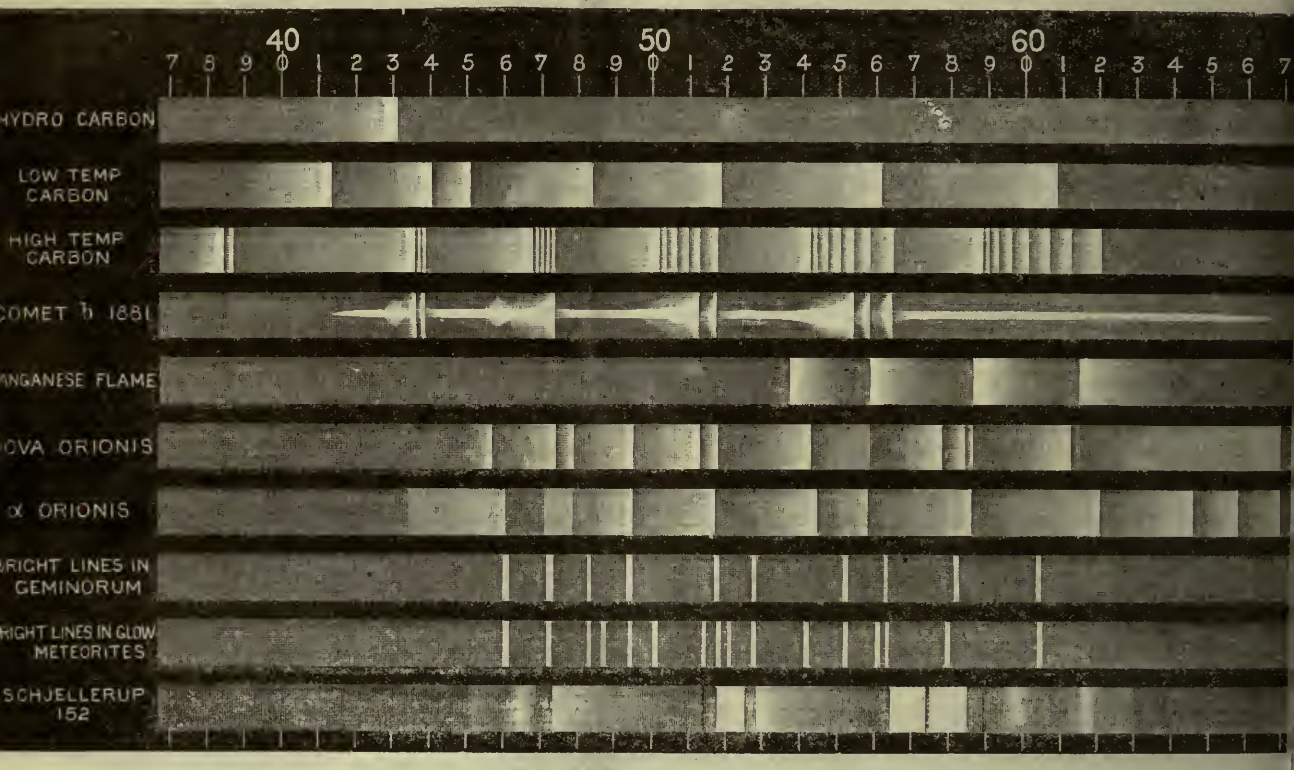
The main conclusions from this comparison are then: (1) that the temperature of luminous meteors is higher than that of the bunsen flame; (2) that the meteorites which produce the phenomena we are now discussing are hotter than those in the experimental glow taken generally; and (3) that in both cases flutings of carbon may be seen.

β. Comets.

When the meteorites are *strongly* heated in a glow-tube, the whole tube when the electric current is passing gives us the spectrum of carbon.

When a meteor-swarm approaches the sun, the whole region of space occupied by the meteorites, estimated by Prof. Newton in the case of Biela's comet to have been thirty miles apart, gives us the same spectrum, and further it is given by at all events part of the tail, which in the comet of 1680 was calculated to be 60,000,000 miles in length. The illumination therefore must be electrical, and possibly connected with the electric repulsion of the vapours away from the sun; hence it is not dependent wholly upon collisions.

Passing now from the flutings seen in cometary spectra, it is found that most of the lines which have been observed at perihelion are coincident with lines seen in experiments with meteorites,



MAP 3.—Comparison of flutings seen in the spectra of "stars" and comets with flutings of carbon, manganese, and zinc; and, in the case of R Geminorum, lines with remnants of flutings and lines seen in a meteorite glow. (The Zn fluting is at λ 544 in α Orionis.)

while the low temperature lines of Mg are absent. In the great comet of 1882, to which particular attention has been given on account of the complete record of its spectrum by Copeland,[2] the lines recorded were the D lines of sodium, the low-temperature iron lines at 5268, 5327, 5371, 5790, and 6024, the lines seen in the manganese spectrum at the temperature of the bunsen burner at 5395 and 5425, and a line near *b* which might be due to magnesium, or to a remnant of the carbon fluting. In addition to these there was a line at 5475, probably due to nickel, the absence of the blue strontium line indicating that it is not likely to be the green line of strontium. There were also four other lines less refrangible than D, the origin of which has not yet been determined. As the comet got further from perihelion the lines gradually died out, those which remained longest being the iron line at 5268 and the line near *b*. The absence of D before the disappearance of all the lines is probably to be accounted for partly by the greater brightness of the continuous spectrum in that region.

In the comets of 1866–67, when seen away from the sun, the only line seen was the one at 500.[1]

It is fair to myself to say that I was not aware of these observations when I began to write this paper. The fact of the line at 500 remaining alone in Nova Cygni made it clear that if my views were correct, the same thing should happen with comets. It now turns out that the crucial observation which I intended to make was made twenty years ago.

[1] Herschel, letter to NATURE, vol. xxiv. p. 507.

[2] *Copernicus*, vol. ii. p. 234.

[1] "In January 1866 I communicated to the Royal Society the result of an examination of a small comet visible in the beginning of that year (Proc. R.S. vol. xv. p. 5). I examined the spectrum of another small and faint comet in May 1867. The spectra of these objects, as far as their feeble light permitted them to be observed, appeared to be very similar. In the case of each of these comets the spectrum of the minute nucleus appeared to consist of a bright line between *b* and F, about the position of the double line of the spectrum of nitrogen, while the nebulosity surrounding the nucleus and forming the coma gave a spectrum which was apparently continuous" (Huggins, Proc. R.S. vol. xvi. p. 387).

In Comets *b*, 1881, and *c*, 1882, the only lines recorded were magnesium *b*; but, as before, the apparent absence of other lines might be due to continuous spectrum.

Of the five bands shown in Huggins's photograph of the spectrum of Comet Wells, taken with a wide slit, no less than three agree fairly in position with three lines seen in the spectra of meteorites. The wave-lengths of these are 4253, 4412, and 4769, and it is interesting to note that, so far, the origin of these lines is undetermined. The two remaining bands are at wave-lengths 4507 and 4634.

It is seen, then, that the spectra of comets—when their internal motions are relatively either slow or fast, and when therefore the number of collisions, and with them the heat of the stones in collision, will vary extremely—resemble the spectra of meteorites seen in glow tubes.

(γ) "Stars" with flutings which have been observed in the laboratory and in luminous meteors and comets.

The most prominent bright flutings of carbon are not only observed in luminous meteors and comets, but in stars of Class III.*a*, and in some "Novas," notably Nova Orionis. So far, then, these bodies may in a certain measure be classed with luminous meteors and comets. But there is an important difference in the phenomena, for we have absorption as well as radiation. The discussion shows that the dark (or absorbing) flutings in these bodies are partly due to the absorption of light by the most prominent flutings of Mn and Zn, seen at low temperatures. This inquiry is being continued.

We have, then, in these bodies a spectrum integrating the *radiation* of carbon and the *absorption* of Mn and Zn vapour.

The law of parsimony compels us to ascribe the bright fluting of carbon in these stars to the same cause as that at work in comets, where we know it is produced by the vapours between the individual meteorites or repelled from them.

Hence we are led to conclude that the absorption phenomena are produced by the incandescent vapour surrounding the individual meteorites which have been rendered intensely hot by collisions.

These stars, therefore, are not masses of vapour like our sun, but clouds of incandescent stones.

We have here probably the first stage of meteoritic condensation. J. NORMAN LOCKYER.

(*To be continued.*)

FAIRY-RINGS.

THE rains have come, and we have heard from all sides of the prolific crops of mushrooms and toadstools—paddock-stools, as they are termed in some northern districts—which have been springing up in the meadows and woods of England, Wales, and Scotland. Not only is surprise evinced at the marvellously rapid up-growth of these fungi, for the popular mind may well be amazed at that until a knowledge of the biology of these plants is more universal, but country people and dwellers in towns alike exclaim at certain other phenomena associated with their growth in the fields, and at none, perhaps, so much as what have been known from of old as "Fairy-rings" in England, *Hexenringe* and *Cercles de sorcières* on the Continent. Now fairy-rings, like very many other poetical objects, have of late years undergone the process of being explained away to an extent which, although it in no way removes the beauty from them, demands from us an admiration of a more stimulating and healthful character than the old awe which they inspired was capable of producing.

Disbelief prevails regarding Prospero and the beings that

> "By moonshine do the green sour ringlets make,
> Whereof the ewe not bites;"

and

> "whose pastime
> Is to make midnight mushrooms."

Fairy-rings are more or less regular and complete rings of grass, sharply distinguished from the ordinary grass surrounding them by means of their darker hue, more luxuriant growth, and other characters; in spring or autumn they are to be found with vigorous growths of mushrooms or toadstools springing from their outer margins, and the centre of the ring is often marked by a very poor crop of withered-looking herbage.

Before proceeding to give an account of the modern explanation of these remarkable objects, a few statements may be made as to their sizes, structure, and occurrence.

They are not always complete or regular rings, but may be parts of circles or ovals, or mere wavy strips. Nor are they always provided with the outer belt of fungi, though the rule is that a good season sees them so accompanied; if not, they do not remain long. In the typical cases, where the ring is annually provided with its fringe of fungi, it may go on increasing in size for years: records exist of rings which have been known to go on flourishing for forty or sixty years, and large rings on a hill-side could be seen from a considerable distance.

As to their sizes, they are known to commence as very small patches, but specimens have been measured as much as 60 feet and more in diameter. Indeed one observer refers to a fairy-ring which was nearly 100 feet across. While regarding these cases as rare extremes, it is well known that rings 12–20 feet in diameter have often been recorded, and, as we shall see, these must be several or many years old.

Although fairy-rings are usually noticed in meadows and on pasture lands, they are found on hills as well as in valleys, on dry soil as well as on wet, in woods and on heaths, and even in rocky places and situations near the sea. Perhaps the only generalization possible in this connexion is that they do not occur on highly-cultivated rich land.

On regarding carefully a typical fairy-ring, it may be found to present the following characters:—The central area, encompassed by the dark-green ring, consists of poor or even withered herbage—it may be of inferior grasses alone, or of these mixed with other plants. Then comes the band of luxuriant grass forming the ring proper: the grass composing this may be of more than one kind—*e.g. Lolium perenne* (the perennial rye-grass), *Dactylis glomerata* (the cock's-foot grass), and *Bromus mollis* (the soft brome) are common.

These grasses are rank, tall, and of a distinctly darker, bluer green hue than the rest; it is their coarseness, height, and especially the deeper colour, which render them so prominent. Fringing this ring, at the proper season, are found the spore-bearing heads of the Agarics *i.e.* the mushrooms or toadstools as the case may be; and if the observer digs carefully below the soil, he will find that these Agarics spring from a felted mass of root-like threads, the mycelium of the fungus. Then, outside all, comes the general herbage of the pasture, or whatever it may be: this is often scanty, indicating poor soil, and in any case is less luxuriant and lighter in colour than the rank herbage of the ring itself.

As with the herbage composing the rings, so the Agarics fringing them may be of different kinds. In the autumn the fairy-rings of this country and on the Continent commonly contain *Marasmius oreades*, Fr., a small pale mushroom with cream-coloured gills, and much esteemed as an esculent. It has a somewhat strong aromatic odour, and its mycelium is attached to the roots of the grasses among which it grows. It must not be confounded with certain acrid species allied to it.

The common mushroom (*Agaricus campestris*, L.) is also frequently found in large circles, fringing more or less complete fairy-rings. Among other forms may be mentioned the gray *Agaricus terreus*, Schoeff, not uncommon in beech- and fir-woods; the "parasol mushroom" (*Agaricus procerus*, Scop.), also not uncommon in fir-woods and pastures, and spoken of as one of the best of the esculent forms; also *Agaricus personatus*, Fr., with a lilac or purple stem. This is a late form,

good to eat, and called the "Blewit." *Agaricus subpulverulentus*, Pers., is also not uncommon, and several others are known.

In the spring, fairy-rings have been found containing *Agaricus gambosus*, Fr., an edible mushroom known in England as the "St. George's mushroom," and much esteemed on the Continent.

There are also other forms, several of them poisonous, or at least inedible or dangerous; and even puff-balls are known to be associated with fairy-rings.

And now we come to the question, How do these fairy-rings arise and increase? It cannot be wondered at that the people of earlier days, wishing to explain a phenomenon which none could overlook, sought for satisfaction in their myths and folk-lore, and believed them to be "caused" by fairies and elves and other mystic beings of the woods and fields, dancing in circles beneath the moonlight, and enchanting the ground into a richness which it did not previously possess.

Then came the era of science, and people were dissatisfied with beliefs, and in course of time the followers of De Candolle at least tried to solve the problem according to what was known of Nature. It was at least necessary to explain (1) why the centre of the ring is so poor, (2) why the fungi are confined to the margin, and (3) why the ring goes on enlarging, as continued observation showed that it did.

The first theory of any merit was, that the "ring" takes its origin from a single mushroom, which sheds its spores from the gills down on to the ground around the thick stem: this necessarily produces a ring of spores, as the stem dies down in the centre. Now the physiologists of those days believed that a plant excretes into the soil at its base substances which are harmful to its further development, and so, they argued, the soil on the inside of the ring of spores is poisoned, as it were, and only the outer spores produce new plants. The new mushrooms come up in a ring, and in their turn shed spores in a ring of rings; but since the soil on the inside of all these rings is poisoned by the excreta, only the outer series can germinate and grow, and thus a new ring arises next season, and so on. But, it was thought, though the excreta are injurious to the growth of the same plant (the fungus in this case) in that particular soil, other plants can grow there (in the present instance, grasses), and so a ring of rank grass follows on, which in its turn spoils the soil for its own kind as it increases.

Now it has to be admitted that there was much ingenuity in this hypothesis, and it was maintained for some time; until, in fact, physiologists had to give up the excretion theory as not in accordance with observed facts.

Then followed the beginnings of the celebrated doctrine of the rotation of crops, and the facts accumulated about fairy-rings had to be looked at again. They had become too much for the excretion theory; how did they look when regarded from the new point of view? First, however, we may bear in mind the fact noticed by several observers. When two fairy-rings gradually extend so as to interfere, the green circles coalesce and form a single ring: evidently the conditions of the soil in the wake of the advancing ring are such that the grass of another advancing ring cannot go on luxuriating there. It is true this fact was as easily made use of by those who maintained the excretion theory as by those who advanced the theory we are now going to examine.

It gradually came to be recognized that the reason one species of plant cannot be continually grown on the same soil was not because the first crop poisons the soil by leaving injurious excreta behind it, but because it *takes away* certain mineral substances in such proportions that too little is left for the well-being of a second crop of the same species; in other words, it *exhausts* the soil of certain necessary ingredients. A crop of some other species may be raised on the partially exhausted soil, however, provided it is a plant which does not need the materials now deficient, in such large quantities as its predecessor. This is, roughly sketched, the *rationale* of the doctrine of the rotation of crops, and it was subsequently suggested that the "fairy-rings" we are considering are a natural illustration of this. The vegetable physiologists then came to the conclusion that the fungus causes the fairy-ring by exhausting the soil of certain substances which are necessary to its existence, and is only able to produce continued crops by extending centrifugally into soil which still yields these substances: the grass, however, does not need these substances in such large proportions, and so follows the fungus. But, as we have seen, the grass which immediately follows the fungus is particularly rank and luxuriant, and it was necessary to find an explanation for this fact. It was then suggested that the dying mycelium of the fungus acts as a manure for the grass to feed upon, and until this is exhausted the growth is peculiarly rich and rampant.

Before leaving this part of our subject, it should be pointed out that Dr. Wollaston, in an essay on fairy-rings published in the Philosophical Transactions of the Royal Society so long ago as 1809, ventured on the explanation that the fungi spread in rings, because the soil was, by their mycelium, progressively "exhausted of some peculiar pabulum necessary for their production. . . . An appearance of luxuriance of the grass would follow as a natural consequence, as the soil in the interior of a circle would always be enriched by the decayed roots of fungi of the preceding year's growth."

Meanwhile, the physiology of plants was passing into a more scientific phase of existence, and the beginnings of modern agricultural chemistry were made; and in 1846 an important contribution to our knowledge of fairy-rings was afforded by Way, who chemically analyzed the soil, the herbage, and the fungi of some of these curious formations. This chemist found that the fungi of his fairy-rings were remarkably rich in phosphoric acid and in potash; and that they also contained relatively large quantities of nitrogen. We know now that this is true of fungi generally, but these facts were by no means so well understood at that time. Way also analyzed the grasses composing the ring, and found that they also contained a larger proportion of phosphoric acid and potash than the herbage in the neighbourhood, but by no means so much as the fungi: the grass also contained considerable quantities of nitrogen.

The net result of these investigations was to explain fairy-rings as an illustration of the rotation of crops, but of course putting the explanation on much firmer grounds. Way also pointed out that as the rank green grass was cut or otherwise removed, valuable ingredients (phosphorus, potassium, alkalies, &c.), were removed with it, and so the crops of grass further inwards become poorer and poorer, accounting for the bare patches often found inside the dark ring.

Messrs. Lawes and Gilbert, whose magnificent experiments on the vegetation of agriculture will never be forgotten, supported the above view of the matter, and showed that the dark-green colour of the rank grass is due to the relatively large quantities of nitrogen. It was at this time (about 1850) customary to suppose that plants obtained their nitrogen from the atmosphere, a view now known to be erroneous from the brilliant researches of Boussingault, and of Lawes and Gilbert themselves. On this supposition the extraordinary accumulation of nitrogen (in the fungus and rank grass) was thought probably due to a power on the part of the fungus of taking nitrogen from the air. Subsequently the whole matter was again taken in hand by Messrs. Lawes and Gilbert, and the results published in the Journal of the Chemical Society, 1883.

The chief additional facts may be summarized as follows:—The fungi remove large quantities of carbon, nitrogen, and especially phosphoric acid and potash, from the soil. The soil inside the ring contains less nitrogen than that under the ring, and this again less than the soil outside the ring; a gradual exhaustion of nitrogen, then, is taking place as the fungus and rank grass extend the ring centrifugally, and this is promoted by the removal of the grass.

These observers also demonstrated the spread of the mycelium: it is in greatest abundance just below the outer edge of the ring. They conclude that the fungus has powers of obtaining nitrogen from compounds in the soil which are not available to the roots of the green herbage, but after the decay of the fungus mycelium the grasses can avail themselves of part of the nitrogen. The grasses—being plants containing chlorophyll—of course obtain their carbon from the carbon dioxide of the atmosphere; but the fungus—equally of course, in the light of physiology—obtains its carbon from some organic substances in the soil. The accumulation of phosphoric acid and potash has already been accounted for.

We may now sum up, then, the rational explanation of these curious fairy-rings as follows.

A mushroom spore may be supposed to start its growth in or beneath the dung of cattle, or a bird, on poor soil; the first crop of mushrooms, produced from the mycelium to which the spore gave rise, exhausts the soil of available carbon, nitrogen, phosphorus, potash, and other substances, storing all it can get in its own substance. The mycelium extends centrifugally "into fresh fields and pastures new," and the next crop of mushrooms arises at a distance from the centre; and so the growth proceeds. The grasses, among the roots of which this extension is going on, now avail themselves of the rich manure afforded by the decomposition of the older mycelium, and a struggle for existence is set up which results in the victory of the coarsest and rankest-growing species. These in their turn exhaust the available supply, and if cut it is removed in their substance: no wonder, then, that the inner parts of the area are poor, and support little or no herbage.

Messrs. Lawes and Gilbert's researches also showed that if the growth of the herbage is promoted by means of manures containing much available nitrogen the fungi are found to suffer, and the "fairy-ring" may be brought to an end. Again, unfavourable seasons of drought may cause the death of the mycelium, and rings which have flourished for years be thus destroyed.

We have attempted in this article to give a complete explanation of the rise and progress of "fairy-rings," as afforded by modern science. That much is clear which was previously obscure will have to be conceded; but are all the facts covered by the explanation? There are some inquiring spirits who are never satisfied with an explanation, and we run the risk of being classed among these malcontents, but there are one or two curious little points which still obtrude themselves upon our attention.

There is, in the first place, some difficulty in realizing how the fungi manage to obtain their large supplies of carbon and nitrogen and other elements from poor shallow soil, in the absence of larger quantities of organic matter than may occur: there is, in fact, considerable difficulty about the whole question of the nutrition of the fungus. A second point is that we find the ultimate filaments into which the mycelium of the fungus breaks up becoming lost among the roots of the grasses; and if the latter are carefully washed and examined with the microscope, their fibrils and root-hairs can be seen to be infolded by delicate hyphæ, and in some cases the root-hairs are pierced by them. We do not know that this has been demonstrated before, but we find it the rule with *Marasmius*, and have already succeeded in detecting something of the kind in other forms.

Now this looks very like parasitism; and we are tempted to pause before accepting the last explanation of fairy-rings as conclusive, or covering all the facts. It may be, in fact, that the hyphæ of the fungus stimulate the roots of the grasses to increased activity: this would account for the rampant growth and the result of the struggle for existence. Subsequently the hyphæ kill the grass-roots—or at any rate those of some species—which accounts for the bare patches in some rings. It also easily explains the sources of the carbon and nitrogen, if the hyphæ absorb nutritive materials from the hard-working grass-roots. This being the case, fairy-rings become still more interesting, since they afford an illustration of symbiosis of a peculiar kind, at any rate during part of the time that the grass and the fungus are in contact; and it seems not improbable that the theory of the formation of fairy-rings will have to be modified somewhat as follows.

A fungus-spore starts its mycelium among the roots of the grasses, and the hyphæ obtain a hold on some root-hairs and fibrils; the mycelium thus parasitic on the roots reacts in a stimulating manner on the latter, and we have a symbiotic relationship established between the fungus and the host. The consequence is that both flourish, and become rampant. It may be that only some grasses are thus stimulated, or even attacked, and this will affect their struggle for existence, and result in the selection of a few coarse forms. In time the hyphæ or the roots get the upper hand, and this is expressed in the survival of the grass, or its decay; in some cases it is clear that hyphæ are living at the expense of dead and dying roots.

However, until the results of investigations at present going on are set forth more at length, it is impossible to say which of the above explanations is the true one; in any case, the attachment of fungus hyphæ to the living grass-roots needs explanation, and it must also be allowed that at present we have no satisfactory theory to account for the nutrition of these rampant mycelia. But this is not the place to do more than point out how interesting the subject is, and how promising a field for further research it offers.

NOTES.

MR. W. BATESON, Fellow of St. John's College, Cambridge, who has just returned from a zoological expedition to Central Asia, and is well known for his researches on *Balanoglossus*, has been awarded the Balfour Memorial Studentship in Animal Morphology.

THE second meeting of the newly-formed Anatomical Society of Great Britain and Ireland will be held on Tuesday, November 22, at University College, Gower Street, at 5 p.m. The following papers will be read:—Prof. Sir William Turner, F.R.S., (1) "Variations in the Hippocampus Major and Eminentia Collateralis," by Robert Howden, and (2) "A Metallic Body in the Spinal Canal," by David Hepburn; (3) "Minute Anatomy of Clarke's Column in Spinal Cord of Man, the Monkey, and the Dog," by Dr. Mott; (4) "The Arteries at the Base of the Brain," by Prof. Bertram C. A. Windle; (5) "Note on the Functions of the Sinuses of Valsalva and Auricular Appendices, with some Remarks on the Mechanism of the Heart and Pulse," by Mayo Collier. A number of interesting exhibits are also announced.

ON Tuesday evening the second part of an important paper upon the causes of accidents in mines and the development of measures and applications for combating or avoiding them was read by Sir Frederick Abel at the Institution of Civil Engineers. The first part of the paper was read in May last, at the close of the session. Sir Frederick's ideas will be discussed at the meeting of the Institution next Tuesday.

We are glad to hear that the Scottish University Extension Scheme is likely to prove successful. A brilliant start has been made in Perth, where Dr. H. R. Mill is giving a course of lectures on physiography to a class of over 240 students.

Some time ago it was arranged that three lectures on "Heredity and Nurture" should be delivered at the South Kensington Museum, on behalf of the Anthropological Institute, by Mr. Francis Galton, President of the Institute. We are requested to state that these lectures have been postponed in consequence of Mr. Galton's indisposition.

Prof. J. McK. Cattell's paper on "The Psychological Laboratory at Leipzig," to be read before the Aristotelian Society on the 21st, will contain an account of the aim of experimental psychology, of the Leipzig Laboratory, and of the researches which have been carried on and are being carried on in it. The paper will be published in the January number of *Mind*.

The borings in the Delta of the Nile carried on by the Royal Society have been brought to a standstill by the breaking of the pipe. The depth reached is over 324 feet, still without the solid rock being found. It is possible that the work may be recommenced upon a larger scale.

Several years ago three Russian "lady doctors" started at Tashkend a consulting hospital for Mussulman women. From the beginning the experiment proved a success, and the popularity of the hospital has been increasing ever since. During the last twelve months no fewer than 15,000 consultations have been given.

The Russian Consul at Kashgar writes to the Russian Geographical Society that his endeavours to obtain from the Chinese authorities permission to erect a memorial to Adolf Schlagintweit on the very spot where Schlagintweit was killed have not been successful. The memorial will be erected in the Russian cemetery, where it will be at least protected from injuries.

The money necessary for sending out M. Wilkitski to make pendulum observations in Novaya Zemlya has been granted by the Russian Geographical Society. He will be accompanied by a naturalist, M. A. Grigorieff.

The Moscow Society of Naturalists invites those scientific bodies which would like to receive, in return for their own publications, the series of the Moscow *Bulletin*, to communicate with the Secretary of the Society.

Reports from Bergen, in Norway, seem to indicate that another great rush of herring under the west coast may be expected this winter, similar to those which have taken place periodically during the last two centuries through some unknown cause. The two greatest rushes on record were those of 1740 and 1807.

During a hailstorm at Mors, in Denmark, a few days ago, a flash of forked lightning—the only one occurring—struck a farm, and, having demolished the chimney-stack and made a wreck of the loft, descended into the living-rooms on the ground-floor below. Here its career appears to have been most extraordinary; all the plaster around doors and windows having been torn down, and the bed-curtains in the bed-rooms rent to pieces. An old Dutch clock was smashed into atoms, but a canary and cage hanging a few inches from it were quite uninjured. The lightning also broke sixty windows and all the mirrors in the house. On leaving the rooms it passed clean through the door into the yard, where it killed a cat, two fowls, and a pig, and then buried itself in the earth. In one of the rooms were two women, both of whom were struck to the ground, but neither was injured.

The last two numbers of the *Folk-Lore Journal* (vol. v. Parts 3 and 4) exhibit very varied fare, and show how this interesting Society is gradually embracing the whole world. Side by side with Miss Courtney's Cornish folk-lore, we have Mr. Mitchell-Linne's birth, marriage, and death rites of the Chinese, followed by the indefatigable Mrs. Murray-Aynsley's account of secular and religious dances in Asia and Africa, which extends over both numbers, and in Part 3 is succeeded by Mr. Clouston's two Pacific folk-tales. Folk-lore amongst the Somali tribes follows that of British Guiana, and is succeeded by Cornish, Irish, Malay, and North Friesland tales. Dr. Gaster's paper, in the same part, on the modern origin of fairy-tales, is a very suggestive one. Its conclusion, after an examination of certain examples, which "can be infinitely multiplied," is "that the literature of romance and novel, be it a religious romance or one of chivalry, has passed nowadays to a great extent into the literature of fairy-tales, and that, far from being the basis, the fairy-tales are the top of the pyramid formed by the lore of the people. They are the outcome of a long *literary* influence, as well as an oral one, which was exercised upon the mind and soul of the people during centuries." What may be called the editorial matter—the notes, news, &c.—is of the usual varied and interesting character.

Sir D. Salomon's little work on accumulators, issued by Messrs. Whittaker and Co., has passed rapidly through two editions. A third and much improved edition, with many illustrations in the text, will be ready shortly.

We have received the first instalment of what promises to be an important book, "Die Elektricität des Himmels und der Erde," by Dr. Alfred Ritter von Urbanitzky. The complete work will contain about 400 illustrations, including several coloured plates. The publisher is A. Hartleben, Vienna.

We have received the first number of the *American Journal of Psychology*, edited by Prof. G. S. Hall. The object of this periodical, as the editor explains, is to record psychological work of a scientific, as distinct from a speculative, character. The present number contains, besides reviews and notes, articles on the following subjects: the variations of the normal knee-jerk and their relation to the activity of the central nervous system, by Dr. W. P. Lombard; dermal sensitiveness to gradual pressure-changes, by Prof. G. S. Hall and Mr. Y. Motoro; a method for the experimental determination of the horopter, by Christine Ladd-Franklin; and the psycho-physic law and star magnitudes, by Dr. J. Jastrow.

Six Bulletins of the United States Geological Survey, Nos. 34–39, have been sent to us. The subjects are: on the relation of the Laramie Molluscan fauna to that of the succeeding fresh-water Eocene and other groups, by Dr. C. A. White; physical properties of the iron-carburets, by Mr. C. Barus and Mr. V. Strouhal; the subsidence of fine solid particles in liquids, by Mr. C. Barus; types of the Laramie flora, by Mr. L. F. Ward; peridotite of Elliott County, Kentucky, by Mr. J. S. Diller; and the upper beaches and deltas of the glacial Lake Agassiz, by Mr. W. Upham.

In a paper which has just been reprinted from the Transactions of the New York Academy of Sciences, Mr. J. S. Newberry maintains that the decorative ideas expressed in the monuments of the ancient inhabitants of Central America have a close resemblance to the carvings executed by the Indians of the north-western coast of America, and by the people of the Pacific Islands. "Hence," says Mr. Newberry, "I am inclined to believe, as has been suggested by Baldwin, that the seeds of this ancient civilization were brought from the East Indian Archipelago from island to island across the Pacific, and that finally reaching our continent, and prevented by the great and

continuous chain of the Cordilleras from further eastward migration, it slowly spread southward to Chili, and northward to our western territories."

FIVE years ago the increase of wolves in France had become so serious that the Government found it necessary to raise the awards for killing them. In 1882, 423 wolves were killed; in 1883, 1316; in 1884, 1035; in 1885, 900; and in 1886, 760. The awards are now 200 francs for the killing of a wolf which has attacked human beings; 150 francs for one in young; 100 francs for a male wolf, and 40 francs for a cub.

IN the current number (vol. i. No. 4) of the Journal of the Pekin Oriental Society, the well-known scholar Dr. Edkins writes on local value in Chinese arithmetical notation. The principle of local value is used in Chinese commerce, strokes being used instead of special symbols for 1, 2, 3, &c., the relation of the strokes to each other showing the value of the symbol. The abacus, with its upright strings and balls, is only a help to calculation, and does not contain any new principle. Dr. Edkins describes calculating slips which have been in use in China from the most ancient times. It is curious to notice that the principle of local value adopted by the Chinese was from left to right as with ourselves. The slips here mentioned, in which local value played an important part, had been in use fourteen centuries and probably more, when in the fourteenth century the abacus was introduced. Dr. Edkins assigns the origin of the principle of local value to the Babylonians, for several reasons. The first Chinese example known to us is dated B.C. 542, while in the sixteenth century B.C. the Babylonians could extract the cube and square roots of numbers: the Hindoos do not seem to have been proficients in mathematics at so early a date as B.C. 542; so that the probability is the principle of local value in arithmetical notation found its way to China through the Phœnician traders. The Chinese, in fact, acquired it where they acquired a knowledge of the clepsydra, the dial, astronomy, and astrology.

THE creation of provincial museums in Eastern Siberia is progressing very favourably. The example given by the Minusinsk Museum has been followed at Yeniseisk, and will be followed at several other towns. The Minusinsk Museum has now 4000 speeimens of plants, 2000 of animals, and 1500 of minerals. The anthropological department has numerous models of huts and houses of the Russian and native population. The archæological collection is especially interesting; it contains 218 implements of the Stone Age, 1260 of the Bronze Age, and 1850 of the Iron Age. There is, moreover, a collection of implements used in, and produced by, local domestic trades. The whole is described in a good catalogue. Last year the Museum was visited by 8000 persons.

TWO bones which were found some time ago at Pitchery Creek, Central Queensland, attracted the attention of several persons interested in science. They were lately exhibited at a meeting of the Royal Society of New South Wales, and Mr. Etheridge explained that they were portions of the vertebral column of an extinct reptile, *Plesiosaurus*. From the transverse elongation of the portions preserved, the bones partook more of the facies of the Plesiosauri of the Cretaceous group than of those found in the Lower Mesozoic deposits.

DR. SCHWERIN, who was despatched last year by the Swedish Government to the Congo, in order to ascertain whether that place was suitable for the establishment of a Swedish colony, and to make scientific researches, has returned to Sweden with good results. He also reports having made an interesting discovery at the mouth of the river, viz. the marble pillar or *padro* erected here by Diego Cam in 1484, the first Portuguese traveller who reached the Congo. The Portuguese were in the habit of raising such *padroes*, bearing the arms of Portugal, in prominent places on the West Coast of Africa, when taking possession of territory, and it was known that one had been erected by Cam at the mouth of the Congo, but it was believed that it had been destroyed. However, Dr. Schwerin, having worked out a theory of his own, searched for this ancient monument some 6 miles further inland than the position indicated on English charts, viz. Point Padro, and here he found it. Dr. Schwerin is preparing an exhaustive account of his work on the Congo, at the expense of the Swedish Government.

AN electric railway for the dinner-table is one of the recent achievements of French ingenuity (*La Nature*, October 29). It makes the presence of servants unnecessary. The train, which runs on a line along either side of the table before the diners, consists of a platform pivoted on two bogies, one of which carries the motor, while the other is merely a supporting truck. The expenditure of electric energy is but slight, and the train is said to be thoroughly under control of the host.

THERE has been much speculation as to how the ancient Egyptians managed to erect their enormous monoliths, sometimes 100 feet in height and weighing hundreds of tons. An interesting recent article in the *Revue Scientifique*, by M. Arnaudeau, offers the explanation that water was employed. Round the obelisk, lying horizontally, with the base towards the pedestal, was raised a circular inclosure, of height equal to that of the monolith. This latter had pieces of wood, or other floats, fitted to it, especially at the upper part; so that when water was brought into the inclosure, the obelisk rose gradually to the vertical. The process may be simply imitated by introducing the end of a screw nail into a piece of cork, putting it in a basin, and then introducing water.

THE pulverizing of minerals for analysis often consumes much time, requiring, as it does, great care. A mill for the purpose, constructed on the model of the wet mill in porcelain work, has been recently brought before the Berlin Chemical Society by Herr Zulkowsky (*Berichte*, October 24). The grinding-surfaces are both agate, and the circular runner, on a vertical axis, has a sector cut out of it, and one edge of this rounded. The mill is driven by water-power, a pressure of two to three atmospheres being sufficient.

IN a paper on colour-blindness, contributed to vol. v. Part 2, of the Proceedings of the Bristol Naturalists' Society, Prof. W. Ramsay suggests that the particular defect which causes colour-blindness may lie in the brain, not in the eye. Certain persons, he points out, are incapable of judging which of two musical tones is the higher, even when they are more than an octave apart. Yet such persons hear either tone perfectly; the defect is not one of deafness. "It must be concluded," says Prof. Ramsay, "that in such a case the brain is the defaulter. And it may equally well be the case that the inability to perceive certain colours is not due to a defect in the instrument of sight—the eye, but to the power of interpreting the impressions conveyed to the brain by the optic nerve. If this is the case, the problem is no longer a physical one: it falls among those with which the mental physiologist has to deal."

A SUPPLEMENTARY mail has just arrived from Iceland, from which we learn that in spite of the ice which has blockaded the eastern and northern shores of the island there has been a good summer and autumn inland, and the harvest has been above the average. However, on the east coast the ice did not disappear till the middle of September, and on the north coast it has not remained so long as during this summer since 1846, and even then the ice-masses were far smaller than this year. In spite of this the weather has been unusually warm inland. Dr. Th. Thoroddsen, the well-known Iceland explorer, has been travelling in the north-western peninsula this year. The fisheries have entirely

failed this autumn on the north and east coast, on account of the drift-ice, but they have been good on the south coast.

THE additions to the Zoological Society's Gardens during the past week include a Mongoz Lemur (*Lemur mongoz* ♂), an Olive-gray Lemur (*Hapalemur olivaceus*) from Madagascar, presented by Capt. J. Bonnerville; an Anubis Baboon (*Cynocephalus anubis*); an Angolan Vulture (*Gypohierax angolensis*) from West Africa, presented by Capt. Augustus Kent; a Peregrine Falcon (*Falco peregrinus*), European, presented by Mr. J. G. Keulemans; a —— Scops Owl (*Scops* ——) from Baltcitan, Himalayas, presented by Mr. John H. Leech, F.Z.S.; two Rough-scaled Zonures (*Zonurus cordylus*) from Robben Island, South Africa, presented by Mr. W. K. Sibley.

OUR ASTRONOMICAL COLUMN.

THE ASTRONOMICAL SOCIETY OF FRANCE.—The science of astronomy has become so increasingly popular in France within the last few years, and Frenchmen have done so much to aid its progress that there is ground for wonder that hitherto there has been no Society in France explicitly devoted to its interests. Such a Society, on lines very similar to those of our own Royal Astronomical Society, has at length been founded, and its first meeting was held on October 12, M. Camille Flammarion, the President, being in the chair. MM. Paul and Prosper Henry, General Parmentier, and M. E. L. Trouvelot are the Vice-Presidents; and MM. Gérigny and Gunziger the Secretaries; whilst Dr. Lescarbault, M. G. Secretan, and M. Ch. Trépied are amongst the members of Council. At the first meeting, M. Trouvelot read a paper on a remarkable double shadow of the first satellite of Jupiter, observed by him in 1877 when at Cambridge, U.S.; and M. Ch. Mousette exhibited a fine photograph of a sunspot, and some large-scale photographs of portions of the solar spectrum.

THE LICK OBSERVATORY.—The *Sidereal Messenger* for the current month states that Mr. E. E. Barnard, of Nashville, Tenn., and Mr. J. M. Schaeberle, of the Ann Arbor Observatory, both well known for their cometary discoveries, have been appointed as astronomers at this Observatory.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *NOVEMBER* 20–26.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 20

Sun rises, 7h. 29m.; souths, 11h. 45m. 44·8s.; sets, 16h. 3m.: right asc. on meridian, 15h. 42·4m.; decl. 19° 42′ S. Sidereal Time at Sunset, 20h. 0m.

Moon (at First Quarter November 22, 11h.) rises, 12h. 15m.; souths, 16h. 43m.; sets, 21h. 18m.: right asc. on meridian, 20h. 41·0m.; decl. 17° 52′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian h. m.	Declination on meridian ° ′
Mercury..	6 48	11 22	15 56	15 18·5	16 56 S.
Venus ...	3 1	8 48	14 35	12 44·5	3 22 S.
Mars ...	1 6	7 35	14 4	11 30·9	5 1 N.
Jupiter ...	6 34	11 10	15 46	15 6·7	16 36 S.
Saturn ...	20 53*	4 40	12 27	8 36·1	19 0 N.
Uranus...	3 27	9 2	14 37	12 58·2	5 31 S.
Neptune..	16 8	23 49	7 30*	3 47·4	18 10 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Nov.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
20	B.A.C. 7202	6	16 46	18 2	124 279
20	B.A.C. 7209	6½	17 35	18 32	159 259
20	19 Capricorni	6	20 30	21 30	114 347

Nov.	h.	
21	1	Mercury at least distance from the Sun.
21	6	Neptune in opposition to the Sun.
24	10	Venus in conjunction with and 1° 6′ north of Uranus.
26	23	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·3	81 16 N.	Nov. 22,	1 48 *m*
R Arietis	2 9·7	24 32 N.	,, 25,	*m*
λ Tauri	3 54·4	12 10 N.	,, 25,	5 40 *m*
U Ophiuchi	17 10·8	1 20 N.	,, 22,	5 27 *m*
			and at intervals of	20 8
β Lyræ	18 45·9	33 14 N.	Nov. 22,	6 0 *m*
η Aquilæ	19 46·7	0 43 N.	,, 24,	22 0 *M*
S Sagittæ	19 50·9	16 20 N.	,, 22,	19 0 *m*
			,, 25,	19 0 *M*
δ Cephei	22 25·0	57 50 N.	,, 20,	21 0 *M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
The Andromedes	24°	44° N.	Very slow; with trains.
Near μ Ursæ Majoris.	155	40 N.	Swift; streaks.

GEOGRAPHICAL NOTES.

THE Owen Stanley Range of New Guinea, which has been so long known at a distance, has at last been ascended. Mr. E. H. Martin, of Queensland, in August last, reached the summit of the range, which he found to be 13,205 feet high. He reports the north side of the range to be a paradise with great tree-ferns, palms, and other magnificent tropical vegetation. Mr. W. R. Cuthbertson, the leader of the Australian Geographical Society's Expedition, started for Port Moresby on July 20 last, with Mr. G. Hunter as interpreter. Mr. Cuthbertson has not yet succeeded in ascending to the highest point of the Owen Stanley Range, as he intended, but ascended Mount O'Bree, 10,240 feet.

IN No. xi. of *Petermann's Mitteilungen*, Dr. Paulitschke describes Captain Stuart King's journey into the country of the Ejssa and Gadaburssi Somali, some 70 miles to the south of Zeyla, in 1886. The paper is accompanied by a map. Dr. von Jhering and P. Langhans conclude their long and elaborate memoir on the southern colonial region of Rio Grande do Sul. Dr. Hans Schinz, who has been so long in the Lake Ngami region, criticizes severely Mr. Farini's narrative of his journey to the Kalahari Desert, the conclusion being very adverse to the trustworthiness of Mr. Farini's narrative. Perhaps the most important contribution to this number is a beautiful map of the Russo-Afghan frontier region, based upon the work of Colonel Holdich's Commission. It is remarkable that while Colonel Holdich's work is carefully locked up in the India Office as "confidential," so far as English geographers are concerned, it should be accessible to the geographers of other countries.

HERR KRAUSE has returned to the German settlement of Togo, on the Gold Coast, from his journey from Salaga through Dahomey. He has collected from 600 to 800 specimens of plants and seeds, a large number of insects, and numerous specimens of prehistoric articles found between Mosi and Timbuktu.

THE principal paper in the third part of this year's *Bulletin* of the Paris Geographical Society is an account of a journey made in 1881 by Count de Chavagnac, from Fez to Morocco, north-east to Mekenessa, and eastwards across the numerous wadis that run south into Wed Mellouja, and as far as Ajda. There is also a paper containing a good deal of useful information, and accompanied by an excellent map, on the ports of Tonquin, by M. J. Renaud. M. Datreuil de Rhins concludes his useful summary of our knowledge of Eastern Tibet.

THE session of the Royal Geographical Society began on Monday, with a paper on Siam, by Mr. J. McCarthy, Superintendent of Surveys in Siam. Mr. McCarthy has been at work for seven years on the survey of Siam, and some of the results he described in his paper, and embodied in the map by which

it was illustrated. After indicating the position of Siam in the Malay Peninsula, the author went on to say that on the west is a chain of mountains which runs in an unbroken range to Singapore, the southernmost limit of the Malay Peninsula; some of its peaks between Burmah and Siam rise to a height of 7000 feet, while one peak in the Malay Peninsula reaches 8000 feet. On the east there is another range of mountains which forms the grand watershed of all the rivers that flow into the Gulf of Tonquin and Chinese Sea on the one hand, and the Meinam Kong on the other. There are peaks in this range that reach even 9000 feet above mean sea-level. Besides these ranges there is another which breaks away from the western range from a point north-east of Chingmai, and forms the watershed between the Meinam and Meinam Kong valleys. In this range, at the source of the eastern branch of the Meinam, are famous salt-wells. The salt is procured at depths varying from 35–45 feet—in these land-locked countries as valuable as money. The greater part of the valley of the Meinam Kong and the Meinam is flat, diversified by isolated hills, and broken and jagged ridges of limestone mountains. The most important river, though not the largest, is the Meinam Chau Phraya. It is the Nile of Siam, a good rice harvest very much depending on whether the river overflows its banks or not. The eastern branch of the river is specially known for the numerous crocodiles which yearly carry off some victims. Two other rivers converge towards the Meinam, the Mei Klong and Bang Pla-Kong. All these rivers are connected by canals, rendering communication easier in a country where roads are conspicuous by their absence. The Meinam Kong is the largest river, and flows through the northern and eastern parts of the kingdom, receiving the waters of many large affluents; but the channel of this mighty river is so blocked with large rocks and cataracts, that its navigation is very difficult, and in some parts impossible even for native craft. Mr. McCarthy then went on to describe some of his journeys in detail, especially the one to the north-east frontier, which led him through scenes of surpassing beauty, and during which he opened up much new ground.

THE Arctic land seen by Sannikof eighty years ago has been seen again by the Expedition of MM. Bunge and Toll from the northern extremity of the Kotelnyi Island. The Great and Small Liakhov Islands, the Thadeus Island, and New Siberia have also been visited by the Expedition, which has returned with rich zoological, botanical, and geological collections. Throughout the summer of 1886 the ice on the Siberian coast did not move from the shores, and the hunters said that the sea had not been clear from ice since the *Vega* Expedition.

METEOROLOGICAL NOTES.

WE have lately been subjected to a series of storms which fortunately in the British Islands is not of very common occurrence. The storm of October 30, which was noticed in NATURE a few days after its occurrence, had scarcely left our shores before a fresh disturbance was approaching us from off the Atlantic, and by the evening of Monday, the 31st, another gale was blowing in Ireland, and during the night this storm extended to all parts of the British Islands. The central area of low barometer readings, which primarily occasioned the renewal of disturbed weather, kept to the westward of our coasts, but the Daily Weather Chart of November 1 shows that two secondary disturbances had been formed, one having its centre in the St. George's Channel, and the other over the Bay of Biscay. The very severe gale experienced in the south-west and west of England on November 1 was due to the former of these, the storm area passing during the day slowly up the Irish Sea. The fall of the barometer for this gale amounted to 1·02 in. at Pembroke in fourteen hours, from 6 p.m. 31st to 8 a.m. 1st; and at Lyme Regis the wind attained the velocity of 83 miles an hour between 7 and 8 o'clock in the morning. Another disturbance skirted to the westward of Ireland on the evening of the 2nd, and during the following day, causing southerly gales in many parts of the country, the barometer standing below 29 inches over the whole of the United Kingdom. On the evening of the 3rd another subsidiary was formed in the Irish Sea, and subsequently passed over the north of England, causing gales and disturbed weather in parts adjacent to its path. Before the expiration of the week a fresh disturbance was shown in the west, and on Saturday, the 5th, the barometer was again falling: the force of the wind, however, was not severe, although it blew a fresh gale in places. It will be seen from this notice that no fewer than five distinct storms were experienced in seven days, and in each case the wind was accompanied by heavy rain.

THE Meteorological Council have published Part I. of the "Hourly Readings" for 1885 (January to March) made at their self-recording observatories, together with the daily means, daily maxima and minima, and the daily range for pressure and temperature. Hourly values have now been issued in either lithographed or printed form since 1874, and afford valuable data for discussion in various ways, although the hourly means are not calculated. Corrections are given for reducing the barometric observations to mean sea-level. In connection with these observations it may be mentioned that the Meteorological Institute of the Netherlands lately published an interesting paper by M. Schokker on atmospheric disturbances studied by means of the hourly readings issued by the Meteorological Office and elsewhere; he traced on charts the positions of depressions for various hours, and showed that many phenomena which are clearly traceable from hourly observations are entirely lost sigh of on charts giving only one or two hours a day. He also quoted instances where timely warning of storms could have been given, which were not possible from the usual observations received by telegraph.

THE Hydrographic Office of the United States calls special attention to a new form for reports of storms, fog, ice, and derelicts, issued for the use of trans-Atlantic steamers. This form replaces those hitherto issued by that Office and the Signal Service, and the information thus collected is immediately utilized in preparing the telegrams sent daily to France by the United States Signal Service for the benefit of westward-bound vessels. Captains of trans-Atlantic steamships are requested, in the interest of navigation, to send in prompt and complete reports. No doubt British ship-owners will instruct their officers to co-operate in this enterprising experiment, as this country has at least equa interest with others in the safety of Atlantic navigation.

A DISCUSSION on the distribution of cloud over the eastern part of the North Atlantic, by Dr. W. Köppen, will be found in the *Annalen der Hydrographie und Maritimen Meteorologie* for October. The author points out that the cloud-conditions over the Atlantic are now fairly well known from the publications of the Meteorological Office (Captain Toynbee's great work for nine 10° squares), and the six 10° squares discussed by the Deutsche Seewarte. Dr. Köppen gives a table showing the mean monthly cloud from 20°–50° N. and from 10°–40° W., and the number of observations used, for every 5°, showing that, with regard to longitude, in the months January to April the cloud decreases north of 10° N. as we approach the shores of Africa and Europe, while in the other months this does not hold good. South of the equatorial calm-belt, May has the least cloud towards the east of the district, and in the months September to February the least cloud is towards the west. The differences of the amount of cloud with regard to latitude are much more decided, and these changes are shown on a map of equal lines of mean cloud, on the same plan as was adopted by the author in his discussion of the rainfall (NATURE, vol. xxxvi. p. 617). He also compares the cloud and rainfall curves for the yearly period, and draws attention to their marked difference in the zone of 15°–27° N. lat. While the tropical summer rains cease between 15° and 20° N., the summer maximum of cloud extends as far as 25° N. In the same way the winter maximum of cloud only extends southwards to 25° N., while the rain extends to 17° N. In these latitudes the minimum of cloud falls in the autumn, and the minimum of rain in spring. Only from 15°–17° to the southwards is the amount of cloud in spring less than in autumn, while northwards of 27° N. both minima coincide in the late summer season. Between 15° and 20° N. the end of the long dry season, lasting from February to June, is very cloudy. The author also compares his cloud-results with those obtained by M. Teisserenc de Bort from independent data (NATURE, vol. xxxvi. p. 15), and on the whole expresses himself satisfied at the agreement between the two investigations.

WE have the pleasure of recording the commencement of the publication of meteorological observations in the *Boletin de Estadistica* of Puebla (Mexico). Observations taken three times a day are published for several stations, and monthly means for several others. The stations are generally at great altitudes above the sea.

PART 2, vol. iv. of the Indian Meteorological Memoirs contains a very lucid discussion of the disastrous storm which visited Orissa in September 1885, and whose centre was at False Point on the 22nd, drawn up by Prof. A. Pedler. This storm is of considerable meteorological interest from several points of view: viz. the rapidity of its formation; its smallness, the diameter at the part of greatest wind-force being only from 100 to 200 miles; its enormous fierceness; particularly as it approached the land; and the decided indraught towards the centre as opposed to the circular theory; the extraordinary low reading of the barometer, 27·135 inches, being recorded at False Point at 6h. 30m. a.m. of the 22nd. The reading at 8h. p.m. of the 21st was 29·622 inches, thus giving a fall of 2·487 inches in 10½ hours. This is the lowest pressure ever recorded in a storm in the Bay of Bengal, and in fact is the lowest on record for any part of the world.

THE Journal of the Scottish Meteorological Society for the year 1886 contains a large amount of useful information, and testifies to increased activity, both observational and experimental. Among the various papers, all of which are of the highest importance, may be specially mentioned, (1) an address by the Hon. R. Abercromby on the modern developments of cloud knowledge (see NATURE, vol. xxxv. p. 575); (2) discussions on the winds and rainfall of Ben Nevis, and on a peculiarity of the cyclonic winds of the mountain, which has an important bearing upon weather forecasting, viz. the outflow of the wind from the cyclone when the centre is north or east of Ben Nevis towards an anticyclone or area of high pressure somewhere in an opposite direction. The prevalent wind on the Ben is north, while south-east and west-south-west are secondary points of maxima. Compared with the winds of other stations in the north of Scotland and Ireland, the wind curve is quite different. The year divides about equally into cyclonic and non-cyclonic periods. The most frequent cyclonic wind is south-west; next to this comes north, apparently due to the cyclones passing to the north of Ben Nevis. The relative frequency of the winds in non-cyclonic periods is quite different: while north still retains its place as a maximum point, the most frequent wind is south-east. In the curve for the whole year tha west-south-west winds are chiefly due to cyclonic winds, south-east to non-cyclonic, and north to both systems. In both systems the north-west wind is wettest while it blows, and the east is driest. The south-east winds, which are generally west at low levels, are the driest on Ben Nevis, with the exception of the east winds. The total amount of precipitation for the year was nearly 108 inches; the wettest month was November, 14·6 inches; and the driest February, 2·8 inches. The journal also contains an interesting account of the biological work of the Scottish Marine Station, and the results of observations at the Northern Lighthouse Station, at the stations connected with the Medical Department, including observations in Iceland, Faroe, and Uruguay, and at fifty-five stations established by the Scottish Meteorological Society, and well distributed over the country.

GEMS AND ORNAMENTAL STONES OF THE UNITED STATES.

ON Saturday, October 22, an evening lecture on this subject was delivered by Dr. A. E. Foote, of Philadelphia, in the Trophy Hall of the American Exhibition. The speaker was introduced by Mr. F. W. Rudler, the President of the Geologists' Association.

Dr. Foote remarked that hitherto mining for gems in the United States had been of a very desultory character, being principally carried on in connection with mica and other mines. The emerald and Hiddenite mines of North Carolina and the tourmaline mines of Maine are the only ones which have been worked systematically. The gems peculiar to America are *chlorastrolite*, *zonochlorite*, and *Hiddenite*. Chlorastrolite, or green star-stone, was discovered by Prof. J. D. Whitney, of the United States Geological Survey, about forty years ago. The only place where it is found is Isle Royale, Lake Superior. The island, belonging to the State of Michigan, forty miles long and five miles wide, and about twenty miles from the mainland, is composed of amygdaloidal trap, in the almond-shaped cavities of which the gem principally occurs. This green stone has a radiating structure, and shows a beautiful *chatoyance* similar to cat's-eye and other fibrous minerals.

Zonochlorite is a green-banded stone, similar to chlorastrolite in composition, discovered by Dr. Foote at Neepigon Bay on the north shore of Lake Superior. The full description was published in the Transactions of the American Association for the Advancement of Science in 1872. Its hardness is about 7; it takes a very high polish, and if it could be found in sufficient quantities would undoubtedly be extensively used.

Hiddenite is a green variety of the well-known species spodumene. A yellow variety from Brazil has been cut as a gem for many years. The green variety has been known for about seven years, and is fully as beautiful, and valued as highly, as the diamond. It occurs in connection with emeralds in North Carolina. Of *gold quartz* about £28,000 worth is sold annually. Most of this comes from California, where it is not only used as a gem, but in the manufacture of various ornaments.

Although the flexible sandstone, the reputed gangue of the diamond in Brazil, is found in mountain masses in North Carolina and other States, no very large diamonds have as yet been discovered. Many small ones are recorded from California, North Carolina, Virginia, and elsewhere. The largest was found at Manchester, near Richmond, Virginia, and weighed 23¾ carats in the rough and 11$\frac{11}{16}$ carats cut. Prof. Whitney states that the largest found in California was 7¼ carats. Rubies and sapphires have been found in the rock in the corundum mines of North Carolina, and Mr. C. S. Bement has an uncut green one in his collection that would give 80 to 100 carats' worth of good stones, one of which would probably weigh 20 carats. The largest red and blue crystal weighs 312 pounds, and belongs to Amherst College. The best sapphires are found in the placer mines of Montana. Asteriated corundums are found in Pennsylvania and elsewhere.

About £2200 worth of quartz or rock crystal is mined annually. The best localities are Hot Springs (Arkansas), North Carolina, New York, and Virginia. A portion of a mass that must have weighed over 40 pounds was recently received from Alaska, that cut a hand-glass 3 inches by 5. Rock crystal is frequently dug up in the prehistoric mounds, and was used by the medicine-men and others for telling future events. Amethysts are found in very fine specimens in Pennsylvania, Georgia, Texas, and the Lake Superior region. From the latter region they are very remarkably lined, some specimens showing "phantom crystals" equal to the Hungarian. Near the Yellowstone National Park and in the chalcedony forests of Arizona are tree-trunks, some of which are 100 feet long, mineralized by the action of silicated waters. Some of these trees are still standing upright, others, having fallen, bridge deep chasms. The once hollow cavities of some are lined with amethyst, others with agate. The Arizona agatized or jasperized wood shows the most beautiful variety of colours of any petrified wood in the world. Probably the most remarkable locality anywhere for smoky quartz, or cairngorm stone, is Pike's Peak, Colorado. Here it is found in a graphic granite associated with Amazon stone, which also makes a very beautiful green ornamental stone. The rutilated quartz, or Cupid's arrows, is found in remarkably fine specimens in North Carolina. Perhaps the most remarkable mass is one 7 inches by 3½, now in the collection of the Academy of Natural Sciences of Philadelphia. The crystals of rutile are about the size of knitting-needles. Some of the North Carolina rutile has been cut, furnishing brilliant gems, closely resembling carbonado. The rutile, geniculated till it forms a perfect circle or rosette, from Magnet Cove, Arkansas, is often mounted and worn as a gem. While opals are found at many places in the United States, they do not rival those of Queretaro in Mexico. Here are found not only the "milky opals that gleam like sullen fires in a pallid mist," but fire opals and almost every other variety known. Rhodonite, in specimens suitable for polishing, is found in Massachusetts and New Jersey. At the latter locality were obtained the finest crystals ever seen. The garnets from New Mexico and Arizona are superior to the "Cape rubies" from South Africa; and from Alaska the most beautiful crystals ever seen, in a setting of gray mica schist, have recently been obtained.

The New Mexican turquoise is mined to the value of about £700 annually. It has recently been described very fully by Prof. Clarke, Curator of the Mineralogical Department of the National Museum, and is especially interesting as being the material from which the "chalchihuitls," or most sacred images of the Aztecs, were made. The Indians still regard it as a lucky stone.

Labradorite, lately so popular for gems and ornamental stones, is found in many localities. The tourmalines of Maine are

probably the finest in the world. Here are found the Oriental sapphire, ruby, and emerald, in perfection.

Topaz has recently been found at Pike's Peak, Colorado, in large quantity. Some masses weighed 2 pounds each; and very fine clear white stones have been cut, weighing from 125 to 193 carats.

Among ornamental stones should be mentioned a very beautiful variety of serpentine from Maryland, called verd antique, which is being largely used in the interior decorations of the Philadelphia Court House. Another variety, resembling jade, is the green williamsite from Pennsylvania. Alabaster of various colours abounds in many localities; and marbles, some as beautiful as the Mexican onyx, are found in nearly every State. The malachite and azurite, jet, and many other gems of minor importance were briefly described.

THE OCTOBER METEOR-SHOWER OF 1887.

THE display of Orionids has been recently observed at this station with greater success than has attended my efforts in any previous year. This shower has not, perhaps, exhibited such richness as it did in 1877, but the present occasion has been more favourable as regards the conditions; the moon being absent from the morning sky, and a period of tolerably clear weather occurring just at the important time.

In all, I numbered ninety Orionids between October 11 and 24, and the radiant-point during this period exhibited a stationary position amongst the stars. The shower has this year met with rather a formidable rival in a bright display of forty-five meteors from a radiant at 40° + 20° close to ε Arietis. I have witnessed the latter stream in several preceding years, though not in such conspicuous strength, and have particularly referred to it in the *Monthly Notices*, vol. xliv., pp. 24–26, as furnishing many bright fireballs at this season.

It will be convenient to arrange my new observations in a tabular form:—

Date 1887 Oct.	Period of Observation. h. h.	Real Duration.	Meteors seen.	Orionids.	Arietids.	Radiant of Orionids.
11	7½ to 12½	4½	30	1	2	91° + 17°
12	8½ ,, 12½	4	31	2	1	
13	10 ,, 12	2	16	1	1	
14	9½ ,, 16¾	7	75	1	10	
15	7 ,, 8½; 10½ ,, 17	7½	86	17	7	91° + 16°
17	8 ,, 12½	4½	29	3	3	90° + 15°
19	13 ,, 15	1½	19	10	—	90½° + 15½°
20	10 ,, 15½	5¼	61	22	9	90° + 14½°
21	9 ,, 16	6¼	76	23	7	92° + 14°
23	12½ ,, 14	1½	13	1	3	— —
24	12 ,, 14½	2¼	23	9	2	91° + 16°
11 nights		46¼	459	90	45	91° + 15°

The 16th and 22nd were overcast, and on the 19th and 23rd the observations were much obstructed by clouds. It is noteworthy that I only recorded one Orionid on October 14 during a watch of seven hours, though on the following night this shower supplied seventeen meteors.

The radiant-point of the October meteors has long been accurately known. Prof. A. S. Herschel observed it with great precision on October 18, 1864, and October 20, 1865, and found the centre at 90° + 16°, and 90° + 15° respectively, in those years. All the best of later determinations have agreed closely with these results, and it will be noticed that my value for the present year, as given above, is nearly identical with them. In further confirmation I may mention that Mr. David Booth, of Leeds, observed more than sixty shooting-stars during a watch of five hours, from 10½h. to 15½h. on the night of October 20 last, and saw twenty-four Orionids which gave a sharply-defined radiant at 90° + 16°.

One of the principal objects of my late observations was to ascertain whether the radiant centre of this stream showed any displacement of position on successive nights, and similar to that affecting the Perseids of August—a peculiarity which I first pointed out in NATURE, vol. xvi. p. 362. But the radiant of the Orionids has (when the small, unavoidable errors of observation are allowed for) quite failed to exhibit any change of place relatively to the contiguous stars. It appeared to maintain an absolutely persistent position 1° north of the star ξ Orionis. My observation on October 15 placed it at 91° + 16°, and nine nights later, viz. on October 24, I found the meteors were radiating from exactly the same focus. In 1877 and 1879, October 15, I derived the radiant at 92° + 15° and 93° + 17°, and in 1878, October 22, I fixed it at 92° + 14°. A comparison of all these values renders it sufficiently obvious that there is no visible displacement in the position of the Orionid radiant during its active display from October 11 to October 24. And there is a high degree of probability that the point is stationary during the whole period of the shower's sustenance from about October 9 to October 29; but I have never secured many paths and been enabled to get a good radiant near the limiting epochs of its display, when it is extremely feeble.

Mr. Booth, at Leeds, has been carefully observing numbers of meteors during the past few months, and a searching comparison of his results with those obtained at Bristol during the progress of the Orionid shower has shown that several of the same meteors were observed at both stations. Three of these are typical members of the October display, whilst three others had their origin in the minor systems which are so plentifully distributed over the sky at this season of the year. The computed heights and paths of these six meteors are:—

Date 1887 Oct.	Hour G.M.T. h. m.	Mag.	Height at appearance.	Height at disappearance.	Length of real path.	Radiant point. ° °	Inclination to horizon. °
13	10 25	1–2	69	·50	26	73+61	48
13	11 25	2–4	70	42	37	127+83	49
[14	12 5½	4–5	64	40	26	355+36	67½
15	14 48½	1½–3	89	61	39	87+15	46
20	11 45	4–4	106	[90	34	87+21	28½
20	12 55	1–1½	92	53	70½	87+13½	33½

The three last in the list were Orionids, and they appear to have been observed at somewhat greater elevations in the atmosphere than is usual. The 4th magnitude meteor of October 20, 11h. 45m., was no less than 106 miles high at its first appearance, over a point near Eversham, Kent, and the two observations are in perfect agreement in indicating these figures. The mean of the three Orionids gives 96 miles for the beginning points and 68 for the ending, and the average radiant comes out at 87° + 16°, which is 3° or 4° west of the usual position. But the average values deduced from so small a number of instances cannot have much weight as indicating accurately either the heights or radiant of the general body of the meteors forming this notable group.

The Arietids, which have developed into an important shower this year, traverse their paths with medium speed, and are rather conspicuous meteors, without trains or streaks except in exceptional cases. As to the Orionids, they move swiftly, and are accompanied in almost every instance with streaks. The latter will sometimes brighten up considerably after the nuclei of the meteors have died away. The more brilliant Orionids are fine flashing meteors, leaving streaks which are occasionally very durable.

The contemporary showers of the October epoch, though extremely abundant, are not marked by special activity, except perhaps in the case of the Arietids, already referred to. This year the following have been the best of the minor streams:—

Date.	Radiant. ° °	Meteors.	Appearance.
October 14–15	25+44	10	Slow, faint.
,, 14–21	54+71	12	Swift.
,, 14–21	105+22	12	Very swift, streaks.
,, 20–21	125+43	7	Very swift, streaks
,, 14–23	135+68	11	Swift.
,, 12–20	312+77	8	Swift.

Of these the most pronounced is at 105° + 22°, near δ Geminorum, which I also observed in 1877 and 1879. It has also been recorded as a prominent stream by Zezioli and others, and is identical with the Gemellids of Mr. Greg's catalogue (1876). It is chiefly a morning shower; its meteors are often brilliant, and regularly display the phosphorescent streaks which form so characteristic a feature of the Perseids, Orionids, and Leonids. The shower in the head of Ursa Major at 135° + 68° is also an active one at this epoch; I saw it in 1877 at 133° + 68°, October 2–19, and these appear to be the only two observations of it obtained hitherto.

W. F. DENNING.

ON SOME OF THE AFFINITIES BETWEEN THE GANOIDEI CHONDROSTEI AND OTHER FISHES.

THE group of Ganoidei Chondrostei has hitherto been regarded as one developed during the latest period of the history of the earth. Its structure is so different from that of other classes of fish that its relationship with them cannot be easily detected. The zootomic and embryological works of the last ten years, and especially the works of Zalensky in Odessa, Parker in London, Davidoff in Munich, and van der Wighe in Holland, have brought together many important facts as to the organization and development of these interesting animals, but the information provided by these writers is either fragmentary or not full enough, and long study and labour will be required before it can be satisfactorily summed up and completed.

During the last two years I have studied the anatomy of *Acipenser ruthenus*, the commonest representative of the Acipenseridæ to be found here; and although my work is far from being completed I may beg the reader's attention to some interesting facts, which must, I think, be taken into consideration by those who try to settle the question as to the relationship of the Ganoidei Chondrostei to this or to that group of fishes.

We may begin with the teeth of these fishes, as an indication of great value, which served to distinguish this group from other Ganoidei. Teeth have been found in *Polyodon folium*, a member of the Ganoidei Chondrostei, inhabiting the rivers of North America; it has been thought that they might also be found in *Psephurus gladius* of the River Yang-tse-kiang, in China; and Prof. Zalensky has found them in *Acipenser ruthenus*, at the age of from three weeks to three months. I have had the good fortune to find teeth in almost all the Ganoidei Chondrostei of the different ages that I have examined, but they were palatine teeth, not mandibular or maxillary teeth. I have discovered and studied the palatine teeth in a two-months-old sterlet; in an *Acipenser stellatus* of from seven to eight months old; in a *Scaphirhynchus kaufmannii* from Amu-Daria of a year old; in a grown *Scaphirhynchus fedschenkoi* from Sir-Daria; and in full-grown *Polyodon folium*.

The relationship of the dimensions of the snout of fish to the age at which teeth can be found is very interesting. The long and flat snouted *Acipenser stellatus* has teeth to a more advanced age than the short and narrow snouted sterlet; the teeth of a wide-snouted Scaphirhynchus not attaining a good development but are preserved until maturity; the spade-snouted Polyodon preserves its teeth during the whole of its life. In all the other representatives of Acipenser and Scaphirhynchus can be found at any stage traces of palatine teeth in the shape of two similar prominences, which, by their structure, can be distinguished from the surrounding parts of the mouth.

This dependence of a long preservation of teeth on the development of the snout of Ganoidei Chondrostei, together with the geographical distribution of these fish, shows the greater antiquity of the tooth-preserving kinds of Scaphirhynchus and Polyodon, than of the Acipenser. Species of one kind, inhabiting such widely separated water-reservoirs as the Aral Sea and Mississippi, or the Yang-tse-kiang and the continental rivers of North America, must be representatives of very old forms, remains of former fauna; their having, at a mature age, organs that do not serve them, but which merely remain as an inheritance from former periods, is a confirmation of their supposed antiquity—a conclusion drawn from zoogeographical observations.

The structure and development of the dorsal shields, which, in the case of Acipenseridæ, spread all along the dorsal surface, from the back edge of the head down to the dorsal fins, may also, I think, help us to discern affinities between Ganoidei Chondrostei and other fish. The first to pay attention to these shields, and to suppose they were an embryonal dorsal fin, was Prof. Zalensky. About the same time Prof. Goethe described a similar fin of a six-weeks-old sterlet, hinting, by the way, that the dorsal shields might be compared with the dorsal rays of a fossil fish, Cœlacanthus. I have succeeded in investigating the dorsal shields of a two-months-old sterlet, and in making a whole series of cross sections, and I have arrived at the conclusion that Zalensky's and Goethe's suppositions are fully established by facts. Indeed, between the shields spreads a membrane, in which can be seen the same horny rays that are generally seen in developing fins of fish; right and left of each dorsal shield there is a muscle, traces of which can also be found under the shields of grown sterlets. At last, having made cross-sections of oxidized dorsal shields of grown sterlets, a canal could be perceived in them. These canals are particularly well seen in Scaphirhynchus, as an older and a better representative of the original type.

Knowing that Dr. Günther in his excellent book on ichthyology places the Acipenseridæ and Cœlacanthi next to the Polypteroidei, I availed myself of the offer of Prof. Bogdanoff, Director of the Moscow Museum, and my teacher, to let me examine the only dry specimen of *Polypterus senegalensis* that was in the Museum. Comparing the numerous small fins spreading all along the back of Polypterus, there being a great and wide front bone-ray, and the others being thin and horny, I became convinced of their complete similarity to the dorsal shields of a young sterlet and to the membranes which connect them.

In the wide bone-ray of Polypterus a ray channel could also be discerned, and the rays of the membrane that spreads behind the wide ray were also horny, like the rays of the membrane of an embryonal fin of a sterlet. This brought me to the conclusion that the ancestors of both the Acipenseridæ and the Polypteroidei had not only a back fin, but also well developed front dorsal fins, with great bone-rays and smaller horny rays, and were, perhaps, nearer to each other than their present descendants.

A study of other organs, especially those in young Acipenseridæ and Scaphirhynchus, convinces me that there is a closer relationship between the Ganoidei Chondrostei and the Polypteroidei than has hitherto been supposed. It is well known that the conus arteriosus of Acipenser is distinguished from the same organ of the Polypterus and Lepidosteus by a much smaller number of transversal rows of valves. In young sterlets I have found, besides developed valves, undeveloped folds lying between the valves. In place of such undeveloped valves, in the case of grown fish, as for example in a specimen of *Acipenser huso* which I dissected, and which was about 10 feet long, an unevenness and roughness of surface are noticed. The air-bladder, which in Lepidosteus and Polypterus partly resembles the lung of Dipnoi, when attentively studied in the Acipenseridæ does not appear to be so well adapted to its new functions. Its coatings include many ramifications of vessels, the histological structure of which is so similar to the structure of the coatings of the digestive organs that it is much easier to recognize their relative layers than in those of other fishes, where the air-bladder is fully adapted to its functions. The ductus pneumaticus, in young sterlets especially, is very wide; a two-months-old sterlet has it of almost the same width as an œsophagus, and the food of the small fish, consisting mostly of forms of Cladocera and Ostracoda, and also of statoblasts of Polyzoa, especially Alcyonella, fills the cavity of the air-bladder like the cavity of the stomach.

Though the brain of these fishes has been well investigated, yet in its organization one finds much that is interesting. For example, the cerebral hemispheres of the prosencephalon of Scaphirhynchus proved to be more similar to the hemispheres of Dipnoi and Lepidosteus and Protopterus, than to those of Acipenser. The lateral layers are turned upward, so that the upper portion of the hemispheres proved to consist, not of one pallium, as in Acipenser, but also of the coating of the cerebrum. The epiphysis cerebri, being a changeable organ, proved to vary even in the limits of the genus Acipenser. Thus, its front end in *Acipenser sturio* reached as far as the line connecting the two lower nostrils, forming an angle of nearly 28° with the surface of the brain, whereas in *Acipenser ruthenus* the epiphysis forms an angle of almost 80°, and becomes a much shorter organ. In some sterlets the end of the epiphysis cerebri went through the cranium, and was only covered by the bone shields of the exterior coating. Scaphirhynchus had the epiphysis less changeable and more similar to the epiphysis of other Ganoidei and Dipnoi. In other respects the brain of Scaphirhynchus also proved to have a closer resemblance to the other Ganoidei than to the Acipenser. Thus, its valvula cerebelli and lobi inferiores are more developed than those of a sterlet, and even remind one of the brain of Amia and its near relatives Teleostei.

Notwithstanding the scantiness of the facts stated here, I indulge the hope that they may add something to the means at our disposal for the settlement of the relationship between Ganoidei Chondrostei and other Ganoidei.

NICHOLAS ZOGRAFF.

Moscow, 20/8 September 1878.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. x. No. 1 (Baltimore: Johns Hopkins University, 1887).—The number opens with the concluding lecture (the 33rd) of Prof. Sylvester's course on the theory of reciprocants, in which is investigated the differential equation of a cubic curve having a given absolute invariant S^3/T^2. A supplemental "lecture" is supplied by the reporter (Mr. Hammond) from the lecturer's surplus material: this "constitutes probably the most difficult problem in elimination which has been effected up to the present time." All admirers of Prof. Sylvester's brilliant genius will be glad to have the fine presentment of his features which accompanies this number.—Algebraic surfaces of which every plane section is unicursal in the light of n-dimensional geometry is devoted to a proof and to illustrations, by Mr. E. H. Moore, Jun., of a theorem due to Picard, viz. "Les seules surfaces algébriques dont toutes les sections planes sont unicursales sont les surfaces réglées unicursales et la surface du quatrième degré de Steiner."—Mr. Morgan Jenkins, in a paper on Prof. Cayley's extension of Arbogast's method of derivations, presents in a simplified form results given by the elder mathematician in a memoir printed in the Phil. Trans. (read December 1860).—Properties of a complete table of symmetric functions, by Capt. P. A. Macmahon, R.A., establishes some remarkable features of a tabulation set forth by Mr. Durfee in vol. v. of the *Journal*.—Oskar Bolza, in his article on binary sextics with linear transformations into themselves, considers those binary sextics which remain unchanged (or are only changed by a constant factor) for certain linear transformations of the variables.—Prof. Cayley follows with the sequel to his memoir on the transformation of elliptic functions (vol. ix.), and Prof. Woolsey Johnson closes the number with the symbolic treatment of exact linear differential equations.

Bulletin de la Société des Naturalistes de Moscou, 1887, iii.—Comparative osteology of the penguins and its bearing upon the classification of birds, by Dr. M. Menzbier (in German; with a plate).—The Hessian fly, by Prof. K. Lindeman (in German).—Chemical composition of the Lipetsk mineral springs, by A. Kislakovsky. A series of chemical analyses has been undertaken in order to ascertain how far the composition of the springs is liable to undergo changes at different times of the year. The admixture of water flowing from sweet springs makes the amount of $FeCO_3$ to vary from 0·016 to 0·032, and from 0·008 to 0·025 in different springs.—On the increase in the number of thunderbolts and its causes, by J. Weinberg (in German).—Enumeration of the vascular plants of Caucasus, by M. Smirnoff (in French). This fourth paper of the introduction which the author has written to precede his enumeration of plants discusses the following important subjects: evaporation, limits of perennial snow in Caucasia and neighbouring highlands, the present and ancient glaciers of Caucasus, and the geology of the country since the later Tertiary. The twelve botanical regions into which the author divides Caucasia are given with short characteristics of their physical features. On the whole the paper is a most valuable contribution to the knowledge of Caucasus.—List of plants growing in the province of Tamboff, by D. Litvinoff (continued).—*Otiorhynchus turca*, Steven, an enemy of the vine-tree, by E. Ballion. It has been found at Novorosiysk, on the east coast of the Black Sea, and must have immigrated from Asia Minor and Syria.

SOCIETIES AND ACADEMIES.

London.

Mathematical Society, November 10.—Sir J. Cockle, F.R.S., President, in the chair.—Prof. Sylvester, F.R.S., being incapacitated by an accident to his leg from attending in person to receive the De Morgan Medal, awarded him by the Council in June last, deputed Mr. J. Hammond to represent him. The President, after a few remarks eulogistic of Prof. Sylvester's numerous discoveries, presented the medal to Mr. Hammond, who made a felicitous reply.—The Treasurer (A. B. Kempe, F.R.S.), after having read his Report, announced to the meeting that the Society's application to the Privy Council for the grant of a charter had failed.—The following were elected to act as the Council for the ensuing session:—President: Sir J. Cockle, F.R.S. Vice-Presidents: Dr. J. W. L. Glaisher, F.R.S., Prof. Hart, and Lord Rayleigh, Sec.R.S. Treasurer: Mr. A. B. Kempe, F.R.S. Hon. Secs.: Messrs. M. Jenkins and R. Tucker. Other Members: Messrs. A. Buchheim, E. B. Elliott, A. G. Greenhill, J. Hammond, J. Larmor, C. Leudesdorf, Captain P. A. Macmahon, R.A., S. Roberts, F.R.S., and J. J. Walker, F.R.S.—The following communications were made:—On pure ternary reciprocants and functions allied to them, by E. B. Elliott.—On the general linear differential equation of the second order, by the President.—On the stability of a liquid ellipsoid which is rotating about a principal axis under the influence of its own attraction, by A. B. Basset.—On modular equations and geometry of the quartic, by R. Russell.—The differential equations satisfied by concomitants of quantics, by A. R. Forsyth, F.R.S.—On the stability or instability of certain fluid motions (ii.), by Lord Rayleigh, Sec.R.S.—Notes on a system of three conics touching at one point, by Dr. Wolstenholme.

Geologists' Association, November 4.—Mr. F. W. Rudler, President, in the chair.—The President delivered the opening address of the session, entitled "Fifty Years' Progress in British Geology." He drew a picture of the state of geology in 1837, and contrasted it with that in 1887. The principal questions discussed were the old controversy between the Catastrophists and Uniformitarians, the development of Palæozoic geology, the origin of the Drift, and the antiquity of man. In recent years the warmest discussions have referred to the Archæan rocks and to the Glacial Drift. Attention was directed to the debt which geology owes to engineering, especially to the development of our railway system and to artesian borings. The sub-Wealden exploration was explained, and a Jubilee boring suggested. Deep-sea exploration was touched upon. Turning to petrology, its low condition in 1837 was pointed out, and its recent development traced to the introduction of microscopic methods of research. The history of palæontology was briefly sketched, special attention being called to the work of the Palæontographical Society. Improvements in the Geological Department of the British Museum were noticed, and reference was made to the history of the Geological Survey and the Museum of Practical Geology. In conclusion, it was pointed out that by a happy accident the meeting of the International Geological Congress in London next year will coincide with the centenary of the foundation of British geology—the original publication of Hutton's "Theory of the Earth" in 1788.

Chemical Society, November 3.—Mr. William Crookes, F.R.S., President, in the chair.—The following papers were read:—Note on the atomic weight of gold, by Prof. T. E. Thorpe, F.R.S., and Mr. A. P. Laurie.—The ni teraction of zinc and sulphuric acid, by Mr. M. M. Pattison Muir and Mr. R. H. Adie.—Note on safety-taps, by Mr. W. A. Shenstone.—Note on Guthrie's compound of amylene with nitrogen peroxide, by Dr. A. K. Miller.—The dehydration of metallic hydroxides by heat, with special reference to the polymerization of the oxides and to the periodic law, by Prof. Carnelley and Dr. James Walker, University College, Dundee.—The bromination of naphthalene β-sulphonic acid, by Mr. G. Stallard.—The constitution of the three isomeric pyrocresols, by Dr. W. Bott.—Preliminary note on certain products from teak, by Mr. R. Romanis.

Paris.

Academy of Sciences, November 7.—M. Janssen in the chair.—On a paradox analogous to the St. Petersburg problem, by M. J. Bertrand. The paper deals with the doctrine of probabilities, and shows that, if a gambler plays under conditions involving all but inevitable ruin, equity requires the remotely contingent prize to be infinite.—On the state of the potassa in plants, in the soil and vegetable humus, and on its quantitative analysis, by MM. Berthelot and André. These studies have been undertaken to determine how far the potassa present in plants and arable land is in the condition of salts soluble in water, or of insoluble salts capable or not of resisting the action of attenuated acids. The researches are in continuation of those already described connected with the analysis of the soluble and insoluble carbon present in the soil, and of the nitrous compounds in their various forms of nitrates, free ammonia, &c.—Inquiry into the two fundamental principles of the accepted doctrines regarding cerebral dualism in voluntary motions, by M. Brown-Séquard. In continuation of his recent communication on this subject, the author here advances facts and arguments, some of which go directly to show that each half of the encephalon may independently serve for the production of voluntary movements in both sides of the body, while others

tend to overthrow the fundamental principles of the views generally held regarding the part played by both hemispheres in producing voluntary movements. Several interesting manifestations are described, proving that the motor effects of cerebral irritations are in absolute contradiction to the current theories.—On the *Elasmotherium*, by M. Albert Gaudry. In connection with some remains of this extinct mammal recently found on the River Kinel in the government of Samara (Russia), and presented to the Academy by M. Paul Ossoskoff, some remarks are made by the author, who assigns to the *Elasmotherium* a position intermediate in size between the mammoth and *Rhinoceros tichorhinus*, his contemporaries. In his general structure he appears to have approached more nearly to the latter animal, the radius, tibia, cubitus, calcaneum, and some other bones presenting the closest resemblance to those of a gigantic rhinoceros.—On a geometric form of the effects of radiation in the diurnal motion of the stars, by M. Gruey. A number of propositions are here announced, whose further development and demonstration are reserved for a future number of the *Bulletin Astronomique*, where a full demonstration will be given of the theorem that, in a sidereal day the apparent position of a star describes a conic section round its true position.—On the internal temperature of glaciers, by MM. Ed. Hagenbach and F. A. Forel. The different temperatures determined by careful experiment in the Arolla glacier are explained by the varying pressure to which different parts of the glacier are subjected. The normal temperature below zero is shown to be the effect of pressure, which lowers the melting-point of ice, thus verifying in Nature facts already theoretically demonstrated by Sir W. Thomson and others, but hitherto studied only in the laboratory.—Remarks on the Gulf Stream, by M. J. Thoulet. Comparing his own observations made on board the *Clorinde* in 1886 with those of Mr. Buchanan during the *Challenger* Expedition, the author finds that the Gulf Stream is comparable to a river with a greater fall in its upper than in its lower reaches. A relatively steep valley separates it on the left from the United States current setting southwards from Newfoundland, while its more gently sloping right bank skirting the ocean presents a much broader expanse. Thus is explained the direction of the driftwood carried from America towards the north-west coast of Europe.—Researches on the distribution of temperature and of barometric pressure on the surface of the globe, by M. Alexis de Tillo. The author describes some general charts which he has prepared, based on the labours of M. Léon Teisserenc de Bort, and of Herr J. Hann, of Vienna, showing the mean isobars and isothermal lines for the year, and the months of January and July, for the whole world. For the general conditions of the terrestrial atmosphere he finds that, when the mean temperature $\left.\begin{matrix}\text{falls}\\ \text{rises}\end{matrix}\right\}$ within the limits of 1°·6 and 4°·7, the pressure $\left.\begin{matrix}\text{increases}\\ \text{diminishes}\end{matrix}\right\}$ to the extent of 1 millimetre.—On the metallic derivatives of acetylacetone, by M. Alphonse Combes. From the researches here described, the author concludes that this substance decomposes all the carbonates, even that of potassa; that it displaces the acetic acid of the acetate of copper, and even the hydrochloric acid; that it consequently acts as a strong acid on the metallic salts. Nothing, so far, distinguishes its action from that of a monobasic acid, although this function is clearly distinguished by certain properties of its salts from the acid function properly so called.—On the part played by the stomata in the inspiration and expiration of gases, by M. L. Mangin. From the experiments here described the author concludes generally that the stomata are indispensable for the circulation of the gases in aërial plants, the occlusion of these orifices bringing about a greater or lesser diminution in the exchanges of the respiratory gases, and a very considerable decrease in the exchanges of chlorophyllian gases.—On the invasion of *Coniothyrium diplodiella* in 1887, by MM. G. Foex and L. Ravaz. This organism, already observed in 1879 by Spegazzini in Italy, and in 1885 by Viala in the department of the Isère, has this year invaded an extensive region in the South of France. Whether it is a true parasite, or a saprophyte, or whether it assumes both of these characters according to circumstances, is a point which has not yet been decided.

Berlin.

Meteorological Society, November 1.—Prof. von Bezold, President, in the chair.—Dr. von Helmholtz discussed his most recent researches on the formation of mist under the influence of chemical processes, and laid stress at the same time upon the relation of his results to the phenomena of meteorology.—Dr. Sprung gave an account of observations made with thermometers attached to various barometers. During a comparison of the barometers from various stations with a normal barometer, the experiments being conducted in a cellar, he found that the thermometers showed considerable differences in their readings; their differences were still observed when the comparison of the barometers was made in a room at the surface of the earth, and the barometers were placed side by side in the same frame. The speaker was hence led to compare three thermometers, of which one was surrounded by a nickel-plated cylinder; the second was surrounded by a varnished cylinder, and the third had no covering at all. When placed near an open window the instrument with the nickel-plated covering registered the highest temperature, but when placed near a hot stove it recorded the lowest. The differences in reading varied at different times of the year, and amounted to several degrees. In practice these differences of the thermometer-reading can have no influence on the reading of the barometer, since it may be assumed that the mercury in the barometer has always the same temperature as that indicated by the thermometer, and that the reading of the barometer is reduced to zero.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Practical Treatise on Bridge Construction: T. Claxton Fidler (Griffin).—The Real History of the Rosicrucians: A. E. Waite (Redway).—Calendar of University College, Nottingham, 1887–88.—Totemism: J. G. Frazer (Black).—Animal Magnetism: Binet and Féré (Kegan Paul).—Living Lights: C. F. Holder (Sampson Low).—L'Homme avant l'Histoire: Ch. Debierre (Baillière).—The Flora of Howth: H. C. Hart (Hodges, Figgis, and Co.).—Lectures on Bacteria: A. De Bary; second improved edition, translated by H. E. F. Garnsey, revised by I. B. Balfour (Clarendon Press).—The Final Results of the Triangulation of the New York State Survey (Albany, N.Y.).—Catalogue of the Moths of India, part 1: Cotes and Swinhoe (Calcutta).—China in America; a Study in the Social Life of the Chinese: S. Culin (Philadelphia).—Catalog der Conchylien-Sammlung, Vierte Lief (Paetel, Berlin).—Fishery Barometer Manual: R. H. Scott (Eyre and Spottiswoode).—Folk-lore Journal, vol. v. part 4 (Stock).—Proceedings of the Royal Society of Edinburgh, No. 123.—Journal of the Royal Agricultural Society, October (Murray).—Archives Italiennes de Biologie, tome viii. fasc. iii. (Loescher, Turin).

CONTENTS.

THURSDAY, NOVEMBER 24, 1887.

CHARLES DARWIN.

The Life and Letters of Charles Darwin, including an Autobiographical Chapter. Edited by his Son, Francis Darwin. In Three Volumes. (London: John Murray, 1887.)

TO write a biography is a task which is almost a proverb for difficulty. It is no easier for a relative than for a stranger, because, if a more intimate knowledge of the details lightens the labour, affection is apt to warp the judgment, and checks perfect freedom of expression. In the biography, however, of Charles Darwin, there was no temptation to reticence, no need for firmness. His was a life, simple, noble, blameless. Still, this very simplicity and unostentatious rectitude presented their own difficulties. After the long and interesting voyage in early manhood, it was a life singularly uneventful, a life of patient labour, one long struggle against sickness. Thus its record when written might readily have been unexceptionable, but dull.

This cannot be said of the Life of Charles Darwin. It will take its place, I venture to predict, with Boswell's Life of Johnson, Lockhart's Life of Scott, Stanley's Life of Arnold, and the comparatively small number of biographies which have attained to first-class rank in literature. Mr. Francis Darwin has made excellent use of the materials at his disposal. These were considerable. They consisted of a short sketch written in his later years by Charles Darwin himself, for the information of his family, and of a large number of letters. Mr. Francis Darwin had, in addition, the special advantage of having shared in the labours of his father during the last eight years of his life.

The chapters written by the editor are of the highest interest and value, but as far as possible the story is told by Charles Darwin himself; the letters being merely strung together by occasional explanatory paragraphs, which form a connecting thread. Selection must have been no easy task, but it has been well done, and numerous as are the letters and large as is the book one almost wishes they had been more and it had been larger. Yet Darwin was hardly what most people would call a good letter-writer. His letters were often written hurriedly, and bear the marks of hasty composition; but there is not seldom a terseness of phrase, and always a vigour of expression, which makes them peculiarly attractive. These letters, too, are thoroughly characteristic of the man. They breathe the quenchless energy, the "dogged" endurance, the hidden tenderness, the sweet reasonableness, the imperturbable equanimity, of his nature; and they show, on rare occasions, that capacity for indignation without which a character so amiable might have degenerated into weakness.

The autobiographical sketch tells us the particulars of Charles Darwin's early life. Born at Shrewsbury in 1809, the son of a physician in large practice, and grandson of the well-known Dr. Erasmus Darwin, author of "Zoonomia," Charles Darwin was educated at the grammar-school of that town, under Dr. Butler, one of its most noted head masters. Neither in childhood nor in boyhood does he appear to have given promise of exceptional powers, though the taste for collecting manifested itself at a very early age, and he was obviously more thoughtful and determined to understand things thoroughly than the average boy. But the school system of education which, as usual then, was wholly classical, did nothing to bring out the special powers of his mind. Indeed, he was once even rebuked by the head master for wasting his time on such a useless subject as chemistry. He passed, in short, through Shrewsbury school as a well-conducted boy of ordinary ability, perhaps a little below the average. "A good lad, but not quick or particularly studious," would probably have been the verdict of his masters.

After leaving Shrewsbury, Darwin studied for a couple of years at the University of Edinburgh, attending medical lectures, with the idea of adopting that profession. But for the surgical side of it he already felt a disinclination; and unfortunately for him, as he relates, the dreariness of the lectures on anatomy indirectly deterred him from the practice of dissection, which would have been a useful training for his later life. He made, however, friends, who aided in developing his love for natural history, though he tells us that the dullness of the geological lectures produced in him "the determination never as long as I lived to read a book in geology, or in any way to study the science." A resolve, happily, before long rescinded.

From Edinburgh, Charles Darwin went to Cambridge. Here he entered at Christ's College, where his elder brother, Erasmus, was already a student. There was then an idea that, as he clearly had no strong taste for medicine, he should be ordained. As he says, "Considering how fiercely I have been attacked by the orthodox, it seems ludicrous that I once intended to be a clergyman." Darwin had at no time of his life any tendency to superstition, otherwise one might observe that extremes in religious opinion are not so wide apart as the remark just quoted seems to imply. At the same time he tells us that a German phrenologist had declared that he "had the bump of reverence developed enough for ten priests."

He brought little classical knowledge from Shrewsbury, and left much of that behind at Edinburgh; he had no taste for mathematics, and natural science was not then recognized in the curriculum at Cambridge. So he read little, and took an ordinary degree. Thus he doubtless appeared to be wasting his time, and accuses himself of so doing. But one can see that the groundwork was being laid for the future. He acquired friends, some of like tastes; his interest in natural history increased, and was developed by the opportunities which the district afforded, especially for collecting beetles, then the ruling passion. His health, too, at this time appears to have been good; he was an active pedestrian and a keen sportsman, enjoying society, and not without a love of music.

But the friendship of Henslow was probably the greatest boon which he owed to Cambridge. Acquaintance soon ripened into steadfast friendship, and the wider knowledge and formed habits of the older man produced, as Darwin gratefully admits, the best possible influence on the younger. Through Henslow also, shortly after Darwin had taken his degree, the offer was made to join the *Beagle* as naturalist, which may fairly be called the turning-point of his life. It is interesting to see how

evenly balanced the reasons for and against acceptance then seemed to be, and how nearly the offer was refused. He wished to go, but Dr. Darwin, his father, for various reasons—among them the fear that so long a voyage would unsettle his son for life—was opposed to the plan. Chiefly through the influence of Darwin's maternal uncle, Mr. Josiah Wedgwood, the father's objections were overcome. Capt. Fitzroy, however, who was a disciple of Lavater, was nearly refusing his services because of the shape of his nose, which was not sufficiently indicative of determination and energy! Considerable delay arose from one cause or another, but the vessel finally sailed from Plymouth on December 27, 1831.

Though Henslow was Professor of Botany, it was impossible to know him without being infected with geology; so by this time the resolution against that science had been rescinded, and Darwin had even accompanied Sedgwick on one of his journeys in North Wales. The tale of the work during the voyage of the *Beagle* has been told in the well-known volumes; but we have here a series of letters which record many incidents of the journey, and indicate the development of the writer's mental powers and the thoughts which were already beginning to quicken into life. It is amusing to read that now the new love is sometimes stronger than the old. "But geology carries the day; it is like the pleasure of gambling. Speculating on first arriving what the rocks may be, I often mentally cry out, 'Three to one Tertiary against Primitive,' but the latter have hitherto won all the bets." Later there is a conflict even in his geological preferences. "I am quite charmed with geology, but, like the wise animal between two bundles of hay, I do not know which to like the best, the old crystalline group of rocks or the softer and fossiliferous beds." But notwithstanding these mental "lovers' quarrels," notwithstanding the serious drawback of incessant suffering from sea-sickness, and one grave illness of an unknown nature, a large number of specimens and a wonderful store of observations were accumulated in almost every department of natural history, which served as the foundation for the great superstructure to which his life was devoted.

On Darwin's return to England in 1836, he oscillated for a time between Cambridge and London, working at the results of his voyage as hard as his health, now seriously impaired, permitted. In 1839 he married, and after residing in Gower Street till the autumn of 1842, moved, in the hope of benefit from country air, to a house which he purchased at Down, in Kent, and in which the remainder of his life was spent.

Here, after the immediate fruits of the *Beagle* voyage were given to the world, he began to develop the great idea of which the germ had been sown and quickened during his wanderings. One chapter describes how "the foundations of the Origin of Species were laid" between the years 1837 and 1844; another narrates its growth. In 1856, partly in consequence of Lyell's advice, the book was begun for which such long preparation had been made, and by the month of June 1858 about one-half was written. It was, however, on a much greater scale than that which has now become classic in England,—"three or four times as extensive." Then suddenly all was changed by the receipt of a manuscript from Mr. Wallace, from his distant sphere of work in the Malay Archipelago, which coincided so exactly with his own views that, as Darwin writes to Lyell, "if Wallace had my manuscript sketch written out in 1842, he could not have made a better short abstract." The story of this interesting episode, so honourable in every respect to all concerned, is told, chiefly by means of letters, in one chapter of the book. In these days, when too often the stream of scientific life is ruffled by miserable personal squabbles about priority in some trifling discovery, it is well to learn how men acted whose hearts were as large as their intellects were great.

The result of the simultaneous announcement of the hypothesis thus independently framed was that the plan of the larger work was abandoned, and the "Origin of Species by means of Natural Selection," or "An Abstract of an Essay on the Origin of Species through Natural Selection," as the author would have preferred to call it, was published in 1859. It is needless to epitomize the story of its reception by the public—of the opposition which it encountered—of the storm which it aroused—of its ultimate triumph: all this is admirably told by Prof. Huxley in a chapter which he has contributed to the work.

Of all the accusations brought against Darwin, perhaps the most unreasonable was the frequent one that he had "abandoned the true Baconian method." I do not profess to be very familiar with the philosophy of Bacon; but if accumulating a mass of facts, co-ordinating them, and then drawing inductions, is not the true method of science, I do not know of any other; and this method inspires every chapter of the "Origin of Species."

In the correspondence which was written during the remainder of the author's life, occupying nearly half the work, we read of how the "Origin of Species" won its way, edition following edition, and of the series of later works and occasional papers which continued till within a few months of the end. This came rather suddenly, though in the fullness of years. For the last ten years of his life his health had appeared somewhat better than formerly, but in the early months of 1882 it gave repeated cause for alarm, and at last, on April 19, after a brief period of suffering, he ceased from that labour which only sickness had ever made a burden.

The quantity of work which he had accomplished is astonishing when its quality—always the best that could be done by the man—is considered. True, Darwin, though only to be called wealthy towards the end of his life, was always free from the necessity of bread-winning. But then—and what a terrible set-off this implies—"for nearly forty years he never knew one day of the health of ordinary men, and thus his life was one long struggle against the weariness and strain of sickness."

In one respect Darwin was *felix opportunitate vitæ*. He lived before scientific literature had attained its present overwhelming proportions. It is charming to read such a passage as this: "Geology is a capital science to begin, as it requires nothing but a little reading, thinking, and hammering." We might add, "with the mind of a Darwin,"—at least to get such wonderful results as in the "Geological Observations." If anyone of the present day is getting proud of what he may have done in petrology, I would prescribe Part I. of that work as a corrective. But if now we learn much from others, and gain much

from the perfection of our means of research, we are apt to lose in independence and vigour of mind, to say nothing of the time which is wasted in the weary wading through piles of periodicals, often with but little fruit as the result. Mathematicians know that solving problems gives a strength to the mind which cannot be obtained from the most careful study of book-work, and I have often ventured to think that to write the section on the "literature of the subject" as the last stage of a research is not so much to "put the cart before the horse" as it seems. Something, too, may be lost through the very perfection of the means of research in natural history: the mind may be tempted to dwell too much on details; and the over-careful study of these may lead men to miss the greater principles. Darwin was an observer, precise and minute as any, but it is interesting to note that he was always guided by a selective principle.

The greatest charm of the "Life" is that it draws so vivid a picture of the man himself—partly from the unconscious self-portraiture of his letters, partly from the tender touch of his son's hand, aided by the loving memories of other members of his family. Before us rises that tall, slightly stooping form, either walking with swinging though often feeble step, cloaked and staff in hand, along the "sand walk," or seated or reclining in that study which bore silent testimony to the orderly habits learnt in the tiny cabin of the *Beagle*; we see that massive forehead, those keen yet kindly eyes, shadowed by those overhanging brows, the sparse gray hair, the long gray beard, that winning smile which lit up those rugged features; we hear once more the kindly voice; but better still, there rises, fresh and ever instructive, the memory not only of one of the grandest intellects, but also of one of the noblest and truest natures, among the sons of men. Unruffled by carping criticism and virulent abuse, in silent dignity Charles Darwin laboured on, in the quiet consciousness of strength and the conviction that truth would at last prevail. No one can read the life of Darwin without feeling as if some healthful air from a better world had braced his moral fibre and nerved him for more earnest and more unselfish work.

Truly the last scene of all was a "Great Lesson." His family would have laid him in the quiet churchyard near his own home, but his fellow-workers in science desired and obtained that his grave should be made in Westminster Abbey. Some quarter of a century before that day many thoughtful men hesitated in accepting, or even opposed, the views which he had maintained; while the camp-followers and swash-bucklers of the religious world had discharged at him their volleys of vituperation. The one had been for the most part persuaded; the other had slunk away to growl in obscurity. Now, around that grave in the Valhalla of Britain, were gathered the leaders in literature and science, men of every rank in life, of every form of creed—from the most sincere Christian to the no less sincere Agnostic. Time had shown that there was no necessary opposition between the inductions of science and those deeper aspirations and beliefs upon which we must not here touch, and men who on such points felt deeply but differently from Charles Darwin came no less willingly than others to pay the last honours to one who was not only a great philosopher but also emphatically a good man.

T. G. BONNEY.

OUR BOOK SHELF.

A Treatise on the Integral Calculus. Part I. Containing an Elementary Account of Elliptic Integrals and Applications to Plane Curves, with numerous Examples. By Ralph A. Roberts, M.A. (Dublin: Hodges, 1887.)

MOST students, on taking up this book, will be disposed to ask, "Is there any room or necessity for another work on the Calculus just now? Is not Williamson up to date?" Mr. Roberts gives no sign, and so we are led to search out for ourselves a reason for the existence of the work, and a justification of the same. In his two previous books our author makes great use of elliptic functions, and a chapter is devoted to the discussion of them in the book before us, and, further, this fact is prominently noted on the title-page; hence we conclude that Mr. Roberts has had in view mainly the treatment of these integrals, and to make his treatise self-sufficient he has surrounded this special subject with such preliminaries and accessories as he deems suitable for the elucidation of his theme. The author has produced a capital book, for he writes with extreme care, and full knowledge and command of his subject. There appears to us to be in many parts a novel treatment—*i.e.* considering the matter in the light of English treatises on the Calculus—and there is copious illustration. There is large opportunity for practice afforded by the numerous examples inserted in the body of the work, and also at the end. Many of these are not intended, or at any rate are not suitable, for babes; they are strong meat for adults. There is an index and the usual table of contents.

Solutions to Problems contained in a Treatise on Plane Co-ordinate Geometry. By I. Todhunter, F.R.S. Edited by C. W. Bourne, M.A. (London: Macmillan and Co., 1887.)

THIS is not a work brought out with a rush, for the greater portion of the solutions were drawn up by the author fifteen years ago. To students using the text-book this will be a valuable companion, for Mr. Bourne has executed his task with care and ability. Geometrical as well as analytical solutions are given, and impart a pleasant feature to the book. For Mr. Bourne's sake we regret that the foundation is giving way, as few students now read the "Conics," for that fate is befalling it which the author himself says is "the fate of all academical text-books," viz. obscurity ("W. Whewell," vol. i. p. 24). Todhunter's own views respecting "Printed Solutions" are given in his "Essays" (p. 81). The exercises, however, will retain their utility as tests for ascertaining a pupil's grasp of the subject, in spite of the decay of the setting, and the "Solutions" we can recommend to students "after a vigorous effort has been made to obtain the solution without the book."

Lectures on Bacteria. By A. De Bary. Second Improved Edition. Authorized Translation by Henry E. F. Garnsey. Revised by I. B. Balfour, F.R.S. (Oxford: Clarendon Press, 1887.)

THIS work is in the main an abridgment of a number of lectures, some of which were delivered in a connected series as a University course, others as occasional and separate addresses. The author's aim is to set forth the present state of our knowledge respecting the objects included under the name of Bacteria. Having dealt with cell-forms, cell-unions, and cell-groupings, he describes the course of development of Bacteria, and then proceeds to discuss questions as to the position of Bacteria in the organic world, and as to their origin and distribution. A chapter on vegetative processes is followed by one on the relation of Bacteria to, and their effect upon, their substratum; and this leads to an account of the forms which excite fermentation, and of parasitic Bacteria. The remaining chapters are on the harmless parasites of warm-blooded animals, on anthrax and fowl-cholera, on the

causal connection between parasitic Bacteria and infectious diseases, especially in warm-blooded animals, and on diseases caused by Bacteria in the lower animals and in plants. The work will be very useful to all who may wish to obtain "a general view" of this important subject. It has been well translated, and we may note that a valuable list of publications relating to Bacteria is given at the end of the volume.

Mattie's Secret. By Émile Desmaux. (London: George Routledge and Sons, 1887.)

THIS book is evidently a French work very well translated into English. It is practically a book of delightful gossip, touching on many important points of science; while theoretically it is a pleasing story of a sister who devotes her time to her little brother driven from school and books by approaching blindness. The scientific part opens with the explosion of fire-damp, and goes on to the history of coal, how it is found, in what shapes; and then to the coal-mine itself, how the work is done, and the precautions which have to be taken. Next follows the history of diamonds, what they are, how they are shaped into different forms; and then comes graphite manufactured into pencils. The history of beer here follows, how it is prepared, and its use. Then the author explains torpedoes and torpedo-boats, how they are worked, and the method of launching the torpedo. Glycerine, dynamite, and gunpowder, their dangerous properties, and how they are prepared, are next referred to, and this is followed by an introduction to the phenomena of sound.

The book contains a hundred good illustrations showing the different scientific processes, and it is thoroughly interesting throughout.

The question arises whether fairy tales of science are not as interesting to children as fairy tales of the ordinary description. The author is evidently of this opinion, and we are inclined to agree with him. A. L.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Politics and the Presidency of the Royal Society.

YOUR leader of last week reminds me not a little of one of those days that begin somewhat brightly but end with a thunderstorm. As a Fellow of the Royal Society, I fail to see what our President has done to incur the reprobation of the writer of this article. I will take in order the two charges brought against him. Of these, the first is that he became President of the Victoria Institute, the second being that he has allowed himself to be returned as member of Parliament for his own University.

I do not fancy the writer means to assert that the predecessors of Prof. Stokes, more than one of whom held strong views connected with theology, virtually laid these aside during their tenure of office. At any rate, they gave ample expression to them both before and after this tenure. I gather that the objection rather is that Prof. Stokes, during his tenure of office, became President of another Society—the Victoria Institute. Now, Sir, I can see at once an objection to the President of the Royal Society being at the same time President of any of the other scientific Societies, such as the Astronomical, the Physical, the Chemical, or the Zoological. But I confess I fail to see any objection to his taking office in a Philosophical Society, which treats of subjects not really connected with science.

It may perhaps be thought that the Victoria Institute was deficient in breadth of view, and I think that until lately it was open to this objection. But I have reason to think that Prof. Stokes has infused into it a better spirit, and his admirable opening address to this Society has, if I mistake not, appeared in your columns.

In this address he acts entirely the part of a peacemaker, endeavouring to show that the conclusions of science cannot be held to come into collision with what may be regarded as the essential truths of the Christian religion. It is probable that a minority of Fellows of the Royal Society may believe that certain scientific doctrines have disposed of the claims of Christianity. Must, therefore, the President be precluded from going to church during his tenure of office? Unquestionably the going to church implies taking part in a public action about which the opinions of the Fellows could be divided.

It is in truth exceedingly difficult, if not impossible, to formulate a principle which shall extinguish the peculiarities of one individual while it leaves untouched the field around him representing the rights and privileges of others. The thing was tried once before in the time of Darius the Mede, but the results of the experiment were not of an encouraging nature.

I come now to the second and most important charge against our President. And here I confess I cannot help being a little amused at the writer's dread that the President will be hunted out of his scientific chair at all inconvenient hours, and driven to his seat at St. Stephen's by one of the Whips of the House. And I confess that I am equally amused at the thought of the Royal Society suffering the fearful political degradation depicted by the writer, entertaining as I do the most complete confidence in the integrity of this Society. I grant freely that under ordinary circumstances it is undesirable that the President of the Royal Society should enter the House of Commons. But these are no ordinary times, and we are now engaged in a struggle that means more than mere party warfare.

I do not wish to introduce politics into these pages, but I may state that in my opinion, and I think I may say in that of many Fellows of the Royal Society, the question just now is one between law and anarchy. But in a state of anarchy, what will suffer more than knowledge? In such a state will there be leisure to investigate—leisure even to dispute? And if this be so, should not Culture, which is more threatened than anything else, raise her voice in the Legislature and do what in her lies to prevent this deplorable consummation?

Surely it is this grave question, rather than any wish to represent the every-day interests of science, that has induced our President to enter the House. He has chosen to be an Englishman first, and a man of science afterwards. Who will blame him for this? BALFOUR STEWART.

THE able article which appeared in your last number (p. 49) under the title of "Politics and the Presidency of the Royal Society," raises a question of such magnitude, not only in its relation to science, but also to many other branches of human activity, that I trust to your courtesy and impartiality to give me an opportunity of briefly indicating some considerations calculated to lead to a conclusion different from that at which the writer of the article in question has arrived.

The Royal Society is composed of members who differ from one another in their views on political and many other subjects; nay, more, of men who differ from one another in their views on many scientific subjects. Their devotion to the advancement of natural knowledge is the common ground on which they meet.

The political opinions of our President are entirely unknown to us officially, and it may confidently be asserted that he is as highly esteemed and valued as President by those among us who may happen privately to differ from him widely in politics as by those who entertain similar political opinions to his own. His action in political matters concerns us as little as his opinions. No doubt we should be concerned if he were to undertake any duties of so engrossing a kind as to prevent him from fully discharging the duties of President, but we should be so equally if the additional work were not political.

It is conceivable, though we will hope not very likely, that at some future time the Society might have to return a member to the House of Commons; the Society would then be in a similar position to that in which several of our Universities are placed; the arguments used by the writer of the article might then be applied.

Our President cannot, however, be supposed to have entered

the House as the political representative of the Royal Society, for two reasons: first, because the Society has, in its corporate capacity, notoriously no political opinions to be represented; and, secondly, because we have not sent him to the House.

ALEX. W. WILLIAMSON.

High Pitfold, Haslemere, November 19.

"The Conspiracy of Silence."

THOUGH I am sorry to have misunderstood the meaning of the Duke of Argyll in his "Great Lesson," when I supposed him to accuse scientific men of virtually conspiring to suppress any unwelcome truth, I think I am not without excuse. Certainly I was not alone in the illusion, and I believe that many would even now say that the Duke of Argyll—in writing some of the passages which I quoted, and in using such phrases as "reluctant to admit such an error in the great idol," "slow and sulky acquiescence," "reluctantly, almost sulkily," "a grudging silence," not to quote any others—has certainly not expressed with felicity the lesson which he intended to inculcate. Further, in regard to the special instance brought forward by the Duke (that of Mr. Murray's paper) it does not appear to me that he has even now established his charge. The Duke states that he has seen a letter, written by the late Sir Wyville Thomson, most strongly urging Mr. Murray to withdraw the paper which he had sent to the Royal Society of Edinburgh. The Duke further tells us candidly that no reason is alleged in the letter. Hence, Sir Wyville Thomson's motive is a matter of inference only. I hope I shall not give offence to my friend Mr. Murray if I suggest that it may have been different from that which the Duke supposes. In 1877, so far as I can ascertain, Mr. Murray had not had much practice in writing papers. There is an art in this, which we have to learn by practice and the kindly criticism of our manuscripts by friends. As the best meat may be spoiled by an inexperienced cook, so the best material may be damaged by an inexperienced author. Sir Wyville Thomson would naturally feel very sensitive about any communications bearing the names of members of the *Challenger* Expedition, for if among its first-fruits had been a paper unsatisfactory either as to style or arrangement, yet controverting the deliberate conclusions of those hardly less well qualified to judge, a spirit of criticism and of distrust as to the thoroughness of the work of the Expedition would have been aroused. Of course this is an hypothesis only, which I trust Mr. Murray will forgive me for making, but I can assure him that I am conscious of my own youthful imperfections (not to mention the mistakes of maturer years), and I submit that it is at least as good as the Duke's, and more charitable to the memory of Sir Wyville Thomson.

In regard to the new case which the Duke of Argyll brings forward, and with which he connects my name, he is not quite accurate in his facts and is wrong in his inference. Mr. Guppy's paper was not "refused" by the Geological Society of London. The President has the power in certain cases, and under certain conditions, to refuse to put down for reading a paper written by a Fellow. I did not exercise that power. The Council, after a paper has been read, can refuse to print it. As Mr. Guppy's paper was never read, obviously this did not happen. Probably the circumstances were as follows,—I say probably, for I have no distinct recollection of them. Mr. Guppy's paper may have been sent, as is often done, for an informal expression of opinion as to whether the paper seemed suitable for the Society's consideration. In such case it would be shown either to one of the secretaries or to the President, and the opinion, favourable or otherwise, communicated to the author, who would then be free to act as he thought best. Now, if Mr. Guppy's paper was identical with that printed in the Proceedings of the Royal Society of Edinburgh (vol. xiii. p. 857) I have no doubt that my answer was to this effect: that it contained so much matter which belonged rather to natural history than to geology that I thought it was likely to suffer much excision before it was printed in our Journal, especially at that time, and was more suited for a Society of a wider scope than our own. I have again referred to the paper, and, without entering upon its merits, of which I am fully sensible, am still of opinion that, while it is in its place in the Proceedings of a Royal Society which includes all branches of science, it would have to be considerably abridged to fit it for those of a Geological Society. Of course that is only my opinion, but after full ten years' experience, eight of them as an officer, on the Council of the Geological Society of London, I may claim some knowledge of the principles on which that body acts. Moreover, at that time the Society was suffering from a falling off in revenue, with no corresponding decline in the number of papers which it was invited to publish. This I knew had compelled the Council to exceptional strictness. The difficulties of the Society were indeed so considerable that I commented on them in my address on quitting office in 1886, expressing at the same time my own view as to how they should be met. But though, as I have said, I have no clear recollection of the circumstances, I can speak positively of one thing, that if in any way I discouraged Mr. Guppy from communicating his paper it was not because I "smelt a heresy." It is something quite new for me to stand accused of being a prompt suppresser of heresies. My orthodoxy has not always been considered unimpeachable among the clergy, and surely my scientific papers are not generally on the side of "established views."

To conclude, the Duke still—and this is our special complaint—treats the matter rather according to ecclesiastical than to scientific methods. He is fully persuaded of the excellence of Mr. Murray's hypothesis, and considers it to be "one of those discoveries in science which are self-luminous," and "must carry conviction to all." Very well, but there are some people, not very few in number, who do not share his opinion. He cannot understand that our doubts can be due to anything else but "prepossession," which has prevented our minds from being "alive to the breadth and sweep of the questions at issue." I humbly reply that this is not the case; that we claim to exercise the right of private judgment, and decline to submit to any pope, from whatever part of the United Kingdom he may issue his Bull.

T. G. BONNEY.

Instability of Freshly-Magnetized Needles.

YOUR reviewer objects to a statement in my "Theory of Magnetic Measurements," to the effect that freshly-magnetized needles give untrustworthy readings for several minutes after magnetization (see NATURE, vol. xxxvi. p. 316). In reply to his statement that this is contrary to experience, I wish to say that it is not contrary to my experience. In working with two 8-inch needles I continually observed this phenomenon for years, and it was so marked that I could not feel satisfied to omit the precaution which the critic condemns. I know of one other observer who has had a similar experience with another needle. My needles were not very hard, and perhaps this may have had to do with the phenomenon.

It is not desirable to make any reply to criticisms, even though they seem not quite fairly taken, but it ought to be suggested that those who are unable to apply general formulæ to a special form of instrument after they have been shown how to apply them to a similar instrument might perhaps meet with more success in some other line of business.

FRANCIS E. NIPHER.

IN the passage to which Prof. Nipher refers I contrasted what seemed to me the excessive precautions prescribed in the directions for obtaining the dip with a rather rough-and-ready method of manipulation elsewhere suggested by him. That the magnetic axis of a piece of steel may shift is possible. My criticism was directed to the question as to whether, as a matter of experience, such a shift is a cause of error of practical importance in the determination of the dip. It would, therefore, be interesting if Prof. Nipher would publish the details of the observations on which his conclusion is based, so that the extent to which a measurement of the inclination may be rendered untrustworthy by not waiting for some minutes after magnetizing the needle may be in evidence. Meanwhile it may be well that I should define my own views on the matter.

On looking through the observations made in the magnetic survey of Missouri, which Prof. Nipher is conducting, I find that the dips obtained with different needles vary widely. Thus, taking the last Report to which I have access, in which the work of the year 1881 is described (Trans. Acad. Sci. St. Louis, vol. iv. No. 3, p. 480), the dip was determined with two needles at fifteen stations. At seven of these the difference between the results obtained by the two needles was equal to, or greater than, 4'. At one station it was 24'·8, and at others 17'·2, 11'·7, 9'·4, and 8'·9 respectively. If these are examples of trustworthy readings (and from their publication we must suppose that they are so), and if the differences obtained when the observations are untrustworthy on account of the shift of the magnetic axis are greater

than these, Prof. Nipher's experience is totally at variance with my own.

During the last four years, Dr. Thorpe and I, assisted at some Scotch stations by Mr. A. P. Laurie, have made about three hundred independent determinations of the dip. The observations have been made in the course of a magnetic survey of the United Kingdom at various stations, in all weathers, and without any delay after magnetization. We have used two sets of $3\frac{1}{2}$-inch needles, and have made determinations with two needles at nearly all the stations. In no single case does the difference between the results obtained with two needles amount to 4′. In three or four cases only does it exceed 3′, while differences of 2′ are relatively rare. Thus in forty-six Scotch stations (for which alone the results are fully tabulated) the difference exceeded 2′ in nine cases only. Mr. Welsh, in the survey of Scotland, recorded in the Report of the British Association for 1859, and the Rev. S. Perry obtained results in which the discordance between the two needles was of the same order of magnitude as in our own work.

If, therefore, Prof. Nipher refers to differences comparable with those exhibited by his published observations, they are contrary to the experience of observers working with better constructed instruments. If he refers to errors smaller but observed with an instrument with which a discordance between the two needles of from 10′ to 24′ can be tolerated, I should doubt if his apparatus is suitable for the elucidation of the point. If he is in possession of good evidence that, in the case of needles for which the maximum difference between observations made without delay after magnetization is not greater than 4′, the accord between them is improved by delay, the matter is no doubt of interest for observatory work. My own experience has been chiefly gained in the field, and I can only say that I have never noticed anything which led me to suspect such a cause of error.

It is, however, capable of proof that the improvement can be but small, as results obtained in a laboratory and without the precaution Prof. Nipher insists on agree nearly to the limit to which the instrument can be read.

This can be illustrated from the observations made by Dr. Thorpe and myself at Kew for the purpose of testing our survey instruments. At first we employed only a circle by Dover, No. 74. The following observations were made with it in the magnetic house by Mr. Baker, the Chief Assistant, and ourselves :—

Date, 1884.	Observer.	Needle.	Dip.	Mean.
June 24	Baker	1	67° 36′·8	67° 36′·6
		2	36·5	
July 18	Thorpe	1	67 36·1	67 36·0
		2	36 0	
,, 19	Rücker	1	67 36·5	67 36·3
		2	36·1	

These results were about 2′ lower than those obtained by Mr. Baker with the Kew instrument about the same time, but whatever the cause of this may have been they certainly do not convey the idea of instability.

Lately we have again compared No. 74 with the Kew instrument and with Dover No. 83, which belongs to the Science and Art Department. Thus six needles (two belonging to each instrument) were used. I quote the results, not as in any way extraordinary, but as types of the accuracy usually obtained by competent observers with good instruments :—

Date, 1887	Observer.	Instrument.	Needle.	Dip.	Mean.
Sept. 30	Rücker	Dover 74	1	67° 35′·4	67° 35′·1
			2	34·8	
Oct. 11	Thorpe	Dover 83	1	67 33·9	67 34·4
			2	34·9	
,, 13	,,	,,	1	67 36·0	67 36·0
			2	36·1	
,, 18	,,	,,	1	67 34·2	67 34·2
			2	34·2	
,, 19	Rücker	Dover 74	1	67 35·0	67 34·2
			2	33·4	

Mr. Baker's observations with the Kew instrument are again (as is shown below) a little higher than those obtained with Dover's circles.

Date, 1887.	Needle.	Dip.	Mean.
Sept. 22	1	67° 35′·3	67° 35′·6
	2	36·0	
Sept. 24	1	67 37·6	67 37·5
	2	37·4	
Oct. 25	1	67 38·8	67 37·9
	2	37·0	

Judging then from these results and from our own field observations, I do not believe that, apart from small instrumental errors, the error of the determination of the dip with a single needle, and without any delay after magnetization, will in general exceed ± 1′. Under unfavourable circumstances it may reach ± 1′·5. These estimates embrace not only the assumed instability of the magnetic axis, but that and all other causes of error combined. That some effect of the kind referred to by Prof. Nipher, which only affects the result below these limits, may exist even in good needles is perhaps possible. As the verniers of the circles only read to minutes it could not be detected except by making a number of observations for the purpose.

In conclusion I may add that for good dip observations good instruments are essential. In a preliminary survey in the neighbourhood of Mull, made in 1883, we employed an older instrument which had been a good deal used in a laboratory. The measurements made with it were less satisfactory than those above described, but the largest difference between the two needles did not exceed 6′. For survey purposes small needles and circles seem on all accounts better than the large ones used by Prof. Nipher. ARTHUR W. RÜCKER.

South Kensington, November 2.

Greek Geometry.

IN the notice of the last part of "Greek Geometry from Thales to Euclid" (NATURE, vol. xxxiv. p. 548) I was uncertain whether Dr. Allman intended it to be Part vii. or not; I observe from the extract before me (*Hermathena*, No. xiii., 1887, vol. vi. pp. 269–78) that the present part is so entitled. The author's plan led him to the temporary omission of Theætetus of Athens, a pupil of Theodorus of Cyrene, and also a disciple of Socrates, who greatly advanced the science of geometry. How his gifts and genius impressed both Socrates and Plato is well known from the dialogue which bears his name. From an analysis which our author makes of part of this dialogue it appears that Theætetus, in addition to Eudoxus and the Pythagoreans, was one of the original thinkers to whom Euclid was most indebted in the composition of the "Elements." Dr. Allman thus recapitulates :—"In the former parts of this paper we have seen that we owe to the Pythagoreans the substance of the first, second, and fourth books, also the doctrine of proportion and of the similarity of figures, together with the discoveries respecting the *application*, *excess*, and *defect* of areas, the subject-matter of the sixth book. The theorems arrived at, however, were proved for commensurable magnitudes only, and assumed to hold good for all. We have seen, further, that the doctrine of proportion, treated in a general manner, so as to include incommensurables (Book v.), and consequently the recasting of Book vi. and also the method of exhaustions (Book xii.) were the work of Eudoxus. If we are asked now: In what portion of the 'Elements' does the work of Theætetus survive? we answer: Since Books vii., viii., and ix. treat of numbers, and our question concerns geometry; and since the substance of Book xi., containing, as it does, the basis of the geometry of volumes, is probably of ancient date, we are led to seek for the work of Theætetus in Books x. and xii.; and it is precisely with the subjects of these books that the extracts (*d*), (*e*), and (*f*) are concerned."

The extract (*d*) states that Euclid, x. 9, is attributed to Theætetus by an anonymous scholiast, probably Proclus; extract (*e*) translates, discusses, and illustrates fully the passage (147 D–148 B) of the Dialogue; and extract (*f*) mentions the statement by Suidas, that our geometer taught at Heraclea, and that he first wrote on "the five solids," as they are called. Attention is

drawn to the difference in expression employed by Proclus, viz. that Euclid *arranged* many works of Eudoxus, and *completed* many of those of Theætetus, from which Dr. Allman infers that, "whereas the bulk of the fifth and twelfth books is due to Eudoxus; on the other hand, Theætetus laid the foundation only of the doctrine of incommensurables as treated in the tenth book. In like manner from (*f*) we infer that the thirteenth book, treating of the regular solids, is based on the theorems discovered by Theætetus; but it contains, probably, a recapitulation, at least partial, of the work of Aristæus" [cf. NATURE, *ubi supra*].

The author, in conclusion, draws the inference that the principal part of the original work of Euclid himself is to be found in the tenth book. "De Morgan suspected that in this book some definite object was sought, and suggested that the classification of incommensurable quantities contained in it was undertaken in the hope of determining thereby the ratio of the circumference of the circle to its diameter, and thus solving the vexed question of its quadrature. It is more probable, however, that the object proposed concerned rather the subject of Book xiii., and had reference to the determination of the ratios between the edges of the regular solids and the radius of the circumscribed sphere, ratios which in all cases are irrational. In this way is seen, on the one hand, the connection which exists between the two parts of the work of Theætetus, and, on the other, light is thrown on the tradition handed down by Proclus, that Euclid proposed to himself the construction of the so-called Platonic bodies as the final axiom of his systematization of the 'Elements.'" Here for the present I take leave of the author. I have read his several parts as they have appeared with very great interest, and have endeavoured, without going far into technical details, to indicate the results arrived at, and I hope that some will have been induced to go to the fountain-head for undiluted draughts from this refreshing stream. I need only repeat the expression of the wish, more than once previously uttered, that the several papers may be collected into a handy volume, in which case they will fitly go side by side with the works of Bretschneider, Cantor, Tannery, and other distinguished labourers in the same field. R. T.

The Chromosphere.

HAVING lately devised a spectroscope with two small sextant telescopes and two small prisms, one of "extra dense" glass by Hilger, I attached it to a 2½-inch telescope, and tried its powers on the sun on the 6th inst., with the result that not only were the rays C and D^3 easily visible as bright lines, but I also found that by opening the slit and keeping the brighter part of the spectrum out of view I could see the actual ragged surface of the "storm-tossed sea of hydrogen."

I found the depth of the chromosphere to be about 10″, by estimating the length of the bright line when exactly tangential to the limb.

This result shows what is possible with small instrumental means, though probably much was due to an exceptionally clear sky. JOHN EVERSHED, JUN.

Perception of Colour.

IN answer to Mr. T. W. Backhouse, I would suggest that he should use the spectroscope in the following manner. Hold it between the luminous object (moon or street lamp) and the eye at a distance of about 12 or 15 inches from the latter, so that only part of the spectrum is seen. Then remove the spectroscope sideways, and pass it quickly through its old position. A flash of coloured light will be seen, and no matter what may be the direction of the spectrum with reference to the line of motion the flash will always be seen to travel from the red end towards the blue end. Each part of the spectrum can be examined separately.

Whether this phenomenon is due to a later perception or longer retention of the blue light as compared with the red I cannot at present say, but I think it is independent of the intensities. C. E. STROMEYER.

Swifts.

ON June 19, and again on June 21 last, in the evening, I watched a vast concourse of swifts flying over this town. They slowly soared upwards, shrieking and striking at each other, and at last went so far up in the sky as to look like a cloud of black gnats. I watched them till dusk, when their faint cries were still audible, and when these had died away in the distance I waited long for the birds to descend, but they did not, probably because they were old birds which had been sitting all day, and were glad of an opportunity to stretch their long wings in a few hours' flight. No great height would necessarily be attained by the birds during the short midsummer nights. I noticed on several subsequent evenings that at least some of the swifts of the town did not stay up till dusk; but I am not the less positive that on June 19 and 21 they spent the night in the sky.

Stroud. C. B. WITCHELL.

Note on a Madras Micrococcus.

THE sole charge of a Presidential Museum and the study of that high-road to pathological eminence, bacteriology, are unfortunately not compatible, but I have not been able to resist the rough investigation of a phenomenon which stands prominently out before my eyes as I write. It consists of a thin, homogeneous, pale pink pellicle, covering the *chunam* (shell-lime) walls of my house on the side exposed to the heavy monsoon rain, which is at present varying the monotony of our "fine sunny days," which so impress our energetic cold-weather visitors, who learn all about India from Calcutta to Cape Comorin in a three weeks' tour. So evenly is the pink-coloured material distributed in my library, that its walls look as if they had been painted on one side, and whitewashed on the other three sides. This coloration, which is well known in Madras, is, I believe, commonly attributed to some occult chemical action on the lime, but a cover-glass specimen stained with methylene blue, and examined with a ⅛-inch objective, decides at a glance that it is caused by a Micrococcus, which, in its microscopical appearance, presents nothing remarkable.

The mode of growth of this organism on or in artificial nutrient media I have not attempted to investigate, but I notice that white lead does not agree with it, as its growth ceases abruptly at the painted framework of the doors and windows.

As I cannot find any description, in the reference-books at my disposal, of a Micrococcus which corresponds to the one here described, I christen it provisionally *Micrococcus madraspatanus*, Madraspatan being the old name of Madras, which is, according to Lassen, a corruption of Manda-rājya, meaning "realm of the stupid." EDGAR THURSTON.

Government Central Museum, Madras, October 26.

Catharinea undulata.

IN October 1886 I found, in Hertfordshire, two specimens of *Catharinea undulata*, Web. et Mohr., bearing fruit in the axils of the leaves; those specimens I unfortunately lost.

When this summer in Norway, I had the good fortune and pleasure of meeting Prof. S. O. Lindberg, of Helsingfors, and I mentioned the fact of the discovery to him. He then told me that similar specimens had been found in Norway some little time before, and described under the name of *C. anomola*, Lindberg and Bryhn. In consequence of my conversation with Prof. Lindberg I looked again this autumn for specimens similar to those I had found last year, and after some little search I found some half-dozen or so near the same spot where I had found them last year.

The specimens I now have in my possession bear fruit at the apex of the stem, and also one, or sometimes two, setæ in axils of leaves below the apex. These pleurocarpous setæ differ from the acrocarpous by being twisted in a spiral manner, not being straight as the acrocarpous fruits are; they are inserted in a vaginula in the axil of the leaf, without any perichætial leaves.

I should be glad if bryologists generally would look out for specimens of this form. I should also consider it a great favour if any collectors who may find specimens would let me know, and provide me with an accurate description, or send me the specimens for inspection. Specimens should be preserved in strong methylated spirit, otherwise it may be difficult to verify some important details.

There is a brief reference to the Norwegian specimens in the *Botanische Centralblatt*, Band xxix. p. 2, 1887; the full description is, I believe, to be found in the *Botaniska Notiser*, 1886, p. 157; the latter I have not yet been able to obtain access to, though I hope to do so soon. J. REYNOLDS VAIZEY.

Botanical Laboratory, Cambridge, November 18.

RESEARCHES ON METEORITES.[1]

II.

The Cases of Nova Orionis and R Geminorum.

THE stars with bright carbon flutings, the same as those seen in comets, are not limited to first-magnitude stars, such as α Orionis, but include at least one new star, Nova Orionis. Because the latter star lasted but a short time we might expect the phenomena presented to be different from those found in the first-magnitude star, which is a variable, like others with similar composite spectra. Practically there is little difference, for in α Orionis, α Herculis, and others of that type, we find well-marked dark absorption flutings of manganese, as well as line-absorption of sodium and magnesium.[2] The absorptions are not so well developed in the Nova, for the reason, perhaps, that condensation due to gravity had not taken place to such a great extent, so that the heat of the stones themselves was not so great, and further because the local absorption around each meteorite would be cloaked by the bright radiation of the interspaces, which gives, as in comets, the maximum intensity to the bright fluting, wave-length 517. In R Geminorum the demonstration of the same meteoric constitution, but without the strong absorption, is given by the fact that in that star so much of the light proceeds from the vapour produced by the meteorites, and from the carbon in the interspaces, that the carbon flutings and the bright lines of barium and strontium, and other substances present in meteorites, are visible at the same time, exactly as they are seen in the glow over a meteorite in an experimental tube, in which, as the pressure is reduced, the edges alone of the carbon flutings are visible, and put on the appearance of bright lines, almost exactly resembling the bright lines of the heated meteorites.

I give on a map the spectra of these two stars side by side with the bright flutings of carbon and the dark flutings of manganese with a view of showing that, both in the Nova and the fi st magnitude one in the same constellation, many of the phenomena are the same and are therefore probably produced by the same cause. Some time after Dr. Copeland's original observations of this star were published, it was pointed out, by

MAP 4.—Spectra of nebulæ compared with the spectra of hydrogen, cool magnesium, and meteorite glow.

Dunér, Vogel, and others, that some of the bright parts of the spectrum observed by him were really coincident with the bright parts of the spectrum of α Orionis; this, of course, is beyond question. But in addition to these bright spaces Dr. Copeland gives some bright regions which, I think, have not been touched by the arguments of Vogel and Dunér above referred to. It will be observed that in the case of R Geminorum, given on the same map as Nova and α Orionis, the bright lines correspond almost exactly with the bright spaces shown in the above-named stars and certain lines seen in meteorites—that is to say, a meteorite glow, when the carbon spectrum is bright, gives us all the lines recorded in the spectrum of the star, showing that some of the lines correspond with the brightest flûtings of carbon.

There can be no question, I think, that in R Geminorum we have another stage, doubtless a prior stage, of the life-history not only of the Nova, but of α Orionis itself.

[1] Continued from p. 61.
[2] The manganese absorptions agree with some of the manganese flutings seen in the Bessemer flame by Marshall Watts (*Phil. Mag.* February 1873).

III. The spectra of meteorites glowing in tubes with the bright lines observed in celestial bodies—

(α) Comparison with the lines seen in nebulæ when C and F (bright) are either present or absent.

(β) Comparison with bright lines (not associated with flutings) seen in stars.

α. "Nebulæ."

Only seven lines in all have been recorded up to the present in the spectra of nebulæ, three of which coincide with lines in the spectrum of hydrogen and three correspond to lines in magnesium. The magnesium lines represented are the ultra-violet low-temperature line at 373, the line at 470, and the remnant of the magnesium fluting at 500, the brightest part of the spectrum at the temperature of the bunsen burner. The hydrogen lines are *h*, F, and Hγ (434). Sometimes the 500 line is seen alone, but it is generally associated with F and a

line at 495. The remaining lines do not all appear in one nebula, but are associated one by one with the other three lines. The lines at 500 and 495 and F have been seen in the glow of the Dhurmsala meteorite when heated, but the origin of 495 has not yet been determined.

The result of this comparison then is that the nebula spectrum is as closely associated with a meteorite glowing very gently in a very tenuous atmosphere given off by itself as is the spectrum of a comet near the sun by a meteorite glowing in a denser one also given off by itself when more highly heated.

Further, it has been seen that the nebula spectrum was exactly reproduced in the comets of 1866 and 1867, when away from the sun. As the collision of meteorites is accepted for the explanation of the phenomena in one case, it must, *faute de mieux*, be accepted for the other. The well-known constituents of meteorites, especially olivine, fully explain all the spectroscopic phenomena presented by luminous meteors, comets, and nebulæ.

I published many years ago an experiment in which I had found that the gases evolved from meteorites under some conditions gave us the spectrum of hydrogen and under others the spectrum of carbon; but in the globes I then used I was not enabled to study the spectrum of the glow itself.

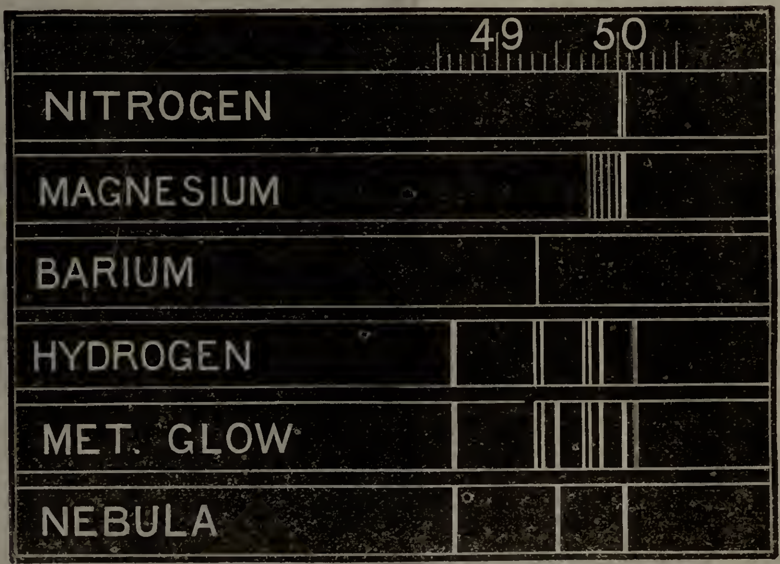
I should add that the line at 495 makes its appearance much more rarely than the one at 500, in meteorite glows.

β. "Stars" with bright lines.

On reference to the maps which I exhibit to the Society, though they and the discussion of them are yet incomplete, it will be seen that the principal lines which are seen bright in star spectra are, if we make due allowance for the discrepancies likely to occur in observations attended with great difficulties, lines which either have been observed in the vapours and gases given off by meteorites in vacuum-tubes or which we might expect to see in a combined series of observations on meteorites having different chemical constituents. Among these lines are H_α, H_β, H_γ, H_δ, 464, 540, 570, 580, 587; in one case (1st Cygnus) there are lines at 5065 and 5268, the latter due to iron. The difficulties attending this part of the inquiry are referred to subsequently, and it must be understood that in the absence of a detailed discussion especially of the spectra of the "Novas," which I have not yet completed, the opinion I express in the next part of this preliminary notice with regard to bright-line stars must be regarded rather as suggestions than as final conclusions.

Discussion of the Maps showing the Bright Lines visible in Stars and Nebulæ.

It results from the discussion of the bright lines seen, whether associated with the bright lines C and F of hydrogen or not,



MAP 5.—Shows the positions of three of the nebula lines as compared with well-known lines.

that, while on the one hand we have a class of bodies—the nebulæ—which give us the lines visible at the lowest temperature of chemical elements known to exist in meteorites, we have in the other class—the "stars" with bright lines—those lines visible at somewhat higher temperatures in meteorites. In the stars with bright lines the two most important lines, which have been separately mapped by Vogel,[1] occur at 540 and 582. The mean readings of all the observations gives the positions of these lines as 540 and 580. In an experiment on the glow of a meteorite rich in manganese, the line of Mn at 5395, easily seen at the temperature of the bunsen, is distinctly seen in addition to the structure-spectrum of hydrogen. There is reasonable ground therefore for supposing that this line, the only one of the iron group of metals visible at the temperature of the bunsen, may be the origin of one of the two lines seen alone in the spectrum of these "stars." It will be seen that in the map it has been easy to arrange all the bright lines hitherto seen in stars into one order, in which we begin with this line of manganese, and a line of iron seen at the temperature of the oxy-coal-gas flame, the wave-length of which is 579. As other lines indicating other substances are added to these fundamental ones, we pass from those stars in which C and F are not visible to those in which they make their appearance. Here, however, it is necessary to move with caution, because it may be that we are in presence of some of the lines visible in the structure-spectrum of hydrogen. The chief lines of hydrogen, as seen in the end-on tube when the conditions are such that C and F are not visible, have been already stated. Some of the lines observed in these stars, even the one at 540, have been found to be very nearly coincident with bright lines seen in the structure-spectrum, as well as with lines seen in the spectra of meteorites.

The suggestion, therefore, that some of the lines seen in bright-line stars are lines of cool hydrogen must be noted, although there are strong grounds for rejecting it, as will shortly appear. One objection is that strong lines of the H structure at 607–610 and 574 have not been recorded in star spectra with those at 540 and 580.

In the nebulæ (see Map 5) we deal chiefly with lines seen in the spectrum of magnesium at the lowest temperature; and these, as far as observations go, have not yet been associated with the lines at 540 and 580 to which reference has just been made, although they may or may not be associated with the bright lines C and F of hydrogen. In the nebulæ, however, no lines coincident with the lines of cool hydrogen have been observed. It will be seen, there-

[1] *Potsdam Observations*, vol. iv. No. 14.

fore, that we have here again strong ground for rejecting the view that the lines seen in "stars" at 540 and 580 are due to cool H, for since hydrogen is common to both nebulæ and stars, there is no reason why structure lines should occur in "stars" any more than in nebulæ.

Another ground for rejecting cool hydrogen as the origin of any of the lines in "stars" is that the structure-spectrum of hydrogen is only seen in confined glows, which is just the condition which cannot occur in space.

At the same time, the apparent coincidences of many meteorite lines with structure lines of hydrogen greatly increases the difficulties of laboratory work; in fact, the structure-spectrum of hydrogen is to observations of meteorite glows in the laboratory what continuous spectrum is to observations of bright lines in stars.

If it be agreed that we are not dealing with cool hydrogen, then it will follow that the only difference between celestial bodies with bright lines in their spectra comes from no difference of origin or chemical constitution, but from a difference of temperature.

At one point in these researches I was under the impression that the differences in the systems of bright lines seen in the nebulæ and the bright-line stars might arise from a preponderance of irons or stones in the swarms. But I was led to abandon this idea, not only by the observation of the meteoritic glows, but by the consideration that even telescopically the "stars" in question are more condensed than the nebulæ.

The spectrum of the nebulæ, except in some cases, is associated with a certain amount of continuous spectrum, and meteorites glowing at a low temperature would be competent to give the continuous spectrum with its highest intensity in the yellow part of the spectrum; so that in this way we should understand that lines due to any gas or vapour in that part would be very much more likely to escape record than those in the part of the spectrum which the continuous spectrum hardly reaches. The general absence, however, of bright lines of metallic vapours, except 495 and 500, and of the bright lines of hydrogen, evidently justifies the conclusion that we are here in presence of those bodies in celestial space, connected with which the temperature and the electrical excitation are at the minimum, and it is very remarkable how the lines seen in a Geissler tube under the conditions stated, when either magnesium, or olivine, or other meteoric constituents are made to glow, should appear, one may almost say, indiscriminately among the orders of bodies in the heavens which up to the present time have been regarded as so utterly different in plan and structure as stars and nebulæ.

The records of purely continuous spectra in the case of many nebulæ, as, for example, the Great Nebula in Andromeda, is in all probability an indication of our inability to observe them properly. For a nebula to give a perfectly continuous spectrum, it is evident that the component meteorites must be incandescent, but still at a lower temperature than that required to give bright lines. Now, the Mg line 500 is seen in some of the faintest nebulæ, where there is little or no continuous spectrum, and it therefore seems likely that these are at a lower temperature than the nebulæ said to give perfectly continuous spectra. This being so, it is difficult to believe that other lines, which require a somewhat higher temperature for their existence than the line at 500, do not become visible at this increased temperature.

There can be little doubt that when our instrumental appliances and observing conditions become more perfect it will be found that the so-called continuous spectra are really discontinuous. There is, indeed, an element of doubt as regards some of the existing observations; thus, the spectrum of the companion to the Great Nebula in Andromeda appears to end abruptly in the orange, and throughout its length is not uniform, but is evidently crossed by lines of absorption or by bright lines (Huggins, Phil. Trans. vol. cliv. p. 441).

Again, the Great Nebula in Andromeda is generally regarded as having a continuous spectrum pure and simple, but an observer at Yale College (name not stated) has observed three bright lines in its spectrum (*Observatory*, vol. viii. p. 385). The lines are—the F line of hydrogen, and two other lines at wave-lengths 5312·5 and 5594·0. The latter two lines are mentioned by the same observer as bright lines in γ Cassiopeiæ and β Lyræ, and are recorded by Sherman (*Astr. Nach.*, No. 2591) as bright lines in these stars and in Nova Andromedæ. No other observations with which I am acquainted give these two lines in γ Cassiopeiæ or β Lyræ, but Maunder (*Monthly Notices*, vol. xlvi. p. 20) gives them as two of the lines seen in Nova Andromedæ. It is possible, therefore, that the two lines in question, in the Yale College observation, had their origin in Nova Andromedæ; at all events there is no evidence to show that they are visible in the Great Nebula of Andromeda under normal conditions.

It is not impossible that the lines at 540 and 580 may be eventually traced in some of the brightest nebulæ, since these are apparently the lines next in order, as regards temperature, to the Mg line 500.

It is right that I should here point out that some observers of bright lines in these so-called stars have recorded a line in the yellow which they affirm to be in the position of D_3; while on the other hand, in my experiments on meteorites, whether in the glow or in the air, I have seen no line occupying this position.

I trust that some observer with greater optical means will think it worth his time to make a special inquiry on this point. The arguments against this line indicating the spectrum of the so-called helium are absolutely overwhelming. The helium line so far has only been seen in the very hottest part of the sun which we can get at. It is there associated with *b* and with lines of iron which require the largest coil and the largest jar to bring them out, whereas it is stated to have been observed in stars where the absence of iron lines and of *b* shows that the temperature is very low. Further no trace of it was seen in Nova Cygni, and it has even been recorded in a spectrum in which C was absent.

It is even possible that the line in question merely occupies the position of D_3 by reason of the displacement of D by motion of the "stars" in the line of sight. On this point no information is at hand regarding any reference spectrum employed. If, however, it should eventually be established that the line is really D_3, which probably represents a fine form of hydrogen, it can only be suggested that the degree of fineness which is brought about by temperature in the case of the sun is brought about in the spaces between meteorites by extreme tenuity.

The Case of Nova Cygni.

The case of Nova Cygni is being discussed, and it appears likely that this "star" passed through all the stages of temperature represented by "stars" with bright lines, comets, and nebulæ. In the initial stage, the principal lines recorded were those of hydrogen, cool magnesium, and sodium. At a later date, in addition to these, lines apparently indicating hotter magnesium and carbon were observed. On the date of its highest temperature (December 8, 1876) the lines observed by Vogel indicate H, Na, Mg, C, Fe, Mn, and Ba, the "star" having then, it would appear from the discussion so far as it has yet gone, approached the condition of the great comet of 1882 at perihelion. The Fe, Ba, C, and Na gradually disappeared, then the hydrogen followed, and the last stage of all was that in which Mg (500) appeared alone, as in the comets of 1866–67 and in nebulæ. The complete discussion, however, must be reserved for a future communication. It is sufficient to say here that it is very probable that all the spectroscopic phenomena of Nova Cygni will admit of explanation on the supposition that it was produced by the collision of two swarms of meteorites. The outliers were first engaged, and at the maximum the denser parts of the swarm.

Difficulties connected with the Discussion.

An inspection of the maps, on which are shown all the observations already made upon bright lines recorded in the spectra of celestial bodies, will indicate at first sight an apparent variation of the positions of the lines greater than might have been expected. This, however, I think will vanish on the consideration of the whole question; and for my part certainly all the examinations which I have been able to make have led me to the conclusion that the various observations have been far better than it was almost possible to hope for when the great difficulties of the observations themselves are considered.

When it is remembered that, in order to get a determination of the position of a bright line, comparison-spectra and prisms are needed, and that, from mechanical considerations alone, the application of these aids to research is very frequently attended with difficulties and uncertainties; and further, when we consider that many of the observations have been necessarily made without these aids; the striking coincidences on the maps become of very much greater importance than the slight variations seen between the positions of the same line recorded by different observers in the same star.

It will be observed, too, that the information in some cases is fuller in the blue part of the spectrum. Here again a reference to what the maps are really intended to show is necessary. The maps do not show the complete spectrum observed, but only the bright lines recorded in it. The actual observations have really consisted in picking out these bright lines from the background of continuous spectrum, whether in stars, nebulæ, or comets; and, as the continuous spectrum will be generally brightest in the yellow and green, so in this part of the spectrum we must expect, first of all, to get the least information, and then, when the information is obtained, to get the greatest uncertainty, on account of the difficulty brought about by the greater luminosity of the background on which the line appears.

The discussion by Hasselberg and others of the various observations of comets which have been made from time to time indicates that under certain circumstances, where men of the highest skill and with the greatest care have determined the wave-lengths of the carbon bands, discrepancies exist too great to admit of their being attributed to errors inherent in this branch of observation.

If for a moment we consider alone the two bright flutings visible in the spectrum of carbon, one with its bright edge just more refrangible than b_4—this is the high-temperature spectrum—and the other—the low-temperature spectrum—with a fluting just less refrangible than b_1, it is at once suggested that sudden changes in comets may very likely be accompanied by a transition from one condition of carbon vapour to the other, so that on this account apparent discrepancies in the measurements of the same comet at different times may represent real facts. Then again we have the motion of the swarm along its orbit, which in some cases we know is comparable to the velocity of light, so that variations of wave-length are produced as indicated in comet 1882. We also have the possibility that the velocity of the vapours in the jets, and that due to the electric repulsion—which, according to Zöllner's view, is the origin of comets' tails—may also produce changes of refrangibility.

Although as a rule the bright fluting seen in comets appears to be that due to high temperature, this is apparently not always the case. In the experiments on the glow of magnesium wire, the flutings of carbon have always been seen, and when the vacuum is approached the flutings have been those of the low-temperature spectrum. When the glow of the metal is seen under certain conditions, mixed with carbon vapour, b_1 and b_2 are seen as bright dots or short lines inside the carbon fluting, exactly as they were observed, probably, by Huggins in Brorsen's comet (Proc. R. S., vol. xvi. p. 386).

Authorities used in the Maps.

The map showing the bright lines in *Stars* is based upon the following authorities:—

3rd Cygnus, B.D. + 36° No. 3956, R.A. 20h. 10m. 6s., Decl. + 36° 18′.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 19.

2nd Cygnus, B.D. + 35°, No. 4013, R.A. 20h. 7m. 26s., Decl. + 35° 50′·8.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 19.
Wolf and Rayet.—*Comptes rendus*, vol. lxv. (1867), p. 292. The wave lengths were obtained from a curve based on the measurements given.

Argelander-Oeltzen 17681, R.A. 18h. 1m. 21s., Decl. − 21° 16′·2.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 15.
Pickering.—*Astronomische Nachrichten*, No. 2376.
Pickering.—*Observatory*, vol. iv. p. 82.

γ Argus, R.A. 8h. 5m. 56s., Decl. − 46° 59′·5.
Copeland.—*Copernicus*, vol. iii. p. 205.
Ellery.—*Observatory*, vol. ii. p. 418.

Stone 9168 (star in Scorpio), R.A. 16h. 46m. 15s., Decl. − 41° 37′·6.
Copeland.—*Copernicus*, vol. iii. p. 205.

1st Argus, R.A. 8h. 51m. 1s., Decl. − 47° 8′.
Copeland.—*Copernicus*, vol. iii. p. 206.

2nd Argus, R.A. 10h. 36m. 54s., Decl. − 58° 8′.
Copeland.—*Copernicus*, vol. iii. p. 206.

Gould 15305 (Argo), R.A. 11h. 5m. 10s., Decl. − 60° 21′.
Copeland.—*Copernicus*, vol. iii. p. 205.

Star in Centauri, R.A. 13h. 10m. 37s., Decl. − 57° 31′.
Copeland.—*Copernicus*, vol. iii. p. 206.

Star in Cygnus, B.D. + 37° No. 3821, R.A. 20h. 7m. 48s., Decl. + 38° 0′·1.
Copeland.—*Monthly Notices of the Royal Astronomical Society*, London, vol. xlv. p. 90.

Lalande 13412, R.A. 6h. 49m. 15s., Decl. − 23° 46′·3.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 17.
Pickering.—*Astronomische Nachrichten*, No. 2376.

1st Cygnus, B.D. + 35° No. 4001, R.A. 20h. 5m. 48s., Decl. + 35° 49′·7.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 17.

γ Cassiopeiæ, R.A. 0h. 50m. 4s., Decl. + 60° 7′·2.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 15.
Vogel.—*Beobachtungen zu Bothkamp*, Heft ii. p. 29.
Gothard.—*Astronomische Nachrichten*, No. 2581.
Konkoly.—Quoted by Gothard in *Astronomische Nachrichten*, No. 2581.
Observatory, vol. vi. p. 332.

β Lyræ, R.A. 18h. 45m. 55s., Decl. + 33° 13′·9.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 15.
Vogel.—*Beobachtungen zu Bothkamp*, Heft i. p. 33.
Gothard.—*Astronomische Nachrichten*, No. 2581.

The map showing the bright lines in *Nebulæ* is based upon the following authorities:—

Nebula in Orion.
Huggins—Proceedings R.S. vol. xiv. p. 39.

Planetary Nebula, R.A. 21h. 22m., Decl. + 47° 22′.
Copeland.—*Copernicus*, vol. i. p. 2.

Planetary Nebula.
Vogel.—*Monatsberichte der Akademie der Wissenschaften zu Berlin*, April 1878, p. 303.

No. 4572, 2075h., 16 H. iv. R.A. 20h. 16m. 7·9s., N.P.D. 70° 20′ 19″·3.
Huggins.—Philosophical Transactions, vol. clvi. p. 385.

Comet 1866.
Huggins.—Proceedings R.S. vol. xv. p. 5.

Nova Cygni.
Lord Lindsay and Dr. Copeland.—*Copernicus*, vol. ii. p. 109.

The map showing the coincidences of flutings of carbon, manganese, and zinc, with bright lines and flutings in stars and comets, and in a meteorite glow, is based upon the following authorities:—

Hydrocarbon ... ...
Low-temperature carbon ...
High-temperature carbon ...
} Work at Kensington.

Comet *b* 1881.
Copeland.—*Copernicus*, vol. ii. p. 225.

Manganese flame.
Lecoq de Boisbaudran.—"Spectres Lumineux."
Work at Kensington.

Nova Orionis.
Copeland.—*Monthly Notices* of the Royal Astronomical Society, vol. xlvi. p. 109.

α Orionis.
Vogel.—*Beobachtungen zu Bothkamp*, Heft. i. p. 20.

R Geminorum.
Vogel.—*Astronomische Nachrichten*, No. 2000.

Meteorite Glów.
Work at Kensington.

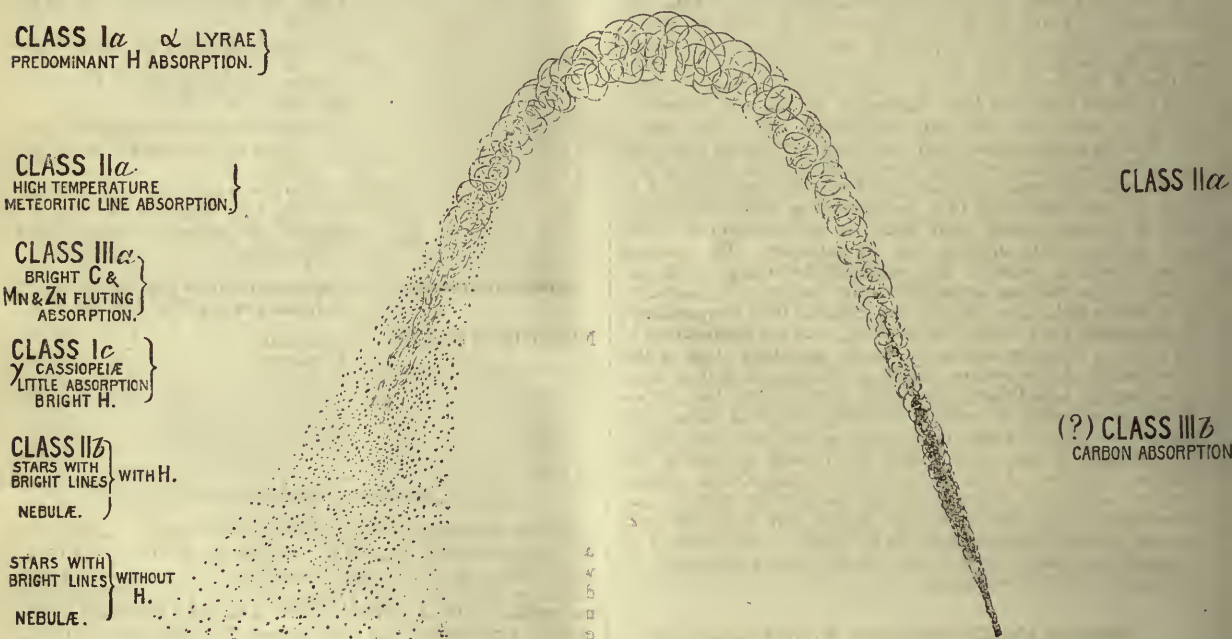
Schjellerup 152.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 30.

On the Absorption Phenomena of Stars with Bright Lines.

In addition to the map showing the bright lines visible in those stars the spectra of which contain them, I have prepared another map showing the absorptions which also occur. The two maps present a remarkable agreement—that is to say, there is the same progression in the absorption phenomena as there is in the bright-line phenomena. In those stars in which bright lines are seen without the lines of hydrogen (in which stars the meteorite swarm is probably at a slightly higher temperature than that observed in the nebula when only the line at 500 is visible) we have no marked absorption-lines, but rather bands. When the hydrogen lines are added, as in γ Cassiopeiæ, then we get the absorption of sodium and *b* of magnesium, as we should expect. The individual meteorites therefore are much cooler in these stars than in the Novas, seeing that the absorption is so little developed. Speaking generally, therefore, we may say that there are two causes of minimum absorption phenomena in stars. In the first place, as in the bright-line stars, only little vapour surrounds each meteorite, and that vapour consists of the substances visible at the lowest temperature; while, on the other hand, in stars like Sirius, in consequence of the absolute state of vapour, we only get practically the absorption of hydrogen, or at all events the absorption of hydrogen in great excess, due, I have very little doubt, in part, to the fact that most other substances have been dissociated by the intense heat resulting from the condensation of the meteorites.

NOTES ON THE PROVISIONAL TEMPERATURE CURVE.

In order to bring the various results referred to in this communication in a definite form before my own mind, I have prepared a diagram which I have called a temperature curve, so



Provisional Temperature Curve.

that on one side of it we may consider those stages in the various heavenly bodies in which in each case the temperature is increasing, while on the other arm of the curve we have that other condition in which we get first vaporous combination, and then ultimately the formation of a crust due to the gradual cooling of the mass. At the top of such a curve we shall of course have that condition in which the highest temperature must be assumed to exist. In a letter to M. Dumas in the year 1872, I suggested that possibly the simplification of the spectrum of a star might be associated with the highest temperature of the vapour, and that idea seems to have been accepted by other observers since that time. We shall have then stars of the first class at the top of the temperature curve. On the one arm of the curve representing increasing temperature we shall have at various heights those aggregations which give us indications of a gradually increasing temperature brought about by collisions, beginning with meteorites as widely separated as they can be to *keep up* any luminosity at all, and finally vaporous condensations due to gravity.

On the arm of the curve descending from stars of the first class to dark bodies like, say, the companion to Sirius, we must place those bodies where absorption of compound molecules is indicated. This we find in stars of Class III.*b* of Vogel. But here a very interesting question arises. Between stars of the first class and that of III.*b* we are bound to insert stars of Class II., already located naturally on the ascending arm.

The Case of Equal Temperatures on Either Side of the Curve.

Speaking roughly, it may be said that the construction of such a curve as this suggests that similar or nearly similar temperatures will be found on either side. This in the main, of course, is true; but it must be pointed out that, on the rising curve, the temperature will be that, as a rule, of individual meteorites and the vapours given out by them, while on the descending arm it will be the temperature of the consolidated mass, whether vaporous or becoming solid. But it is obvious that if we take two points near the top of the curve we shall have very nearly the same temperature of the atmosphere, by which I mean the temperature of the layers in either case which are most effective in producing the phenomena of absorption. To take a concrete case, stars of the second class are obviously, by the consent of all, of a lower temperature than stars of the first class: on which side, therefore, of the curve must they be placed? Or, to take a more concrete case still, our sun is a star of the second class: on which arm of the curve must we place the sun? Here

we find ourselves in a position of some difficulty, but it would appear that future work may enable us really to divide stars of the second class into two series, and if we can do so there is very little doubt that one series will represent the phenomenon of decreasing temperature of the absorbing layers, while the other series will represent the phenomenon of increasing temperature.

What considerations are likely to help us in such an inquiry as this? The atmosphere of a star built up by meteorites should resemble in its constitution the totality of the chemical constitution of meteorites, and therefore it might be inferred that the spectroscopic phenomena presented by such an atmosphere would not be widely different from the spectroscopic phenomena presented by the vapours of many meteorites volatilized together.

To investigate this question I have obtained composite photographs of the spectra of several meteorites, with a solar spectrum for purposes of comparison. I find that, while, on the one hand, the composite photograph giving us the spectrum of the meteorites greatly resembles that of the sun, as it should do, there are some variations which suggest the line of separation to which I have before alluded. From Dr. Huggins's magnificent photographs of the stars we have learned that, as I had predicted years before the photographs were taken, the thickness of H and K varies very greatly in different stellar spectra. In those stars, presumably the hottest ones, in which we get the series of hydrogen lines almost alone as great absorbers, K is almost absent; it finally comes in, however, and after a certain stage has been reached it is the most important line in the spectrum. But there are stars in which the lines *h* and G of hydrogen are not very much more developed than they are in the case of our own sun, in which K is much thinner than in the solar spectrum; and associated with this condition of K there is the absorption of a hydrogen line more refrangible than K at wave-length 3800, which is not represented in the solar spectrum with anything like the intensity. The question arises, therefore, whether the enormous thickening of K observed in the sun and some other stars may not be limited to those stars which, like our sun, are reducing their temperature; for we certainly are justified in assuming that the temperature of the sun now is not so high as it was in an earlier stage of the development of the system. Such a difference as that, if it is subsequently established, can only come from the atmosphere, as an effect of cooling, becoming richer in those substances the lines of which get broader as the star cools down. We can easily imagine that during the process of cooling the relative quantities of the vapours should not always remain constant, although it is impossible in the present state of our knowledge to give any particular reason why such and such vapours should disappear from the spectrum in consequence of chemical combination, while others should develop apparently in consequence of their retirement.

Hydrogen plus Carbon indicates Mixed Swarms.

If we assume a brightening of the meteor-swarm due to collision as the cause of the so-called new stars, we have good grounds for supposing that in these bodies the phenomena should be mixed, for the reason that we should have in one part of the swarm a number of collisions probably of close meteorites, while among the out-liers the collisions would be few. We shall in fact have in one part the conditions represented in Class III.*a*, and in the other such a condition as we get in γ Cassiopeiæ. I have in another part of this paper discussed the flutings observed in Nova Orionis, and have shown that so far as they were concerned we have the radiation of carbon and the absorption of manganese; but there is evidence to show that with these fluted appearances bright lines were observed—D_3 and F, although no mention is made of C.[1]

We have here, there is little doubt, the *vera causa* of stellar long-period variability. 12 per cent. of stars of Class III.*a* are variable, and 9 per cent. of Class III.*b*. In the one case, meteor-swarms produce the increased brightness by colliding with those of the condensing one. In the other, they do so by their periastron passage round the dim condensed one. There is no variability, in the usual sense of the word, in stars like the sun and α Lyræ, and the reason is now obvious.

[1] Konkoly, *Astr. Nach.* 2712, D_3 and F; Riccò indicates D_3 in *Astr. Nach.* 2707.

The Conditions of Collisions of Meteorites.

The Chemical Elements most frequently determined in Meteorites.

I think it well to give here as a reminder a short table showing the chief substances met with in meteorites. It will indicate the cause of the continued reference to the spectra of Mg, Fe, and Mn in what follows.

SIDERITES.

	Nickel-iron, copper, manganese.
	Troilite = FeS.
	Graphite.
	Schreibersite = iron and nickel phosphide.
	Daubréeite = iron and chromium sulphide.

SIDEROLITES.

CHONDRITIC—	
(α) Non-carbonaceous	= Olivine = chrysolite = peridot = $(MgFe)_2O_4Si = SiO_2$ 41·3, MgO 50·9, FeO 7·7.
	Enstatite $MgO_3Si = SiO_2$ 60, MgO 40.
	Bronzite = enstatite in which some Mg is replaced by Fe.
	Nickel-iron, manganese.
	Troilite.
	Chromite = iron protoxide 32, chromium sesquioxide 68, + Al and Mg.
	Augite = pyroxene, SiO_2 55, CaO 23, MgO 16, MnO 0·5, FeO 4.
	Silicate of calcium, sodium, and aluminium.
(β) Carbonaceous ..	... Carbon in combination with H and O.
	Sulphates of Mg, Ca, Na, and K.
NON-CHONDRITIC	= Anorthite.
	Enstatite.
	Bronzite.
	Olivine.
	Augite.
	Troilite.

The Numbers of Meteorites in Space.

It is well known that observations of falling-stars have been used to determine roughly the average number of meteorites which fall on the earth each twenty-four hours; and having this datum to determine the average distance apart between the meteorites in those parts of space which are traversed by the earth as a member of the solar system, Dr. Schmidt, of Athens, from observations made during seventeen years found that the mean hourly number of luminous meteors visible on a clear moonless night by one observer was fourteen, taking the time of observation from midnight to 1 a.m.

It has been further experimentally shown that a large group of observers who might include the whole hemisphere in their observations would see about six times as many as are visible to one eye. Prof. H. A. Newton and others have calculated that making all proper corrections the number which might be visible over the whole earth would be a little greater than 10,000 times as many as could be seen at one place. From this we gather that not less than twenty millions of luminous meteors fall upon our planet daily, each of which in a dark clear night would present us with the well-known phenomenon of a shooting-star.

This number, however, by no means represents the total number of minute meteorites that enter our atmosphere, because many entirely invisible to the naked eye are often seen in telescopes. It has been suggested that the number of meteorites if these were included would be increased at least twenty-fold: this would give us 400 millions of meteorites falling on the earth's surface daily. If we consider, however, only those visible to the naked eye, and if we assume that the absolute velocity of the meteors in space is equal to that of comets moving in parabolic orbits, Prof. H. A. Newton has shown that the average number of meteorites in the space that the earth traverses is in each volume equal to the earth about 30,000. This gives us a result

in round numbers that the meteorites are distributed each 250 miles away from its neighbours.[1]

If, then, these observations may be accepted to be good for any part of space, we may, and indeed must, expect celestial phenomenon which can be traced to meteorites in all parts of space.

Further, we have the experience of our own system that these meteors are apt to collect in groups.

A comet, it is now generally accepted, is a swarm of meteors in company. Such a swarm finally makes a continuous orbit by virtue of arrested velocities; impacts will break up large stones and will produce new vapours in some cases, which will condense into small meteoroids.

A meteorite in space under any of the conditions indicated by the comets, new stars, and such first-magnitude stars as α Orionis, will evidently be subject to collisions, but only to a greater number of collisions than those which must ordinarily occur if space is as full of meteorites as Prof. Newton's calculations, from observations made on the earth, would naturally seem to indicate.

The Velocity of Luminous Meteors.

In spite of the difficulties which attend the observations necessary to determine the velocity of meteors entering our atmosphere, many observations have been made from which it may be gathered that the velocity is rarely under 10 miles a second or over 40 or 50. It is known that the velocities of some meteor-swarms are very different from those of others. Prof. Newton, our highest authority on this subject, is prepared to consider that the average velocity may be taken to be 30 miles a second.

Result of Collisions.

If we take these velocities as representing what happens in other regions of space, and assume the specific heat of the meteorites to be ·10, the increase in their temperature when their motions are arrested by impacts will be roughly as follows:—

Velocity 1 mile per second	... ...	3,000° C.
,, 10 ,, ,,	... ...	300,000°
,, 20 ,, ,,	... ...	1,200,000°
,, 30 ,, ,,	... ...	2,700,000°
,, 60 ,, ,,	... ...	10,800,000°

It is clear, however, that we should under the conditions stated be more frequently dealing with grazes than collisions.

Comets due to Collisions of Meteorites.

The fact that comets are due to swarms of meteorites was first established by Schiaparelli in 1866, when he demonstrated that the orbit of the August meteors was identical with that of the bright comet of 1862.[2]

Nebulæ due to Collisions of Meteorites.

So far as I know the first suggestion that nebulæ were really in some manner associated with meteorites and not with masses of gas was made by Prof. Tait in 1871.[3] I have used the suggestion in my lectures ever since, and it is now some years ago since I put it to an experimental test by showing that both the spectra of comets and nebulæ, so far as carbon and hydrogen were concerned, could be produced from a vessel containing the vapours produced by meteorites. More recently, M. Faye has stated in his works on the nebular hypothesis that the solar nebula may as probably have consisted of a cloud of stones as of a mass of gas. This view, however, has not been favoured by Dr. Huggins, who in his observations both on nebulæ and comets has inferred from the near coincidence of the line of 500 with the strong air line that we are probably in presence of nitrogen, or of a form of matter more elementary than nitrogen; the line at 373 being attributed by him also to some unknown form of hydrogen on account of its coincidence with one of the series of hydrogen lines in the ultra-violet observed in the spectra of stars of the first class.

[1] Article on "Meteorites," Prof. Newton, "Encyclopædia Britannica," vol. xvi.

[2] Letters to Father Secchi, printed in the *Bollettino* of the Collegio Romano, and reproduced in *Les Mondes*, t. xiii.

[3] "It seems to me that we have a series of indications of what (for want of a better phrase) may be called the *period of life* of a star or group, beginning with the glowing gases developed by impacts of agglomerating cold masses (planetary nebulæ and others irresolvable, such as these of Orion, Lyra, &c., where the spectrum consists of a very few bright lines only)" (Prof. Tait, Proc. R.S. Edin., 1871).

"New Stars" due to Collisions of Meteorites.

The idea that the *Novas* which appear from time to time are due to collisions of meteorites was, I think, first advanced by myself in 1877, when I wrote in connection with Nova Cygni:—

"The very rapid reduction of light in the case of the new star in Cygnus was so striking that I at once wrote to Mr. Hind to ask if any change of place was observable, because it seemed obvious that, if the body which thus put on so suddenly the chromospheric spectrum were single, it might only weigh a few tons, or even hundredweights, and, being so small, might be very near us. Mr. Hind's telescope was dismounted, and I have not yet got any information as to change of position; and as I am now writing in the Highlands, away from all books, I have no opportunity of comparing the position now given by Lord Lindsay in R.A. 21h. 36m. 52s., Decl. + 42° 16′ 53″, with those given on its first appearance by Winnecke and others.

"We seem driven, then, from the idea that these phenomena are produced by the incandescence of large masses of matter, because if they were so produced, the running down of brilliancy would be exceeding slow.

"Let us consider the case, then, on the supposition of small masses of matter. Where are we to find them? The answer is easy: in those small meteoric masses which, an ever-increasing mass of evidence tends to show, occupy all the realms of space."[1]

The Effects of Collisions.

The question of what must happen to the meteorites themselves in consequence of this system of collisions is worth going into thoroughly. A very cursory examination seems to indicate that much light is thrown on the condition of meteorites as we know them, and their division into iron and stony.

As 30 miles per second is a very frequent value obtained for the velocity of meteorites when they enter our atmosphere, it is possible to compare temperatures brought about by collisions with those produced by passage through our atmosphere. Two masses of meteoric iron meeting each other in space would probably, if moving with a certain velocity, be formed into a pasty conjoined mass, and this process might go on until an iron of large dimensions was formed, and the various meteorites thus welded together would present in time a very fragmentary appearance. While irons were thus increasing in size, collisions with smaller meteorites would be attended with very local increases of temperature, perhaps sufficient to volatilize the surface or allow it to be indented, and in this manner the well-known "thumb-marks" receive explanation.

The masses of iron, when in a state of fusion, whatever their size, would be able to include stony meteorites in their vicinity. In the case of stones it is easy to see that the result would be very different. Their collisions would have, most probably, the effect of reducing large pre-existing masses to smaller ones, and the collision of a large stone with a large iron would probably effect the driving of the stone into fragments, while the iron would be liquefied so as to inclose some of the fragments in its mass.

These operations of Nature might go on either in free space, or in the head of a comet, or in meteor-swarms. They probably cause the appearance of the so-called new stars, and in these various circumstances the rate of subsequent cooling would of course be very different, so that the results would be very different indeed.

Large masses on collision probably destroy each other, produce fragments and vapour, which again condense. The heterogeneous structure is thus to a certain extent explained. On collision the part of the substance of the meteorite given up will depend on the temperature, and thus a mass of metallic iron mixed with silicates at low temperature will get rid of the iron at once, which must then perforce condense in a separate swarm; therefore under low temperature conditions, say at aphelion, irons alone will be formed and the stones will become spongy. The stones will absorb the C and H vapours.

[1] NATURE, vol. xvi. p. 413.

I have finally to express my great obligations to Messrs. Fowler, Taylor, and Richards, who have helped me in various ways in the researches embodied in this paper. Mr. Fowler, the assistant to the Solar Physics Committee, has made most of the observations on meteorites, and low-temperature spectra generally, which have been recorded on the maps, and he has carried out this work with a care, skill, and patience beyond all praise. The observations have in nearly every case been checked also by myself. Mr. Taylor, the Demonstrator of Astronomy, has been chiefly responsible for looking up the literature and mapping the results, in which he has been aided by Mr. Richards.

J. NORMAN LOCKYER.

SIR JULIUS VON HAAST, F.R.S.

SCIENCE in Australasia, and especially in New Zealand, has recently sustained a great loss by the death, on August 16 last, of Sir Julius von Haast. He was born on May 1, 1824, at Bonn, where his father was a wealthy merchant. After passing through the grammar-schools of Bonn and Cologne, he entered the University of Bonn, and devoted a considerable portion of his time to geological and mineralogical studies. He then spent some years in France, and made journeys for the purpose of scientific exploration in Russia, Austria, and Italy. Being invited by an English firm of ship-owners to visit New Zealand on their behalf in order to report upon its fitness as a field for German emigration, he went to London, and accepted their offer after some negotiation; and on December 21, 1858, he arrived at Auckland. The next day, by a lucky chance, the Austrian ship *Novara*—then on its voyage of scientific research—put into Auckland; and when Dr. von Hochstetter was left behind, at the request of the New Zealand Government, he took Mr. Haast as his lieutenant and companion in all his journeys in these islands. After the departure of Hochstetter, Mr. Haast was engaged by the Provincial Government of Nelson to explore the west coast of the province, and in the journey undertaken in the pursuit of these duties he commenced his examination of the physical geography and geology of the Southern Alps. The results of the exploration were published in a report printed by the Nelson Government and dated January 1, 1861.

Immediately after the conclusion of the Nelson journey—namely, in December 1860—he undertook to report to the Government of the Province of Canterbury as to the possibility of constructing a tunnel through the hills which separate Christchurch from its port of Lyttelton; and in the following year he was appointed to the command of the Geological Survey of Canterbury, being thus the first Government geologist in New Zealand. It was in this capacity that he accomplished the most valuable part of his scientific work. The most striking of his achievements were the examination of the Mount Cook district; the sketching and mapping out of the great glaciers of the Southern Alps, named by him the Tasman, Franz Joseph, Hochstetter, Hooker, and Müller glaciers, and many others; and the forecast and subsequent examination made of the auriferous districts of Westland. All this, with the geographical, zoological, botanical, and meteorological researches carried on side by side with the more exclusively geological work, was in continuation of what had been done in the Nelson or northern portion of the same mountain system. The results of his investigations were set forth in the chief book published by him—namely, "The Geology of Canterbury and Westland." He was also the author of many papers in scientific periodicals.

Last year he acted as New Zealand Commissioner at the Indian and Colonial Exhibition. Afterwards he visited Paris, Brussels, Berlin, Dresden, Vienna, Halle, Venice, Florence, and other centres, obtaining a vast number of things for the Canterbury Museum, the flourishing condition of which is mainly due to his energy and zeal. His labour in connection with the Exhibition, and the subsequent wear and tear of travelling while in weak health, appear to have overtaxed his strength, and he died of heart-disease a month after his return to New Zealand.

NOTES.

THE fourth session of the International Geological Congress will be held next year in London. The Congress was founded at a meeting of the American Association for the Advancement of Science at Buffalo in 1876, the first session being held at Paris in 1878, the second at Bologna in 1881, the third at Berlin in 1885. The following is a list of the Organizing Committee appointed to carry out the arrangements:—H. Bauerman, W. T. Blanford, F.R.S., Rev. Prof. T. G. Bonney, F.R.S., Prof. W. Boyd Dawkins, F.R.S., John Evans, F.R.S., Prof. W. H. Flower, F.R.S., Arch. Geikie, F.R.S., Prof. James Geikie, F.R.S., Sir Douglas Galton, F.R.S., Prof. A. H. Green, F.R.S., Rev. Prof. S. Haughton, F.R.S., Prof. T. H. Huxley, F.R.S., W. H. Hudleston, F.R.S., Prof. T. McK. Hughes, J. W. Hulke, F.R.S., Prof. E. Hull, F.R.S., Prof. J. W. Judd, F.R.S., Prof. J. Prestwich, F.R.S., F. W. Rudler, H. C. Sorby, F.R.S., Sir W. W. Smyth, F.R.S., W. Topley, Rev. Prof. Wiltshire, Henry Woodward, F.R.S. The duty of this Committee will be to nominate the officers, to appoint Executive Committees, and to fix the exact date of meeting. The Congress at Berlin requested that the meeting should be held in London between August 15 and September 15.

DR. DAWSON, Assistant-Director of the Canadian Geological Survey, who headed the party sent by the Dominion Government to explore the country adjacent to the Alaska boundary, has returned to Victoria. Two of his party, Messrs. Ogilvie and McConnell, will winter in the district, preparing the way for the establishment of the international boundary. The Expedition so far has secured a great deal of geological, geographical, and general information about the country, which is far from being the Arctic region it is sometimes represented to be. The point from which Dr. Dawson turned back was at the junction of the Lewis and Pelly Rivers. It is 1000 miles north of Victoria. There the flora was found to differ but little from that on the banks of the Fraser. A great deal of open grassy country exists along the stream's tributary to the Yukon. No areas of tundra or frozen swamps, such as are to be met with in the interior of Alaska, were discovered by the Expedition. Dr. Dawson's conclusion is that the whole country, from Cassiar to the vicinity of Forty-mile Creek on the Yukon River (which must be near the eastern boundary of Alaska), yields more or less gold in placer deposits. This would constitute a gold-bearing region fully 500 miles in length, and of indefinite width.

AT a meeting of the Council of University College, Bristol, held on Wednesday, November 16, it was decided, at the suggestion of the staff of the College, to suspend for a year the office of Principal. Prof. Lloyd Morgan was in the meantime appointed academical head of the College, and Chairman of the Educational Board, with the title of Dean.

AT the Royal Institution, Sir Robert Stawell Ball, the Astronomer-Royal of Ireland, will give a course of six lectures (adapted to a juvenile auditory) on Astronomy: the Sun, Moon, Planets, Comets, and Stars. The course will begin on December 27. Courses of lectures will also probably be given by Lord Rayleigh (Professor of Natural Philosophy at the Royal Institution), Dr. G. J. Romanes, Mr. Hubert Herkomer, Prof. C. Hubert H. Parry, the Rev. W. H. Dallinger, and Mr. William Archer.

WE are requested to state that the lectures to be given on behalf of the Anthropological Institute by Mr. Francis Galton—which were postponed in consequence of that gentleman's indisposition—will be delivered in the Lecture Theatre of the South Kensington Museum on Saturday, the 26th inst., and the two following Saturdays, December 3 and 10.

AN International Exhibition will shortly be held by the Ornithological Society at Berlin.

A MAGNETIC Observatory is about to be erected near the Solar Observatory, on the Brauhausberg, near Potsdam.

A CORRESPONDENT writes to us from Venice that on the 9th inst. an earthquake occurred there at 1.32 a.m. There were five gentle undulations, which lasted ten seconds. On the same morning—at 1.30 a.m.—a shock at Ferrara is said to have lasted seven seconds.

THE other day Mr. Ruskin sent to the *Times* the following extract from a letter, dated November 14, which he had received from a friend at Florence:—"We had an earthquake this morning, which frightened everybody, and my door shook so that I thought somebody was trying to break in, and then there was a terrible noise, but I believe no harm done. The bells rang of themselves at the Carmine, and some say that one or two chimneys fell, but nobody seems to know." From a report issued by Signor Passerini, Director of the Meteorological Observatory connected with the Agricultural College of Scandicci, we learn that three shocks were felt there on the 14th, the first taking place about 5.20 a.m. It was accompanied by a rather loud rumbling, and was powerful enough to shake all the furniture in Signor Passerini's room. A second shock, weaker, and not accompanied by rumbling, was felt about twenty minutes later. At 6.49 a.m. the third shock, the strongest of all, and accompanied by loud rumbling, was felt. During the continuance of this shock people in the open country saw trees much shaken, and quantities of leaves were observed to fall. The direction of the shocks was from north-north-west to south-south-east.

AT the suggestion of Prof. Mushketoff, who has just returned from his official visit to Semiretchia, a special Commission has been appointed to watch the course of any earthquakes that may happen, and to report on them, in those parts of the Russian Empire which are most frequently visited, such as the Caucasus, Turkestan, and the Transbaikal region.

ADVICES from Baku state that a naphtha spring has burst forth near the town of Balachany, the oil being thrown to a height of over 100 feet and carried away long distances by the wind. Sometimes the oil falls like rain over the adjacent districts, and forms small streams, whilst heavy naphtha gases fill the air.

ANOTHER contribution to the subject of photography in colours is published by Mr. Carey Lea in the November number of the *American Journal of Science*. Although the interpretations placed upon his former experiments have not received universal acceptance in this country, still the experiments themselves have been generally received with considerable interest and surprise, and indeed are at the present time being repeated and considerably extended in more than one English laboratory. The appearance of another communication from Mr. Carey Lea is therefore most opportune, and will doubtless form the subject for considerable discussion. It will be remembered that the so-called photo-salts of silver, a description of which appeared in these columns a few months ago, were said to consist of combinations of ordinary chloride of silver with small quantities of subchloride. Mr. Carey Lea now finds that silver chloride combines with small quantities of many other chlorides, besides its own subchloride, to form coloured salts, comparatively stable and remarkably less sensitive to light. Thus if silver nitrate be added to a solution of ferric chloride in presence of free hydrochloric acid, the precipitate obtained is buff-coloured, and the ferric chloride carried down by the silver chloride cannot be washed out even by hydrochloric acid. The most remarkable property of this silver-ferric chloride is that it is almost unacted upon by light. Chlorides of cobalt, nickel, manganese, and mercury give analogous combinations, each having a characteristic colour. As those chlorides, such as ferric and mercuric, which readily part with one equivalent of chlorine, act most energetically in reducing the sensitiveness, it appears probable that the traces of chlorine thus capable of being given up, simply hold in check the commencement of the movement towards reduction.

WE have received from Mr. Stewart Culin the reprint of a paper read by him before the Anthropological Section of the American Association at its meeting at New York during the past autumn. It is entitled "China in America: a Study in the Social Life of the Chinese in the Eastern Cities of the United States," and describes the special districts in Southern China from which the immigrants mostly come, the guilds and associations they form, their mode of life, their pleasures, which are somewhat few and simple, and much else in respect to them that is of a very interesting character. His own contact with the Chinese in the United States leads him to form a favourable estimate of their character and attainments, which have been the subjects of much misconception. They are not "the dregs of the people, given up to gambling and opium-smoking, and distinguished only by their vices," as the anti-Chinese orators aver; nor are their mental and moral qualities quite so high as others allege. But we fear very much that Mr. Culin is over sanguine in the anticipation that the returning emigrants will some day carry enlightenment to their own country. Their work is not of a kind that enables them to acquire very great knowledge of the resources of the West; they come with a special object, viz. the acquisition of a competence, they toil unremittingly until that is attained, when they speed home again, usually with no very pleasant memories of the land of their sojourn. To China herself we must look for the elements of her regeneration, and time, which is the great solvent, will have its slow effect on that huge mass of humanity.

THE German publisher, Herr Trewendt, of Breslau, has just issued the twentieth part of a Dictionary of Zoology, Anthropology, and Ethnology; the twenty-fourth and twenty-fifth parts of a Dictionary of Chemistry; and the twentieth part of a hand-book of Botany. These works belong to the elaborate "Encyklopædie der Wissenschaften," edited by Dr. W. Förster, Dr. A. Kenngott, Dr. A. Ladenburg, and other scientific writers.

PROF. FOREL is at present studying the penetration of light into the Lake of Geneva, by means of the photographic effect on chloride of silver paper. Six photographic apparatus are attached one above another to a rope at 10-metre intervals. They are let down into the lake after sunset, left there one day or more, and taken up again at night. The depth-limit of absolute darkness has been found this year, in the beginning of March, 100 m.; of May, 75 m.; and of July, 45 m. Prof. Forel hopes to carry on these experiments for a whole year, every two months, and so obtain the curve for penetration of light into the lake.

THE loss of electricity by a conductor in moist air has been lately studied by Signor Guglielmo (Turin Academy). He finds that with potentials less than 600 volts, moist air insulates as well as dry air, but with higher potentials, there is more loss in moist air, and more the moister the air, and the higher the potential. The potential at which the difference becomes per-

ceptible is the same for a ball as for a fine point. It occurs with extremely smooth surfaces, and so cannot be attributed to discharges in consequence of roughness of surface. With equal potential the loss of electricity has the same magnitude, whatever the dimensions of the balls used as conductors. In air saturated with vapours of insulating substances, the loss of electricity of a conductor is nearly the same as in dry air.

THE large Russian ironclad *Tchesme*, now being finished at Sebastopol, and having a displacement of over 10,000 tons, is to have boilers heated with petroleum. If the results correspond to what the Sebastopol engineers expect, the example is likely to be followed elsewhere. In this connection we may note an account in *La Nature* of November 5, of a gas-boat, as it may be called, the *Volapuk*, recently constructed by M. Forest, in which a gas-engine of six horse-power is driven, not by coal-gas, but by air charged with carbureted hydrogen, by passage through petroleum-oil. There are two pistons, and the explosive mixture is ignited by means of a spark from a magneto-electric arrangement. The engine consumes six litres of petroleum-oil per hour, giving a speed of sixteen kilometres per hour.

AMONG the various uses of celluloid, it would appear (according to the *Annales Industrielles*) to be a suitable sheathing for ships, in place of copper. A French Company now undertakes to supply the substance for this at 9 francs per surface-metre and per millimetre of thickness. In experiments by M. Butaine, plates of celluloid applied to various vessels in January last were removed five or six months after, and found quite intact and free from marine vegetation, which was abundant on parts uncovered. The colour of the substance is indestructible; the thickness may be reduced to 0·0003 metre; and the qualities of elasticity, solidity, impermeability, resistance to chemical action, &c., are all in favour of this use of celluloid.

THE following interesting observations with regard to the mobility of loess have been made by M. Potanin during his last journey through the region south of the Ordos. As wind steadily moves the shifting sands, so also water steadily moves the loess, transporting it from higher to lower levels. The underground water which filtrates through the loess, begins by making in it a kind of cavern; then a circular crevice appears on the surface over the cavern, and a cylindrical vertical hollow, which soon becomes a deep well, is formed through the thickness of the upper layers of the loess. The whole surface of the loess deposits is dotted with such wells, very dangerous to cattle. By and by the formerly cylindrical well begins to extend in the direction in which the underground water flows, and a narrow ravine grows until it joins the main valley. Then masses of loess continually fall down into the ravine, increasing its width. The fall of these masses is favoured by the numerous crevices in the loess, and it is so frequent that natives warn foreigners not to approach the borders of a ravine. Of course the fallen masses are further dislocated by water, and the loess is thus steadily transported at a remarkable speed to lower levels.

HITHERTO it has been generally supposed that the glaciers of the Caucasus are far from having the same development as those of the Alps. It appears, however, from the last researches of Abich, that, although no glaciers of the Caucasus are as long as the Aletsch and Unteraar glaciers, or the Mer de Glace, there are a great many of them. From tables compiled by M. Smirnoff in a recent issue of the *Bulletin* of the Moscow Naturalists' Society, it appears that the average lowest levels of the Caucasus glaciers are: 2504 metres in the Elburz Chain; 2176 metres in the chain to the west of the Adai-kokh; 2266 metres in the high valley of the Ingur; 2898 metres on the eastern slope, and 2238 on the northern slope, of the Kazbek; from 2428 to 2658 metres in Daghestan; 2776 metres on the Great Ararat; and as much as from 3162 to 3194 metres on the Shah-dagh. Comparing these heights with those reached by the lower extremities of glaciers in other highlands, M. Smirnoff concludes that in the main Caucasus ridge the altitudes of the snow-line and the glaciers are intermediate between the corresponding altitudes in the Alps and those in the chains of Central Asia (Thian Shan and Hindu Kush); and that in the western parts of the Caucasus the altitudes of the perennial snow-line are nearer to those of the Austrian Alps. There is some analogy between West Caucasus and the Himalayas, inasmuch as the lowest limits of perennial snow in both chains are higher on the northern slope than on the southern.

A "PANORAMA-BIJOU" (or toy panorama), has been recently brought before the French Société d'Encouragement, by M. Benoist. It is meant to give a succession of connected views of photographed scenery, &c. Externally the instrument appears as a cylindrical case with a handle projecting from its curved surface. The observer looks through a lens, in the axis, towards a mirror inclined 45°, which reflects a panoramic view fixed round the interior of an inner cylinder which is rotated by clockwork. The back of the case is of ground glass, admitting diffuse light. The instrument may be found a suitable companion to the stereoscope on the drawing-room table.

FROZEN fish are now imported into France, and a Society formed in Marseilles for the purpose of developing the trade (the Société du Trident) has a steamer and a sailing-vessel engaged in it. The steamer *Rokelle* lately came into Marseilles with some 30,000 kilogrammes of frozen fish in its hold, the temperature of which is kept at 17° C. below zero by means of a Pictet machine (evaporating sulphurous acid). The fish are caught with the net in various parts of the Mediterranean and Atlantic. After arrival they are despatched by night in a cold chamber. Experiment has shown that fish can be kept seven or eight months at low temperature without the least alteration. These fish are wrapped in straw or marine Algæ, and have been sent on to Paris, and even to Switzerland.

AT the establishment of the National Fish-Culture Association, Delaford Park, the American char, *S. fontinalis*, spawned as early as October 15. The thriving capacity of these beautiful fish is becoming yearly more and more marked. Their rate of growth at Delaford has been extraordinarily rapid.

DR. R. BALTZER, Professor of Mathematics at Giessen University, died at Giessen on November 7. He was born January 27, 1818.

ON October 22 a monument to Prof. Oswald Heer was unveiled in the Zürich Botanical Gardens. The bust of the great Swiss naturalist has been executed in a masterly manner by Prof. Hoerbst.

THE additions to the Zoological Society's Gardens during the past week include fifty-nine Pleurodele Newts (*Molge walti*), seven Marbled Newts (*Molge marmorata*) from Spain, presented by the Lord Lilford, F.Z.S.; two Moufflons (*Ovis musimon* ♂ ♀) from Sardinia, two Barbary Wild Sheep (*Ovis tragelaphus* ♂ ♀) from North Africa, two South American Flamingoes (*Phœnicopterus ignipalliatus*) from South America, deposited; ten Silky Bower Birds (*Ptilonorhynchus violaceus*) from New South Wales, eight received in exchange, and two deposited; an African Wild Ass (*Equus tæniopus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

AMERICAN OBSERVATORIES.—It is reported that the Dearborn Observatory of the Chicago Astronomical Society is to be removed to Evanston, Ill., the North-Western University at Evanston having made an advantageous offer with respect to it. A large astronomical Observatory is proposed to be erected in connection with the Lake Forest University, Governor Ross,

President of the Board of Trustees of the University, having guaranteed the cost. A new Observatory has been established at Smith College, Northampton, Mass., and Miss Mary E. Byrd, formerly assistant at Carleton College Observatory, has been appointed Director. The equipment of the Observatory at Carleton College, Northfield, Minn., is proceeding rapidly, the new meridian-circle by Repsold is already erected, and one of the two large steel domes is in place. The telescope it is to cover, an $8\frac{1}{4}$-inch refractor by Alvan Clark, will, it is expected, be ready for use within a few days. Mr. Grinnell, the founder of Grinnell, Iowa, has furnished funds for the erection of an Observatory to be attached to the Iowa College, and the building is being rapidly brought to completion. The new Observatory is to have an 8-inch equatorial by the Clarks. Prof. Asaph Hall is to act as the Consulting Director of the Washburn Observatory, whilst Prof. George Comstock will have the more immediate superintendence of the institution as Associate Director.

U Ophiuchi.—Mr. S. C. Chandler gives, in No. 162 of *Gould's Astronomical Journal*, an investigation of the light-curve of this well-known Algol-type variable, the result of which seems to indicate a curious but well-marked retardation in the increase of brilliancy some half-hour or so after minimum is passed. A similar irregularity has been noticed in the light-curve of S Cancri, and occasionally in that of Algol. It is clearly of great importance to ascertain whether this is merely subjective, due to some habit of observation, or a real peculiarity of the star itself. If the latter, it would throw considerable doubt on the satellite theory, which at present seems on the whole the most plausible explanation of variability of the Algol type.

The New Algol Variables.—Mr. Chandler also gives an ephemeris for the minima of the two new Algol-type variables, viz. R Canis Majoris, R.A. 7h. 14·3m., Decl. 16° 11′ S., and Y Cygni, R.A. 20h. 46·6m., Decl. 34° 10′ N., as follows:— Y Cygni, Nov. 26, 22h. 42·5m.; Nov. 29, 22h. 36·1m.; Dec. 2, 22h. 29·7m. R Canis Majoris, Nov. 29, 18h. 48·3m.; Nov. 30, 22h. 4·2m.; Dec. 2, 1h. 20·1m. Greenwich civil time, reckoning from midnight to midnight.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *NOVEMBER* 27—*DECEMBER* 3.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 27

Sun rises, 7h. 40m.; souths, 11h. 47m. 44·8s.; sets, 15h. 56m.: right asc. on meridian, 16h. 12·0m.; decl. 21° 8′ S. Sidereal Time at Sunset, 20h. 21m.

Moon (Full on November 30, 15h.) rises, 15h. 5m.; souths, 21h. 52m.; sets, 4h. 50m.*: right asc. on meridian, 2h. 17·7m.; decl. 8° 30′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	5 52	10 39	15 26	15 3·4	14 39 S.
Venus.....	3 10	8 46	14 22	13 9·8	5 23 S.
Mars......	0 59	7 21	13 43	11 45·1	3 33 N.
Jupiter....	6 15	10 49	15 23	15 12·8	17 1 S.
Saturn....	20 26*	4 13	12 0	8 35·8	19 2 N.
Uranus...	3 1	8 36	14 11	12 59·5	5 39 S.
Neptune..	15 39	23 20	7 1*	3 46·6	18 7 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Dec.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
1	119 Tauri	$5\frac{1}{2}$	16 28	17 18	65 248
1	120 Tauri	6	17 0	17 46	38 272

Dec. h.
... 9 ... Venus at greatest elongation from the Sun, 47° west.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Nov. 27,	1 27	*m*
			Dec. 2,	1 6	*m*
Algol	3 0·8	40 31 N.	,, 3,	5 57	*m*
λ Tauri	3 54·4	12 10 N.	Nov. 29,	4 32	*m*
			Dec. 3,	3 25	*m*
U Monocerotis	7 25·4	9 33 S.	,, 1,		*m*
S Cancri	8 37·5	19 26 N.	,, 3,	0 41	*m*
S Boötis	14 19·3	54 20 N.	Nov. 29,		*M*
β Lyræ	18 45·9	33 14 N.	Dec. 1,	22 0	*M*
R Lyræ	18 51·9	43 48 N.	,, 1,		*M*
η Aquilæ	19 46·7	0 43 N.	,, 2,	2 0	*M*
S Sagittæ	19 50·9	16 20 N.	,, 1,	5 0	*m*
δ Cephei	22 25·0	57 50 N.	Nov. 30,	0 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near λ Persei	60	50 N.	Very swift.
α Can. Ven.	194	42 N.	Very swift; streaks.

GEOGRAPHICAL NOTES.

At the International Exhibition to be held at Brussels next year, a special Section will be devoted to topography, cosmography, geography, and the related sciences. The following are the classes of objects desired for contribution to the Section:—(1) Maps and atlases, topographical, geographical, geological, hydrographical, astronomical, &c.; (2) physical maps of all kinds, plans in relief, terrestrial and celestial globes and spheres; (3) statistical works and diagrams, tables and ephemerides for the use of astronomers and navigators; (4) general, historical, and classical works; (5) instruments, aide-memoires, and articles of equipment for explorers. Among the "desiderata" are the following:—(1) The best map of the Congo, showing the most recent discoveries; (2) the best national map of any country; (3) utilization of the sheets of a topographical map for the preparation of special maps on the same or a different scale; (4) the execution of relief-maps; (5) transference of relief to a plane surface; (6) construction of an apparatus suitable to demonstrate by experiments the various geographical features which may be presented by a river, such as torrents, lakes, cataracts, and rapids, erosions and alluvial accumulations, subterranean streams, islands, and backwaters, freezing and breaking up of ice, floods, deltas, bars, &c.; (7) construction of a tellurium; (8) portable equipment for an explorer; (9) statistical atlases and globes. The Secretary of the Section is Prof. Du Fief, 22 Rue des Palais, Brussels.

In the *Verhandlungen* of the Berlin Geographical Society, No. 8, Dr. Mense describes in some detail a journey up the Kwango, the great southern tributary of the Congo, which he made last December in company with the Rev. G. Grenfell. It contains a good deal of local information.

The November number of the *Alpine Journal* contains Mr. D. Freshfield's diary during his recent visit to the Caucasus, when he ascended some of the highest peaks, and visited some of the principal glaciers. The diary itself and the many excellent illustrations of the peaks and glaciers visited will be found to afford useful geographical information.

At the last meeting of the Paris Geographical Society, Dr. Verneau described the results of his recent missions to the Canary Islands. His special aim was to work out the ethnology of the islands, and for that purpose he has collected many skulls and bones from caves and graves, and made many observations on the present inhabitants. The Guanches he professes to recognize as the direct descendants of a people the type of which is exhibited in the famous prehistoric Cro-Magnon skull—the troglodytes of the Vézère. He maintains that about the end of the Quaternary there must have been a great migration of what he calls the "Cro-Magnon" race from the north to the south, and a section of the migrants found their way to the Canaries. After a lapse of time these were invaded by Numidians and Semites from the north of Africa, people of a superior type and more advanced culture to the Guanches, who were troglodytes. Dr. Verneau has made many collections of anthropological interest from the Canaries, and these are likely to be of much more service to science than his theories.

METEOROLOGICAL NOTES.

MR. H. ALLEN has contributed an article to the *American Meteorological Journal* for October, on the behaviour of pressure and temperature in low and high pressure systems. Recent investigations by M. Dechevrens (and others) tend to show that, while a high temperature accompanies a low pressure at sea-level, the fluctuations are reversed at some height above sea-level. Mr. Allen maintains that this conclusion is not supported by his examination of observations made on Mount Washington, where the minimum pressure does not coincide with the passage of the storm centre over the station, but lags about eleven hours behind it, and he considers that this fact explains the peculiar results obtained by M. Dechevrens. The same number also contains an article by Prof. F. Waldo, "On the Absolute Reduction of Wind Observations at Sea." He recommends the use of some instrument to assist the judgment of different observers, at the actual time of observation.

THE results of meteorological observations made at the Radcliffe Observatory, Oxford, in the year 1884, contain daily means of eye observations and of the self-recording instruments, comparisons of the mean monthly temperatures at 5 and 105 feet above the ground, and rainfall observations on the ground at 22 and 112 feet. Interesting tables are given showing the relations of pressure, temperature, &c., under different winds. The total sunshine during 1884 was 1260·9 hours, being 173·7 hours less than the mean of five years. The observations are reckoned for astronomical and for Greenwich mean time.

ON September 19, 1887, the Russian Government gave notice that storm signals (consisting of day and night signals) would be made at their principal ports in the Black Sea. The signals are shown for forty-eight hours, unless instructions are received to lower them before that time has elapsed: also, the cause assigned for hoisting each signal will be posted up at the respective signal stations. The day signals consist of a cone, hoisted either alone, or with a drum, both painted black, and each about 3 feet in diameter. The night signals consist of three *red* lights, hoisted at the angles of an equilateral triangle, of the same size as the cone used by day. These signals correspond to those in this country—except that the drum is not now used, and night signals are only exhibited at very few stations.

IN *La Nature* of November 12 M. Jules Girard contributes an article entitled "The Probable Temperature of the Pole," based upon the results of the circumpolar expeditions of 1882–83, and upon the observations of some earlier expeditions, in which he has tabulated the mean temperatures for each month. From these data the author traces two principal centres of intense cold, one in the north of Siberia near the mouths of the Lena, and the other to the north of Hudson's Bay, near Boothia. The lowest mean temperature quoted for July is 30° at Jeannette Island, to the north of the islands of New Siberia, and the lowest mean for January is − 49° at Fort Yukon, Alaska.

THE BRITISH ASSOCIATION AND LOCAL SOCIETIES.

THE third Annual Conference of Delegates of Corresponding Societies was held at Manchester, thirty-two of these affiliated Societies having nominated Delegates to attend the meeting. The following Report of the Conference, signed by Mr. Francis Galton as Chairman, and by Prof. R. Meldola as Secretary, has just been issued:—

At the first meeting of the Conference the chair was taken by Prof. W. Boyd Dawkins, F.R.S., the Corresponding Societies Committee being represented by Dr. J. G. Garson and Prof. R. Meldola, F.R.S., Secretary.

The Chairman, in opening the proceedings, stated that the British Association was anxious to be brought into as close a relationship as possible with the local Societies of this country. The work carried on by many of these Societies was of the greatest value to science, and it was felt that their efforts might be promoted by simplifying and unifying their labours. The present meeting was called for this purpose, and for that of bringing together the representatives of the various Corresponding Societies.

The Secretary read the Report of the Corresponding Societies Committee which had been presented to the General Committee of the Association at the meeting on Wednesday, August 31.

The names of the Delegates who desired to be attached to the Sectional Committees as "Delegate Members" were collected by the Secretary in accordance with the resolution passed at the Conference last year at Birmingham (see last Report, NATURE, vol. xxxv. p. 78).

The Chairman called upon the Delegates to make any statements respecting the action that had been taken by their Societies with reference to the suggestions put forward last year, and which had been embodied in the Report just read.

Prehistoric Remains Committee.—Mr. J. W. Davis stated that the Prehistoric Remains Committee had been carrying on their work during the past year, and they proposed to apply for reappointment. Two reports had already been obtained relating to the bronze implements of the East and West Ridings of Yorkshire, and several others had been promised for next year.

Preservation of Stonehenge.—With reference to the preservation of Stonehenge, Dr. Garson stated that the resolution which had been submitted last year to the Delegates at the Birmingham Conference had been considered by the Committee of Section H, and, having been adopted by them, had been brought before the General Committee, and also accepted. He believed that in consequence of this action negotiations were now going on between the Council of the British Association and the proprietor of these remains.[1]

Prof. Boyd Dawkins remarked that the state of neglect into which Stonehenge had been allowed to fall had by no means been overstated in the resolution referred to. A person had recently been seen on a ladder chipping off pieces from the horizontal stone of one of the trilithons.

Ancient Monuments Act.—The Chairman and Dr. Garson made some remarks in explanation of the working of the Ancient Monuments Act. It was pointed out by the latter that the local Societies could do good service by inducing the proprietors of prehistoric remains to communicate with General Pitt-Rivers, the Inspector of Ancient Monuments, with the object of placing these remains under Government protection. The Chairman urged those Delegates who represented the Northern, and especially the Scotch Societies to use their influence in inducing the owners of ancient remains to assist in carrying out the objects of the Act. In reply to a question by Mr. F. T. Mott, as to whether camps and earthworks were to be taken into consideration, the Chairman did not think that any Government could be expected to become a landowner to the extent of all the earthworks in the country.

Provincial Museums Committee.—With reference to the work of this Committee, Mr. F. T. Mott stated that they had been engaged during the past year in collecting particulars respecting museums other than those in London. Considerable assistance had been given by the Secretaries of many of the local Societies. If the Committee was reappointed, as he hoped it would be, he thought there were one or two matters on which the local Societies might possibly render still more valuable aid. The Report of the Committee was not yet passed, but it would, no doubt, be read in the course of the present meeting of the Association, and would then be accessible.

Prof. Boyd Dawkins stated that the schedule issued by this Committee was a very difficult one to fill up, and he expressed a hope that something shorter and simpler would be sent out.

The Rev. H. Winwood expressed similar views.

Mr. Robert Pullar and Mr. J. W. Davis mentioned two museums which the Committee had not heard of—viz. that of the Perthshire Society of Natural Science at Perth, and Mr. Davis's museum at Chevinedge, Halifax.

Earth Tremors.—Prof. Lebour stated that the subject of earth tremors, which he had brought forward at the Conference of Delegates last year at Birmingham, had since taken a more

[1] The following extract relating to this matter is from the Council Report for 1886–87, presented at the Manchester meeting:—

"That the Council be requested to consider the advisability of calling the attention of the proprietor of Stonehenge to the danger in which several of the stones are at the present time from the burrowing of rabbits, and also to the desirability of removing the wooden props which support the horizontal stone of one of the trilithons; and in view of the great value of Stonehenge as an ancient national monument, to express the hope of the Association that some steps will be taken to remedy these sources of danger to the stones."

The Council have carefully considered the question, and, having had the advantage of perusing the detailed report recently prepared by a deputation of the Wilts Archæological and Natural History Society on the condition of the whole of the stones constituting Stonehenge, are of opinion that the proprietor should be approached with the expression of a hope that he will direct such steps to be taken as shall effectually prevent further damage.

practical shape, and that it now seemed to be time that a Committee of the British Association should be formed for taking the investigation in hand. Through the advocacy of Mr. Symons, who was unable to be present at the Conference, Sections A and G had that morning agreed to recommend the appointment of such a Committee in conjunction with Section C, which Section would be approached next day. The work to be done was of a preliminary character, and its object was rather to inquire into the best methods of conducting observations on earth tremors than to actually cause such observations to be made. The North of England Institute of Mining and Mechanical Engineers had, since the Birmingham meeting, carried on a series of seismoscopic observations at Marsden in the county of Durham; and the daily results, extending over several months and compared with a barometric curve, were shown to the meeting in the form of a diagram by Mr. Walton Brown, the Secretary of the Newcastle Institute Committee. The Institute possessed also a more elaborate instrument, made after a pattern supplied by Prof. Ewing, which registered the intensity and direction of the tremors. Prof. Lebour stated that, although such instruments as the last mentioned were probably too costly to be placed at all desirable stations, this would not be the case with the simpler seismoscope, which recorded merely the fact of earth tremors having taken place and the time of their occurrence. Such records would be valuable, though limited. The Corresponding Societies, if they would interest themselves in the matter, might be the means of establishing a great network of seismoscopes with a few seismographs in suitable localities, and results of value would by this means be in all probability obtained. These results would be valuable altogether in proportion as well-equipped seismometrical observing stations were numerous. The expense must in any case be considerable in the aggregate, but need not be great in individual cases. A sufficiently good seismoscope might be had for about £2, a seismograph for £14 to £15, and the cost of keeping them in order would not be great. Prof. Lebour hoped the Delegates present would help in establishing such a network of observing stations all over the country, and he stated, in concluding, that he would be happy to communicate with anyone interested in the subject.

Prof. Ewing, in response to the Chairman, said that from his experience of earth-tremor observations in Japan he could concur in the remarks of Prof. Lebour. To investigate fully the character of the motion, even at one station, required delicate and costly apparatus, and the cost was greatly increased when it was attempted to bring a number of stations into correspondence so as to determine the motion over a large area. It was possible, however, to record the fact that a tremor had occurred, and even to learn something of its character by means of inexpensive seismoscopes; and it certainly seemed to him that no bodies could more appropriately undertake that work than the local Societies represented at the Conference acting in conjunction with a Committee of the Association. From recent observations it appeared probable that tremors would be found wherever they were tested for with sufficient delicacy, so that a Society undertaking the search was not likely to be disappointed.

At the second Conference the chair was taken by Prof. Boyd Dawkins, F.R.S., who was succeeded by Mr. W. Topley, the Corresponding Societies Committee being further represented by Mr. G. J. Symons, F.R.S., Dr. Garson, Mr. William White, and Prof. R. Meldola, F.R.S., as Secretary.

The Chairman invited discussion on the recommendations received from the various Sections.

Section A.

Temperature Variation in Lakes, Rivers, and Estuaries.—The following resolution was forwarded to the Secretary of the Conference by the Secretary of this Section:—

"That Mr. John Murray, Prof. Chrystal, Dr. A. Buchan, Rev. C. J. Steward, Hon. R. Abercromby, Mr. J. Y. Buchanan, Mr. David Cunningham, Mr. Isaac Roberts, Dr. H. R. Mill, and Prof. Fitzgerald be appointed a Committee to arrange for an investigation of the seasonal variations of temperature in lakes, rivers, and estuaries in various parts of the United Kingdom in co-operation with the local Societies represented at the Association; and that Mr. John Murray be Secretary."

Dr. H. R. Mill, as representing this Committee, stated that the question proposed had not been fully worked out, but that the few observations made showed relations of a very interesting kind. As a branch of meteorology, this research was particularly promising, and was one in which the co-operation of local Societies would be valuable. He proposed that the Societies situated in the neighbourhood of rivers and estuaries which were willing to undertake this work should appoint some member to observe the temperature daily or weekly, as the case might be, in accordance with the rules to be drawn up by the Committee. It was first proposed to ascertain how many observers would offer themselves in various parts of the country, then to draw up a scheme of observations and arrange for this being adopted.

Mr. G. J. Symons pointed out the necessity in such observations for having a well-considered scheme drawn up, as well as for having absolutely reliable thermometers, without which no observations would be of value. He also asked whether it was proposed that the cost of the instruments should be met by a grant from the British Association, or whether the Societies taking part in the observations should provide their own thermometers.

Mr. De Rance remarked that in the case of the Committee which had been formed for the observation of underground temperatures, and of which Prof. Lebour was a member, the thermometers had been supplied by the Association.

Mr. J. W. Davis raised the question as to whether it would be of use to extend the observations to the streams in manufacturing districts. He also asked what the Committee proposed to consider as an estuary.

The Rev. H. Winwood remarked that it would be necessary in all cases to record the depth at which the thermometer reading was taken. As a point of interest bearing upon the proposed observations, he stated that it had been observed that the temperature of the lakes in the Hebrides had been unusually high this year.

Prof. Lebour stated that the thermometers used by the Underground Temperature Committee had been supplied by the Association, but these instruments were very costly, and only a few observers had taken part in the work. He was of opinion that, if numerous Societies took part in the observations, these should in each case bear the expense.

Dr. Garson expressed a hope that the temperatures would be recorded on the Centigrade scale.

Dr. Mill, in reply, said that he understood that the fact of the investigation being sanctioned by a Committee of Section A was a sufficient guarantee that it should be carried out in a thoroughly scientific manner with properly verified instruments of a uniform pattern, and employed in the same way. The experience of the Scottish Marine Station for three years suggested many precautions which should be adopted in this work. The temperature of streams in manufacturing districts should certainly be ascertained in as many cases as possible, in order to find whether the increase of temperature of a river passing through a manufacturing town is in any sense permanent. The term "estuary" should in his opinion be used as meaning all parts of a tidal river between the upper limit of the tide and the open sea. Each local Society should be asked to supply its own thermometers, but all these should be verified at Kew, or compared by some person appointed by the Committee. The observations would, of course, be made on a uniform plan, and it would, probably, be found more convenient to use the Fahrenheit scale, but the readings could be easily converted, if necessary.

Section C.

Mr. C. E. De Rance, who represented this Section, referred to the work of the three Committees which he had brought under the notice of the Delegates on former occasions, viz.: (1) The Underground Waters Committee; (2) The Erratic Blocks Committee; and (3) The Sea Coasts Erosion Committee. (See last Report.)[1]

The first of these Committees requires information as to the depth of wells, the sections passed through, the height at which the water stands before and after pumping, daily records of the height and chemical analyses of the waters.

The *Erratic Blocks Committee* wants information as to the position, size, and character of boulders of foreign origin that

[1] The constitution of these Committees remains as last year. The Secretaries are:—
Underground Waters, C. E. De Rance, 28 Jermyn Street, London, S.W.
Erratic Blocks, Rev. H. W. Crosskey, 117 Gough Road, Edgbaston, Birmingham.
Sea Coasts Erosion, Wm. Topley, 28 Jermyn Street, London, S.W.
The schedules and all other information will be furnished on application at the above addresses.

may occur in drift-covered areas, and are anxious that the position of the same should be noted on the 1-inch map of the Ordnance Survey.

The *Sea Coasts Erosion Committee*, like the other two Committees, has a circular form of inquiry, which can be obtained on application to Mr. Topley.

With reference to the work of this last Committee, Mr. Topley stated that but little assistance had as yet been received from the local Societies. The Natural History Society of the Isle of Man had undertaken to collect information; and all similar Societies in maritime counties might greatly assist the Committee by local observation as to present changes, and by researches as to past conditions of the coast.

With respect to the work of the Erratic Blocks Committee, Prof. Meldola said that he had been authorized to state, on behalf of the Manchester Geological Society, that several members of that Society had been interesting themselves in the distribution of boulders in their district, and it was expected that their results would be available by the next meeting of the Association. It was also mentioned that Mr. Adamson had been rendering assistance to this Committee on behalf of the Yorkshire Naturalists' Union.

Mr. Ralph Richardson, as the representative of the Edinburgh Geological Society, pointed out that Scotland had been omitted from the localities dealt with by the Erratic Blocks Committee. He stated that much work in this field had already been carried out under the auspices of the Royal Society of Edinburgh, and he hoped the Committee would be able to utilize their results.

Earth Tremors Committee.—Prof. Lebour stated that since the last meeting of the Conference the formation of a Joint Committee by Sections A, C, and G has been agreed to, and the resolution forwarded to the Committee of Recommendations. The resolution was the following:—

"That Sir F. J. Bramwell, Mr. E. A. Cowper, Mr. G. J. Symons, Prof. G. H. Darwin, Prof. Ewing, Mr. Isaac Roberts, Mr. Thomas Gray, Dr. John Evans, Prof. Lebour, Prof. Prestwich, Prof. Hull, Prof. Meldola, and Prof. Judd be a Committee for the purpose of considering the advisability and possibility of establishing in other parts of the country observations upon the prevalence of earth tremors, similar to those now being made in Durham in connection with coal-mine explosions, and that Prof. G. A. Lebour be the Secretary."

Mr. Symons and Mr. Topley made some remarks on the work of this Committee.

Mr. De Rance remarked that the proposed observations might possibly under certain circumstances become connected with the work of the Underground Waters Committee. Thus the Essex earthquake of April 22, 1884, had caused a rise in the level of the water in Messrs. Courtauld's well at Bocking, which had reached its maximum in June of the same year. Since then the level had been gradually falling, and at its present rate it might be expected that the water would be at the same level as it was before the earthquake about next August.

Section D.

Life-Histories of Plants.—Prof. Meldola said that during a recent visit to Oxford he had had an opportunity of hearing a suggestion in the course of a conversation with Prof. Bayley Balfour, which had appeared to him as likely to be of use to the members of local Societies. He had therefore invited Prof. Balfour to attend the Conference and explain his views on the suggested subject, but as that gentleman was prevented from being present he had forwarded the following communication:—

"It appears to me that much good scientific work might be done by members of local Societies in a direction which has not attracted so much attention in Great Britain as it deserves. The discovery and description of new forms, and the distribution of our indigenous plants, are in botany the lines upon which most of the energies of local Societies are principally spent, whilst habit, construction, and generally the features of life-history of plants come in for attention in quite a secondary way. This arises, I think, in great part from the prevalent notion that the facts of the life-history of our common plants are all well known, and that there is little, if anything, more to find out about them. That this is an erroneous idea may easily be shown—witness, for example, the interesting observations recently published by Sir John Lubbock—and there is a field for a great deal of sound work upon plants growing at our doors.

"Within recent years Mr. Darwin's work, followed up by that of such men as Hermann Müller, Kerner, Ogle, and others, has given a stimulus to observations of adaptations between the vegetable and animal kingdoms in connection with pollination in flowers; and many interesting facts about British plants have been brought to light by workers in local Societies. But little has been done for the subject of the vegetative organs of these plants—I mean the arrangement, true nature, and structure of the members that carry on plant-life. In Germany, many years ago, Wydler and Irmisch published a splendid series of contributions to the knowledge of these features in indigenous German plants—why has this not been done for Britain?

"Now, I venture to think that good results would follow if you would bring before the Delegates at the meeting to-day the importance of encouraging the members of their Societies to study the life-histories of indigenous plants in their entirety, *i.e.* from the stage of embryo in the seed up to the production of fruit and seed again. Anyone who will take up this line of study will assuredly derive great pleasure from it, and will be able to add a great deal to the sum of our knowledge of plant-life. Such work can be well combined with the more usual systematic work; it can be easily accomplished, and it will be found to give much additional interest to the study of British botany."

Mr. C. P. Hobkirk considered that Prof. Balfour's letter was a very important one, and that, as therein suggested, the time and energies of the members of local Societies would be far more usefully employed by following the lines indicated by Prof. Balfour than, as at present, in simply collecting, naming, and registering local plants. As far as he was concerned, he was prepared personally, and also on behalf of the Yorkshire Naturalists' Union, which he represented, to do everything in his power to assist in carrying out practically Prof. Balfour's most useful proposition. Although the compilation of local floras was most useful and necessary work, yet the actual life-history of individual forms was now of really paramount importance, and members of local Societies should be urgently requested to carry on this work without delay.

Section H.

Ancient Monuments Act.—The Secretary read the following communication from General Pitt-Rivers:—

"I am much afraid I shall not be able to be present at the meeting of Delegates of local Societies on Tuesday; but the subject is so important for the preservation of these monuments that in case I am not there I write in order that you may know what my view of the matter is.

"Perhaps I cannot do better than state in a few words what the work of the Inspector of Ancient Monuments is, and you will then see what kind of progress is likely to be made without some assistance such as has been proposed,[1] and in what way the assistance of local Societies can be given.

"You are probably aware that in the original Act of 1882 fifty ancient monuments in Great Britain were scheduled as monuments to which the Act could apply at once if the owners were willing. Some persons suppose that by scheduling these monuments they were actually placed under the Act, but this is not the case. The scheduling was done without the knowledge or consent of the owners, and their consent had to be obtained both for these and for every other monument that has been since added to the list. This has entailed the examination and survey of all these monuments which are distributed over England, Scotland, and Wales. The addresses of the owners had to be obtained, and this could only be done on the spot. After that the owners had to be visited personally, for I soon found an official letter, without a verbal explanation, almost invariably produced a refusal. On this account I have of late found it advisable never to approach an owner without a personal introduction, or without doing it in such a way as to induce him to consider the matter favourably. This mode of procedure for the whole country has, of course, taken a long time, and the result has been that about half of these fifty monuments have been voluntarily put under the Act by their owners, and of the remainder some of the proprietors have refused, whilst in the case of others it has been found impracticable owing to peculiarities in the ownership. All the monuments have, however, been carefully surveyed, planned, and drawn, and in every case in which there has been a refusal the owners have stated their intention of taking good care of the

[1] This refers to the work of the Prehistoric Remains Committee of the British Association.

monuments themselves. In one case only a camp has been partly damaged, and this owing to mining operations involving a question of a large sum of money which made it impossible for the Government to interfere. Other non-scheduled monuments have since been added to the list, and the number is steadily but not rapidly increasing.

"The Government makes no allowance for an assistant; not even so much as a man to hold the end of the tape in measuring, without which no proper survey of the monuments can be made, and I have to employ a private assistant, whom I take about with me at my own cost. With his assistance, and by dividing the work with him—I making the necessary notes and measurements while he is drawing—each monument takes on an average about one day; without an assistant the time would be about doubled. After this the owner has to be visited, and as he generally lives at a distance from the monument, this frequently takes another day or more. A great deal of this time might be saved by the assistance of persons living in the localities and with better chance of success.

"I issued a circular to a number of local Societies inviting them to co-operate, but few responded. One instance, however, shows what may be done in this way. Sir Herbert Maxwell has not only sent me the addresses of several owners in Wigtonshire and Kirkcudbrightshire, but, by using his influence with these, has been the means of placing several monuments under the Act. I would suggest that the same course might well be followed by others.

"The recommendation I would make is this:—Local Societies should (1) report to me what monuments in their district they think worthy of being put under the Act; (2) they should send me the names and addresses of the owners; (3) they should communicate with the owners, and, if possible, obtain their consent to have the monuments placed under the Act, subject, of course, to their subsequent acceptance by the Office of Works; and (4) they should report to me any damage that they find being done or contemplated either to the monuments under the Act, or to others not so protected. With such assistance I think that much more rapid progress may be made."

Prehistoric Remains Committee.—Mr. J. W. Davis stated that this Committee had been recommended for reappointment by the Committee of Section H. The recommendation is as follows:—

"That Sir John Lubbock, Dr. John Evans, Prof. Boyd Dawkins, Dr. R. Munro, Mr. Pengelly, Dr. Hicks, Mr. J. W. Davis, Prof. Meldola, and Dr. Muirhead be reappointed a Committee for the purpose of ascertaining and recording the localities in the British Islands in which evidences of the existence of prehistoric inhabitants of the country are found; and that Mr. J. W. Davis be the Secretary."

Prof. Lebour suggested that it would be convenient if, in registering prehistoric remains, the Committee would adopt a uniform scheme of signs—if possible, an international one.

Mr. William Gray stated that the work of registering ancient remains had been carried on for twenty-five or thirty years by members of their Society (Belfast Naturalists' Field Club) and others in Ireland, and they had long felt the want of some central organization such as that of the present Committee. He also alluded to the necessity for a uniform system of signs.

Mr. William White remarked upon the difficulty which private individuals often experienced in approaching the proprietors of ancient remains, and pointed out that individual efforts would be likely to be more successful if members of local Societies could make overtures backed up by the sanction of a British Association Committee such as the present one.

Work of the Corresponding Societies Committee.—The Secretary stated that during the present meeting of the Association an important resolution had been framed at the instigation of Sir Douglas Galton, with the object of extending the powers of their Committee. According to the present rules the Committee was nominated by the Council and appointed by the General Committee, but they had no power of submitting resolutions or recommendations to the Committee of Recommendations or to the General Committee. The present resolution, which was calculated to give them the necessary power, and thus to put them on the same footing as the Committees of the Sections, was as follows:—

"That the Conference of Delegates of Corresponding Societies be empowered to send recommendations to the Committee of Recommendations for their consideration, and for report to the General Committee."

The Secretary had succeeded that morning in getting this resolution passed by the Committees of Sections B and C, and it had been forwarded by them in due form to the Committee of Recommendations, by whom it had also been accepted. It was subsequently submitted to the General Committee, and accepted by them on the understanding that the recommendations so forwarded should not clash with the recommendations sent up by the Sectional Committees.

The Secretary remarked that he would take the present opportunity of explaining away a misunderstanding that had arisen on the part of some of the local Societies. Some of these had nominated Delegates to attend the Manchester meeting without having previously submitted any claim for election as Corresponding Societies. Such Delegates could not be officially recognized by the Association, as it was only those Societies which had been admitted as Corresponding Societies, and which were still on the list, that were thus entitled to be officially represented. According to the Rules no Society can be admitted without first sending in a formal application, accompanied by a specimen of its publications; this application would be considered by the Corresponding Societies Committee, and only in the event of the Society being recommended for election by this Committee, and this recommendation confirmed by the General Committee, would it be admitted to the privileges of a Corresponding Society.

At the termination of the meeting a vote of thanks was passed to Prof. Meldola, on the motion of Prof. Lebour, for the services which he had rendered as Secretary to the Committee and to the Conferences.

THE METEOROLOGY OF OXFORD.[1]

THE forty-second volume of the Observations of the Radcliffe Observatory has recently been published, and is in nearly all respects a continuation of the previous publications. The Radcliffe takes precedence of all our British Observatories as regards the length of time over which the published hourly observations of atmospheric pressure and temperature extend; to which is to be added a commendable fullness, far from common, with which many other observations have been made and given to the public for a long term of years.

At Oxford, atmospheric pressure attains the maximum, 29·760 inches, in June, and falls to the minimum, 29·677 inches, in March, to which the mean of October, 29·680 inches, closely approximates. The annual mean is 29·720 inches; the highest during the previous thirty years being 29·785 inches in 1858, and the lowest 29·572 inches in 1872, the year to be long remembered for its excessive rainfall. Temperature rises to the maximum, 61°·7, in July, and falls to the minimum, 38°·8, in January, the annual mean being 49°·2. The warmest year was 1868, with a mean of 51°·4, and the coldest mean 45°·5 in 1879. Of individual months, the warmest was July 1859, the mean of which was 66°·5, while the mean for February 1855 was only 29°·5, giving thus a mean monthly range of 37°·0. The rainfall reaches the maximum, 2·81 inches, in October, and falls to the minimum, 1·62 inch, in March, and the mean annual amount is 26·42 inches. The extreme annual amounts were 40·42 inches in 1852 and 17·56 inches in 1870. The month of heaviest rainfall was October 1875, when 7·53 inches fell, and the lightest fall was 0·18 inch in September 1865, when temperature was unusually high for the season.

The diurnal curves of pressure approach closer than those of any other British Observatory of which we have records to the seasonal phases of these curves for continental situations. On the mean of the year, the first minimum occurs about 4 a.m., and the maximum at 9 a.m.; and the second minimum at 3.30 p.m. and maximum at 10 p.m.,—the former being earlier in summer and later in winter, whereas the afternoon phases are the reverse of this. In June the time between the first and second maximum is 14½ hours, but in winter only 12 hours.

Of quite exceptional interest are some of the other diurnal phenomena at Oxford, notably the diurnal distribution of thunderstorms, sheet lightning, and auroras. We have compiled the following table showing the sums of the times of occurrence

[1] "Results of Meteorological Observations made at the Radcliffe Observatory, Oxford, in the Year 1884, under the Superintendence of E. J. Stone, F.R.S." (Oxford, 1887.)

of these phenomena during the several hours of the day for the twenty years ending 1884:—

Hours.	Thunder-storms.	Sheet lightning.	Auroras.	Hours.	Thunder-storms.	Sheet lightning.	Auroras.
	Summer—June, July, August.		Year.		Summer—June, July, August.		Year.
Midt. to 1 a.m.	9	14	10	Noon to 1 p.m.	26	0	0
1 to 2 a.m.	5	4	2	1 to 2 p.m.	24	0	0
2 to 3 a.m.	5	1	2	2 to 3 p.m.	21	2	0
3 to 4 a.m.	6	1	1	3 to 4 p.m.	29	2	0
4 to 5 a.m.	4	0	0	4 to 5 p.m.	17	2	0
5 to 6 a.m.	6	0	1	5 to 6 p.m.	22	4	5
6 to 7 a.m.	4	0	1	6 to 7 p.m.	22	3	10
7 to 8 a.m.	7	1	0	7 to 8 p.m.	5	12	26
8 to 9 a.m.	5	0	0	8 to 9 p.m.	3	22	31
9 to 10 a.m.	8	0	0	9 to 10 p.m.	5	41	27
10 to 11 a.m.	7	1	0	10 to 11 p.m.	5	40	25
11 to noon.	21	1	0	11 to midnight	5	26	16

Thus the daily maximum for thunderstorms is from about noon to 7 p.m., being the period of the day covered by the afternoon minimum of atmospheric pressure in summer; but the maximum for sheet lightning is from 8 p.m. to midnight, being the period embraced by the afternoon maximum of pressure. The absolute daily maximum for sheet lightning, it will be observed, does not occur till from 9 to 11 p.m., or till some time after dusk, and cannot therefore be accounted for by increased visibility as darkness sets in. The opinion is widespread that sheet lightning is merely the reflection of a distant flash of lightning. The Oxford observations show, however, that only a small percentage of all the cases admit of being explained in this way. In connexion with the well-defined maximum from 9 to 11 p.m. it may be remarked that there is no region of the globe nearer Oxford than America where thunderstorms with the accompanying true lightning have the daily maximum at the same physical time, 9 to 11 p.m. G.M.T., when sheet lightning has its daily maximum at Oxford.

The curve for auroras has its diurnal maximum substantially at the same time as sheet lightning, or during the time of the evening maximum of pressure. The agreement of these two maxima with this portion of the daily curve of pressure is all the closer when it is considered that the evening maximum of pressure is from one to two hours later in summer when the sheet lightning was observed than in the autumn and spring months when the great majority of auroras occur. These results are of the greatest importance with respect to recent theories regarding thunderstorms, and to suggested connexions between the aurora in arctic and sub-arctic regions and the lightnings of low latitudes. The time of occurrence of the maxima of aurora and sheet lightning from 9 to 11 p.m. indicates, perhaps, a more direct connexion between these phenomena and the evening maximum of pressure than has been suspected. This maximum is mainly due to an overflow of upper aërial currents back to eastward from the longitudes to westward, where at the time the afternoon pressure is at the minimum ("Encyc. Britt.," *Meteorology*, p. 122); and hence at these hours there is more aqueous vapour spread through the higher regions of the atmosphere in its gaseous and fluid states, and also in the solid state of minute spicules of ice, even though no cloud in the finest pencilled forms of the cirrus be visible.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Drs. Routh and Glaisher, Prof. J. J. Thomson, and Mr. A. R. Forsyth have been appointed Examiners in Part II. of the Mathematical Tripos of 1888.

The following appointments of Natural Science Examiners have been made:—Physics: Profs. J. J. Thomson and W. G. Adams. Chemistry: Prof. H. E. Armstrong and Mr. H. J. H. Fenton. Mineralogy: Messrs. T. W. Danby and H. A. Myers (British Museum). Botany: Prof. I. B. Balfour and Dr. S. H. Vines. Physiology: Dr. W. H. Gaskell and Prof. G. F. Yeo. Zoology: Messrs. H. Gadow and W. F. R. Weldon. Geology: Prof. C. Lapworth and Mr. A. Harker. Human Anatomy: Prof. J. Cleland and Dr. A. Hill. Pharmaceutical Chemistry: Mr. Pattison Muir.

At a meeting of the Senate in the Arts School recently, general approval was expressed of the scheme for providing a new room for botanical microscopy. The scheme for new anatomical and physiological rooms was not so entirely approved, some persons wishing to retain the ugly old Anatomical Museum and buildings, and also considering that the requirements of the Medical School had not been sufficiently considered.

Mr. W. Bateson, M.A., Fellow of St. John's College, has been elected to the Balfour Studentship.

Group E (Natural Science), in the Higher Local Examination, attracts a diminishing number of candidates, we are sorry to see. Only 36 presented themselves this year as against 73 in 1879; but 10 candidates gained a first class this year, as against 4 in 1879: 35 failed then, only 5 this year. Elementary Biology is reported on fairly this year; but Elementary Chemistry does not seem to have been studied practically, and problems were not satisfactorily dealt with. Only four candidates passed in Physics. The Physiology, Zoology, and Geology papers were well answered; but in Botany the general standard was decidedly low.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, November 2.—Dr. D. Sharp, President, in the chair.—Mr. Stevens exhibited a specimen of *Acidalia immorata*, L., purchased by him some years ago at the sale of the collection of the late Mr. Desvignes. He remarked that specimens of the insect lately captured near Lewes had been described last month by Mr. J. H. A. Jenner as a species new to Britain.—Mr. Adkin exhibited, and made remarks on, a series of male and female specimens of *Arctia mendica* from co. Cork; also, for comparison, two specimens of *A. mendica* from Antrim, and a series of bred specimens from the London district.—Mr. Enoch exhibited a specimen of *Calocoris bipunctatus* containing an internal parasitic larva.—Dr. Sharp exhibited three species of Coleoptera new to the British list, viz. *Octhebius auriculatus*, Rey, found some years ago in the Isle of Sheppey, but described only quite recently by M. Rey from specimens found at Calais and Dieppe; *Limnius rivularis*, Rosenh., found by Dr. J. A. Power at Woking; and *Tropiphorus obtusus*, taken by himself on the banks of the Water of Cairn, Dumfriesshire.—Dr. Sharp also exhibited a *Goliathus* recently described by Dr. O. Nickerl as a new species under the name of *Goliathus atlas*, and remarked that the species existed in several collections, and had been supposed to be possibly a hybrid between *G. regius* and *G. cacicus*.—Mr. Eland Shaw exhibited two species of Orthoptera, which had been unusually abundant this year, viz. *Nemobius sylvestris*, and *Tettix subulatus*.—Mr. E. B. Poulton exhibited the cocoons of three species of Lepidoptera, in which the colour of the silk had been controlled by the use of appropriate colours in the larval environment at the time of spinning up. He said this colour-susceptibility had been previously proved by him in 1886 in the case of *Saturnia carpini*, and the experiments on the subject had been described in the Proc. Royal Society, 1887. It appeared from these experiments that the cocoons were dark brown when the larvæ had been placed in a black bag; white when they had been freely exposed to light with white surfaces in the immediate neighbourhood. Mr. Poulton stated that other species subjected to experiment during the past season afforded confirmatory results. Thus the larvæ of *Eriogaster lanestris* had been exposed to white surroundings by the Rev. W. J. H. Newman, and cream-coloured cocoons were produced in all cases; whilst two or three hundred larvæ from the same company spun the ordinary dark brown cocoons among the leaves of the food-plant. In the latter case the green surroundings appeared to act as a stimulus to the production of a colour which corresponded with that which the leaves would subsequently assume. Mr. Stainton suggested that larvæ should be placed in green boxes, with the view of ascertaining whether the cocoons would be green. It had been suggested that the cocoons formed amongst leaves became brown because the larvæ knew what colour the leaves would ultimately become. The discussion was continued by Mr. Waterhouse, Dr. Sharp, Mr. McLachlan, and others.—Mr. S. Klein read "Notes on *Ephestia kuhniella*," and exhibited a number of living larvæ of the species, which he said

had been recently doing great damage to flour in a warehouse in the East of London.—Mr. A. G. Butler contributed a paper "On the species of the Lepidopterous genus *Euchromia*; with descriptions of new species in the collection of the British Museum."—Lord Walsingham communicated a note substituting the generic name *Homonymus* for the generic name *Ankistrophorus*—which was preoccupied—used in his "Revision of the genera *Acrolophus* and *Anaphora*," recently published by the Society.

PARIS.

Academy of Sciences, November 14.—M. Janssen in the chair.—Note on certain definitions in mechanics, and on the unities in current use, by M. de Freycinet. In supplement to the remarks already made in his treatise on mechanics, the author here deals more fully with the notions involved in such terms as *force, weight, mass, bulk*, and shows that considerable advantage might be gained by slightly modifying the generally accepted unities. Fresh definitions are suggested of the unities of length, volume, weight, force, velocity, &c.—On the state of the potassa present in plants and the soil, and on its quantitative analysis, by MM. Berthelot and André. In continuation of a previous communication on this subject, the authors here study the condition and process of analysis of the potassa in living plants, and in the humus produced by their disintegration.—On waterspouts, by M. D. Colladon. In reply to M. Faye's strictures, the author illustrates his views by means of an instantaneous photograph, showing that under certain conditions two waterspouts may be generated, one ascending, the other descending, and crossing each other.—On MM. Houzeau and Lancaster's "Bibliographie Générale de l'Astronomie," by M. Faye. A well-merited eulogium is passed on the authors of this great work, who have earned the lasting gratitude of astronomers for accomplishing their vast undertaking in such a thoroughly satisfactory manner. The Bibliography constitutes a systematized catalogue of all astronomical publications that have appeared from the remotest times down to the present day. Although not absolutely exhaustive, the omissions do not appear on examination to be very numerous; but unfortunately only 300 copies have been issued of a work which should find a place in every Observatory and in every scientific library in the world. M. Houzeau has enriched the first volume with a valuable philosophic history of astronomy, which will be found extremely interesting, especially to those students who have been unable to follow the recent discoveries of specialists on the state of astronomical science amongst the Egyptians, Assyrians, and other ancient peoples.—New nebulæ discovered at the Observatory of Paris, by M. G. Bigourdan. These discoveries have been made during the years 1884–87 with the equatorial of the West Tower. Most of the nebulæ are very weak, and some, indicated as more or less stellar, might, strictly speaking, be regarded as simple stars, it being often quite impossible to distinguish between a small nebula and a star of small magnitude. The positions are approximately given for the mean equinox of 1860·0, in order to facilitate comparison with J. Herschel's "General Catalogue of Nebulæ and Clusters of Stars," and its supplement by Dreyer.—On the theory of magnetism, by M. P. Duhem. From a comparative study of magnetic and diamagnetic bodies the theorem is deduced that all magnetic bodies are attracted from great distances by permanent magnets, but that nothing can be affirmed regarding diamagnetic bodies. A theorem is also established which sets forth the difference between magnetic and diamagnetic bodies, and some remarks are appended regarding the magnetizing of crystals.—Measurement of the heights and movements of clouds in Spitzbergen and Upsala, by M. Nils Ekholm. These comparative studies are based on fifty meteorological observations taken during the Swedish expedition of 1882–83 to Spitzbergen, conducted by the author.—On a new method of formation of safranines, by MM. Ph. Barbier and Léo Vignon. Having in a previous communication explained a special method of forming substituted safranines, the authors here describe a new process for producing phenosafranine and its homologues.—On a new artificial serum intended to dilute the blood for the purpose of counting its globules, by M. Mayet. For the serum here described it is claimed that it is free from the disadvantages of others in general use. It consists of distilled water, 100 gr.; neutral phosphate of anhydrous and pure sodium, 2 gr.; with cane sugar to raise the density of the liquid to 1·085.—On antipyrine as a remedy against sea-sickness, by M. Eugène Dupuy. The author declares that for some time back he has successfully employed this substance as a prophylactic against sea-sickness. He recommends a dose of 3 gr. to be taken daily for three days before sailing, to be continued if necessary during the voyage. Without claiming to have discovered an absolute specific, he considers that the success hitherto attending the use of antipyrine justifies the hope that in this substance we possess a more or less efficacious remedy against one of the chief terrors of travelling by sea.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Dictionary of Place-Names: C. Blackie, 3rd edition (Murray).—Report of the Commissioner of Agriculture, 1885 (Washington).—The Cremation of the Dead: Dr. H. Erichsen (Haynes, Detroit).—Down the Great River: Capt. W. Glazier (Hubbard, Philadelphia).—The Creator, and what we may know of the Method of Creation: Dr. W. H. Dallinger (Woolmer).—Primo Saggio sui Ragni Birmani: Prof. T. Thorell (Genova).—Le Pétrole: W. de Fonvielle (Hachette).—Ants, Bees, Dragon-Flies, Earwigs, Crickets, and Flies: W. H. Bath (Sonnenschein).—Through Central Asia: Dr. H. Lansdell (Low).—The Volcanic Origin of Epidemics; Are Epidemics Contagious? Dr. J. Parkin (Low).—Bulletin of the U.S. Fish Commission, vol. vi., 1886 (Washington).—Mineralogy: F. Rutley (Murby).—A Vertebrate Fauna of Sutherland, Caithness, and West Cromarty: Harvie-Brown and Buckley (Douglas).—Bulletin of the U.S. National Museum, No. 32, Catalogue of Batrachians and Reptiles of Central America and Mexico: E. D. Cope (Washington).—Archiv für Pathologische Anatomie und Physiologie, und für klinische Medicin, Hund. Bandes, Zweites Heft (Reimer, Berlin).—Botanische Jahrbücher für Systematik, Pflanzengeschichte, und Pflanzengeographie, Neunter Band, ii. Heft (Engelmann, Leipzig).—Transactions of the County of Middlesex Natural History and Science Society, 1886-87 (Mitchell and Hughes).—Records of the Geological Survey of India, vol. xx. part 3.—Bulletin of the California Academy of Sciences, vol. ii. No. 7.—Annalen der Physik und Chemie, 1887, No. 11 (Leipzig).—Beiblätter zu den Physik und Chemie, 1887, No. 10 (Leipzig).—Transactions of the Asiatic Society of Japan, vol. xv. part 1.

CONTENTS.

THURSDAY, DECEMBER 1, 1887.

THE MATHEMATICAL THEORY OF PERFECTLY ELASTIC SOLIDS.

An Elementary Treatise on the Mathematical Theory of Perfectly Elastic Solids; with a Short Account of Viscous Fluids. By William John Ibbetson, M.A. (London: Macmillan and Co., 1887.)

IT is strange that students should have had to wait till the present time for a systematic English text-book on the mathematical theory of elastic bodies. The want has been decidedly felt at Cambridge since the introduction of the subject into the schedule for the Mathematical Tripos in 1873; and though parts of Thomson and Tait's treatise on natural philosophy, and the reprint of Green's papers, had already brought a large amount of useful matter into an accessible form for those who had not time or opportunity to read the original memoirs, still it was found that learners, naturally looking for some compendious account of the whole subject, generally fell back upon M. Lamé's treatise.

The book at present under notice will supply this want satisfactorily. The plan on which it has been written is excellent in idea, and has on the whole been followed out well, though perhaps there is here and there some want of proportion, as for instance in the elaborate and purely mathematical details of Chapter V.

It is, no doubt, a difficult matter to decide what results of mathematical analysis should be introduced without proof in a treatise on mathematical physics, and there is little question that, as a matter of convenience to the reader, it is wiser to err on the side of assuming too little knowledge rather than too much. On the other hand, wherever questions of pure mathematics are introduced and discussed at length, they should be such as have a direct bearing on important parts of the physical subject. Now the general forms of the dynamical equations for an elastic body in terms of curvilinear co-ordinates, which are established in Chapter V. after a considerable amount of preliminary analysis, are so complicated as to be practically valueless. Indeed, the one case referred to is dismissed in a single paragraph. The special forms for polar and semi-polar co-ordinates, often to be used with advantage, may be very much more simply established independently.

To return to the general plan of the book. It commences with a short preliminary chapter, headed "Properties of Elastic Solids," in which, after showing that the subject cannot be profitably considered from the point of view of molecular structure, the author defines the ideal solid which must for purposes of analysis replace the real body.

In Chapter II. the general properties of strain are treated very clearly and at considerable length. A little more consideration might perhaps have been given with advantage to finite homogeneous strain. No fewer than three quadrics are introduced for the purpose of putting results into a geometrical form, viz. the strain ellipsoid, the elongation quadric, and the position ellipsoid; while in the succeeding chapter, on "The Analysis of Stress," four surfaces—the first, second, third, and fourth stress quadrics—are used for a similar purpose. It can hardly be doubted that so great a number of surfaces will tend rather to confusion than to that clearness of conception of the properties of strain and stress for which they are presumably introduced.

The nature and mode of specification of stress is carefully expounded in Chapter III., and the dynamical equations to be satisfied throughout the body and over the boundary are obtained in terms of the stress-components. Attention should be called to a statement in § 153 of this chapter, as likely to mislead the student. It is to the effect that "the component stresses are to be considered as small quantities of the first order." Though in a certain sense this is true, it is not true that the ratio of the stress per unit area to, say, the weight per unit volume of the body is a small proper fraction, and this surely is the strict sense the words should bear.

The next chapter, on "The Potential Energy of Strain," is excellent. The method is similar to that used by Thomson and Tait; and the successive simplifications introduced into the expression for the potential energy of the strained body by considering successively greater degrees of symmetry of structure, leading up to perfect isotropy, are well shown. It is also pointed out that from the definition of an isotropic body, its potential energy is necessarily a function of the invariants of the strain, thus reducing the number of independent elastic constants in this case at once to two. Having thus arrived at a definite conception of the isotropic elastic solid, the author expressly limits all the investigations that follow to the case of such a body. From the expression obtained for the potential energy, forms are deduced for the stress components in terms of the strain components and the elastic constants, and thence finally the dynamical equations are obtained in terms of the displacements.

In Chapter V., as already stated, these equations are thrown into new forms, and the remainder of the book is devoted to their solution under various conditions as to the nature of the applied forces and tractions and the form of the body.

As an introduction to the consideration of particular questions the following five general theorems are proved, viz.:—

"(i.) that a state of strain cannot be maintained unchanged without the action of applied forces or surface tractions:

"(ii.) that the state of strain maintained by a given system of equilibrating applied forces and surface tractions is therefore perfectly determinate:

"(iii.) that the most general free motion of an elastic solid consists of a number of superposed harmonic oscillations of the particles about their natural positions:

"(iv.) that the most general motion of such a body under the action of an equilibrating system consists of a number of superposed harmonic oscillations of the particles about the equilibrium positions that would be maintained by the system:

"(v.) that a system of applied forces varying as a simple harmonic function of the time gives rise to forced harmonic oscillations of the particles of the same period about their natural positions."

The proofs given of these theorems, and especially of the third, are rather unnecessarily long; but, with a view to avoiding repetition later on, it is certainly convenient

to have them established as a foundation from which to start.

The problem of free vibrations is first treated, and as an example the propagation of plane waves of (i.) normal, (ii.) tangential, displacement is investigated. It is a pity that the author has not here taken the opportunity of illustrating some previous remarks on the discontinuity of the forms of the strain and stress components which necessarily accompanies a change in the nature of the medium, by considering the question of the reflection and refraction of plane waves.

The general form of the solution of the equations for forced vibrations is next investigated. Then follows the general question of equilibrium. As a simple example the case of a cylindrical tube under external and internal normal pressures is first treated. It is almost annoying to find the solution of this time-honoured question obtained by starting from the general equations, and whittling them down till the very simple conditions are fulfilled. The equilibrium of a solid sphere, with either surface tractions or displacements given, is treated exactly as in Thomson and Tait's work. The chapter of general solutions closes with an account of Airy's general method for plain stress, with a couple of examples. The printing was unfortunately so far advanced that this had to be left; though before the book appeared the author had himself shown, in a communication to the London Mathematical Society, that these examples of Airy's are faulty, and that the method applies only to a very limited class of cases.

Chapter VII. consists mainly of a capital exposition of the solution of St. Venant's problems of the torsion and flexure of prisms. These problems are probably, from a practical point of view, the most important for which an exact solution has been obtained. The author brings out well the bearings of the nature of the solutions on practical questions of construction. The elastic equilibrium and small motions of wires, whether straight or curved, are deduced directly from the results of St. Venant's problems. In connection with this part of the subject, certain interesting questions of stability, due to Mr. Greenhill, are discussed.

Some cases of the equilibrium and vibrations of plates and shells are considered in Chapter VIII. For the equilibrium of a plate of uniform thickness under a system of surface tractions parallel to its faces and acting on its edges, a solution is obtained by analysis very similar to that used in St. Venant's problem. The case of a thin plate under the action of applied faces satisfying certain conditions is quoted from Thomson and Tait again.

Two short chapters headed "Impact" and "Viscosity" complete the volume. The former consists of the solution of two problems, one of which, as the author implies, has nothing to do with impulsive change of motion. Indeed, as is well known, the exact treatment of the impact of elastic bodies involves difficulties, even in comparatively simple cases, which have not yet been overcome. In the last chapter the alteration in the form of the dynamical equations is determined, which results from supposing the shearing stress to vary partly as the shear and partly as its rate of change.

Having thus given some account of the plan of the book and the way in which it has been carried out generally, we may offer some remarks on matters of detail. It may be said at once that as regards accuracy there is a good deal to be desired. The table of errata might have been tripled, and would not then have contained all the misprints. In §§ 299, 306, the wholesale omission of signs of summation in the equations makes the analysis, as given, incorrect; and there is little doubt but that anyone to whom the matter treated was new would be completely baffled. The inaccuracies, moreover, are not confined to mere misprints. There are one or two positive mistakes in the mathematics. Thus at the bottom of p. 58 it is implied that some condition is necessary in order that a family of surfaces, $f(x, y, z) = \xi$ (an arbitrary parameter), may have a system of continuous curves cutting them at right angles; and in a note at the foot of p. 298 it is stated that, supposing this (entirely imaginary) condition satisfied, two other systems of surfaces can always be found cutting each other and the former surfaces everywhere at right angles. Now the three parameters of such a triple system of surfaces have to satisfy three independent partial differential equations, and hence no one of the three can be taken arbitrarily. Statements and reasoning are, in several passages, founded on this erroneous conception. Closely allied with this is the construction given in § 216 for tubes of stress. It is here practically assumed that a given continuous system of curves can always be cut at right angles by a family of continuous surfaces.

An appendix at the end of Chapter II., on "The Geometry of Strains," might have been omitted with advantage. It has no very obvious connection with the preceding chapter, but is devoted to an apparently new classification of vector quantities, in which a velocity and a force are the types of the one group, while an angular and a couple are those of the other! Again, in §§ 270, 271, the solution of a physical problem is made to appear to depend on the choice of an origin. The question treated is the free normal vibrations of a plate; and, after using d and $-d'$ to denote the abscissæ of the two faces, and making the result appear to depend on d/d', the question is *simplified* by taking the origin midway between the faces. Indeed, frequently throughout the book one is reminded of Clerk Maxwell's remark on "the state of a mind conscious of knowing the absolute position of a point."

These slips, such as they are, and an occasional obscurity of language, are but slight blemishes on a valuable book. A friendly but independent criticism of the proof-sheets while the book was passing through the press might have removed them all, and no doubt will in a new edition.

The figures throughout are excellent.

THE VOLCANIC AND CORAL ISLANDS OF THE SOLOMON GROUP.

The Solomon Islands: their Geology, General Features, and Suitability for Colonization. By H. B. Guppy, M.B., F.G.S., late Surgeon R.N. (London: Swan Sonnenschein, Lowrey, and Co., 1887.)

SURGEONS in Her Majesty's navy are favoured beyond most men in the possession of abundant leisure and freedom from many of the common cares of

life. But in spite of the frequent changes of scene which they enjoy, or endure, and their unique opportunities for pursuing scientific researches, and in spite of their early acquaintance with elementary treatises on several branches of science, it is only at rare intervals that naval surgeons appear as observers or investigators. The unusual occasionally happens, and in the work by Mr. Guppy on the Solomon Islands we have an admirable example of what may be accomplished by an energetic observer alive to his advantages.

In this volume it would not be difficult to point out many imperfect forms of expression, some avoidable confusion in arrangement, even a few conclusions that the facts hardly appear to warrant; but these sink into insignificance when compared with the mass of valuable material from which they might be culled.

The object of the book is to describe fully, but in a general way, the author's geological observations on the islands of the Solomon Group, little space being devoted to the other subjects mentioned in the title. It is a compendium of important facts, most of them new to the scientific public. Perhaps the Journal of the Geological Society is hardly suited for recording a series of laborious and detailed observations on the rocks of a remote archipelago, and the publications of the Royal Society of Edinburgh—where detailed papers by Mr. Guppy appear—may not be read by all geologists. It may not be inappropriate, in these circumstances, to mention a few of the facts observed by Mr. Guppy and recorded in this volume.

The book is divided equally between the description of volcanic and calcareous islands, and is illustrated by maps and sections.

The volcanic rocks collected on the islands were submitted for mineralogical analysis to Prof. Judd and Mr. T. Davies; the calcareous formations were studied by Mr. John Murray; and the remains of animal life, both foraminiferal and coral, are being examined by the leading specialists; hence the work is enriched by the labours of well-known men, and the "gold" of the author's data impressed with the "guinea-stamp" of recognized authority.

The volcanic islands of the group are divided into two classes. First, those of comparatively modern formation, composed mainly of little-altered augite-andesites, andesitic pitchstones, tuffs, and agglomerates: these islands still preserve the volcanic outline and sometimes give evidence of recent activity by terminating in craters with hot springs or fumaroles. The second class is composed only in part of these rocks, and in part of much more ancient crystalline masses consisting chiefly of altered dolerites, quartz-diorites and -porphyries, and serpentines. Some islands of the latter class exhibit an extraordinary diversity in petrological character. Fauno, the description of which is illustrated by a geological map, is an interesting instance of this. The northern end of the island is occupied by a precipitous mountain of andesitic tuff sloping steeply down from an altitude of 1900 feet to a narrow isthmus, 150 feet high, composed of hornblende-augite-andesite, and leading to a sickle-shaped peninsula of successive hills connected by low strips of rock. The composition of this crescentic tongue is successively altered dolorites, quartz-porphyries, quartz-andesites, hornblende-andesites, and altered dolerites again. These rocks, almost invariably massive and unassociated with tuffs or agglomerates, each in turn occupy the whole breadth of the peninsula. The mode of formation which Mr. Guppy demonstrates for this promontory is illustrated in various stages by several other islands. A series of small volcanoes arising in a crescentic form, and each pouring out a characteristic lava, were gradually elevated and so brought into connection. Rapid denudation, caused by the great rainfall of the region, wore off the volcanic contours and reduced the chain of peaks to a series of "necks" in close juxtaposition. The comparative rarity of fragmental volcanic rocks, and the mineralogical constitution of the massive crystalline lavas of the surface, indicating their solidification at great depths, prove extensive denudation to have taken place all over those islands.

The main interest of the book centres in the researches of Mr. Guppy on calcareous deposits. He is the only geologist who has visited this most instructive group of coral islands; and he describes what he saw there with a straightforward simplicity that compels confidence in the accuracy of his observations, and affords to those who may find his theory insufficient all possible data for disproving it.

Mr. Guppy gives the following classification of the limestones of a "coral island" in the Solomon Group as revealed to him by the walls of the river gorges he explored:—

Group I.—*Coral Limestones*, properly so called.

Group II.—*Coral Limestones* which have the composition of coral muds or sands now forming near coral reefs. There are three subdivisions of this group: (1) crystalline limestone, in which coral plays a secondary part, and remains of calcareous Algæ and mollusks predominate; (2) chalky limestones; (3) homogeneous fawn-coloured limestones, often crystalline.

Group III.—*Rocks of the composition of volcanic mud and pteropod ooze*, containing also numerous Foraminifera. These are subdivided into (1) partially consolidated volcanic muds; (2) partially consolidated pteropod ooze; (3) hard limestones.

Group IV.—*Foraminiferal Limestones*, or consolidated "Globigerina ooze." There are two classes: (1) composed chiefly of tests of both pelagic and bottom-living Foraminifera; (2) chiefly composed of the tests of pelagic Foraminifera.

Group V.—*Rock resembling a consolidated deep-sea clay* (Red Clay).

The two last-named groups were certainly deposited at depths not much less than 2000 fathoms in an ocean far from continental land, and their existence above sea-level is now for the first time proved.

From all the facts that could be ascertained regarding the coral formations of the group, certain inferences were drawn, which we give in the author's own words:—

"The first is self-evident, viz. *that these upraised reef masses, whether atoll, barrier reef, or fringing reef, were formed in a region of elevation*. . . . It is apparent that Mr. Darwin's theory of coral reefs, which ascribes atoll and barrier reefs to a movement of subsidence, cannot be applied to the islands of the Solomon Group. . . .

"The second inference is, *that such upraised reefs are of moderate thickness, their vertical measurement not exceeding the usual limit of the depth of the reef-coral zone.*

. . I never found one that exhibited a greater thickness of coral limestone than 150 feet, or at the very outside 200 feet. . . .

"The third inference is, *that these upraised reef masses in the majority of islands rest on a partially consolidated deposit which possesses the characters of the 'volcanic muds' that were found, during the 'Challenger' Expedition, to be at present forming around volcanic islands.*

"The fourth inference is, *that this deposit envelops anciently submerged volcanic peaks.*"

Mr. Guppy states that his observations have made him a strong adherent of the theory of formation of coral islands advanced by Mr. Murray.

These observations are indeed crucial between the theories of subsidence and of solution, and point towards the newer. The theory of subsidence demands that a coral reef rising from deep water must be of enormous thickness, and rest upon volcanic or fragmental rock; that of solution requires that the reef be of slight thickness and rest on volcanic rock, or consolidated terrigenous mud, or pelagic ooze. According to the former the reef grows on the whole vertically; according to the latter its main extension is horizontal. Two of Darwin's principal objections to the early conception of coral islands were that it was absurd to suppose that submarine mountains were numerous enough to provide foundations for all the known reefs, and that it was impossible to imagine sedimentation taking place at great distances from land. The recent work of telegraph ships along the West Coast of Africa and elsewhere has shown the extreme probability of submarine mountains existing in large numbers throughout the ocean; the cruise of the *Challenger* proved that the shells of pelagic organisms, wind-borne and meteoric dust and volcanic ashes spread by ocean-currents produce perceptible sedimentation in mid-ocean at a rate varying in some inverse proportion to the depth.

Murray's theory can be brought readily to the test of observation and experiment; Darwin's cannot. It has been shown in the laboratory that calcium carbonate is soluble in sea-water, and is dissolved in greater amount in water containing carbonic acid especially when under pressure; the decomposition of dead corals and the respiration of living ones supply carbonic acid to aid in the removal of their calcareous remains. If atolls are formed in areas of elevation, they may ultimately be seen and measured: if only in regions of subsidence, measurement is impossible, and the vertical extent of the coral limestone can only be guessed at.

It must be confessed that the theory of solution in reef-building has not yet been put before the world with any approach to the completeness, lucidity, and grace with which Darwin convinced and enchained the scientific mind. The theory of subsidence is so beautiful, simple, and satisfactory, that very strong evidence is required to shake it; but in the history of science men have more than once been forced to say of a simple and satisfactory doctrine—

> "'Twas beautiful,
> Yet but a dream, and so—Adieu to it!"

Neither Murray nor Guppy has proved the subsidence theory to be a dream. Still, the solution theory has been plainly set forth, and here we have facts which amount to an absolute proof of its truth for one important group of coral islands. The proof is none the less convincing because it is restricted in its application; for it is concrete and complete in itself, not abstract and cumulative like the evidence for the subsidence theory. Mr. Guppy has demonstrated that the old theory fails and the new succeeds in explaining the formation and structure of the Solomon Islands, and coming at the present time this supplies a powerful argument for the general applicability of the solution theory—an argument that it will not be easy to set aside.

The book is short and interesting; and, besides the important features we have alluded to, it contains much information about the islands visited, and the author's adventures there.

HUGH ROBERT MILL.

AGRICULTURE IN SOME OF ITS RELATIONS WITH CHEMISTRY.

Agriculture in some of its Relations with Chemistry. By F. H. Storer. Two Vols. (London: Sampson Low, Marston, Searle, and Rivington, 1887.)

THIS work, by the Professor of Agricultural Chemistry at the Harvard University, is based on a course of lectures delivered annually by the author. It is addressed to students of agriculture and persons fond of rural affairs, rather than to students of chemistry. Free use has been made of German publications in agricultural chemistry, and of the writings of Prof. S. W. Johnson, of Newhaven, Connecticut. Some of the matters treated of in his two well-known books, "How Crops Grow" and "How Crops Feed," have been omitted, or only lightly touched, in the present volumes, which are therefore, to a certain extent, a supplement to those books.

The present volumes treat of the chemistry of the atmosphere, of waters, of soils, and of manures, and of their several relations to plants; the chemistry of animal life and nutrition is not dealt with. A large amount of valuable information, partly of historical interest, has been brought together; and much of it is presented in the somewhat old-fashioned English of the best writers of New England.

One illustration given by the author, to show that liquids penetrate into plants through their roots, we do not think very happy. He notes an observation made by himself, that Indian corn made to sprout in a flower-pot and watered with milk had white leaves; and he suggests that the minute particles of solid matter in the milk must have entered the plant and caused the whiteness. He admits, however, that the whiteness may have been due to chemical action. In noticing the growth of plants in artificial light, he hardly gives sufficient credit to the observations of Siemens and of Dehérain on growth in the light of the electric arc, both uncovered and variously shaded. Mr. Storer has scarcely that respect for earthworms with which Darwin has imbued us, for on the only occasion he mentions them he styles them pernicious, on the ground that harm is done to plants in pots by their casts, which become slimy mud when watered, and thus clog the pores of the earth and the roots of the plants.

In vol. i. p. 295, a serious mistake occurs, though

doubtless by oversight: it is stated that nitrate of soda used as a top-dressing for mowing-fields that contain true grasses "favours the growth of clover rather than of grass." The reverse of this is the truth. There is a good chapter on irrigation, in which it is pointed out that, "in spite of all that has been done of late years in California and the adjacent regions, it is still probably true that no other subject relating to agriculture so much needs to be attended to by the American people as this matter of watering the land." The questions of the disposal of excreta and of sewage are dealt with in their chemical aspects. Perhaps hardly due credit is given to the latest improvements in some precipitation processes for clarifying sewage, but we are glad to see that the author fully realizes that the sewage subject is essentially a sanitary and not an agricultural question. He also exposes some economic fallacies as to the value of sewage by citing various instances in which valuable matters are found at our doors so diluted as not to be worth the cost of collecting or saving. One illustration is the presence of gold in the clay of Philadelphia—1 of gold in about $1\frac{1}{4}$ million of clay. If the gold from the bricks of the houses could be brought to the surface in the form of gold-leaf, on each brick would be a golden surface of 2 square inches. In the clay beneath the portion of the city already built over is 126 million dollars' worth of gold, yet no one dreams of extracting it. So, except under very favourable conditions for the sewage, valuable manures may be obtained more cheaply than from sewage.

The necessity for the selection of ripe, as well as pure, seeds for sowing, and especially on poor soils, is insisted on and illustrated by records of experiments. The great importance, whether for good or evil, of micro-organisms to the farmer, is often pointed out; and the writer discusses the question of the sources of nitrogen available for plants, and the very important question as to the fixation of free nitrogen from the air by humus or by clay soils. The conclusions of Berthelot, Armsby, Dehérain, and others are stated, and the author regards it as proven, in the light of existing knowledge, that some nitrogen from the air is really fixed as an incident to certain fermentations which occur in the soil. This much debated and debatable point, which is of the utmost economic importance, still requires further elucidation; and we may hope that some further light will be thrown on it by the researches of Sydney Vines on the nutrition of the common bean.

The general nature of the changes brought about in the character of farming by railways and steamships, and the conditions which lead to "high" or to "low" farming, are discussed. An observation of Washington in a letter to Arthur Young is worth recording, in this connexion: "An English farmer must have a very indifferent opinion of our American soil when he hears that an acre of it produces no more than eight to ten bushels of wheat; but he must not forget that in all countries where land is cheap and labour is dear the people prefer cultivating much to cultivating well."

Special chapters are given to barley and oats, and there are three chapters on pastures, grass, and hay, mainly from a New England point of view. In one of these chapters it is stated that the East Anglian word "rowen" for "aftermath," used by old writers, but now, we believe, confined to parts of Suffolk, is in common use in New England.

One minor defect, which might have been remedied by an editor of the English edition, is the use throughout the book of many different systems of weights and measures, *e.g.* the long (English) ton of 2240 pounds, the short (American) ton of 2000 pounds, pounds and bushels per acre as well as kilogrammes per hectare, and German pounds per morgen, per Saxon acre, and per Hessian acre, and even quintals per acre. A reduction of these to one system would have rendered the results more comprehensible, and comparisons easier. Also, a few of the chemical names are not those now in use in this country, and the use of the terms bi-phosphate of lime and di-calcic phosphate as synonymous is very misleading.

For the sake of the British farmer, who is not such a reading man as his American *confrère*, we could wish that some of the subjects had been rather more digested, and that more illustrations had been drawn from English sources, but thanks are due to Mr. Storer for a very suggestive work, that can be confidently recommended to those interested in agriculture for perusal and careful study during the long winter evenings. It cannot fail to awaken a more intelligent interest in the physics and chemistry of the farm. Moreover, notwithstanding the author's modesty, it will be found very useful to the student of agricultural chemistry.

WEATHER.

Weather: a Popular Exposition of the Nature of Weather Changes from Day to Day. By the Hon. Ralph Abercromby. (London: Kegan Paul, Trench, and Co., "International Scientific Series," 1887.)

THE author of this book has undertaken a task the difficulty of which has deterred all previous writers, for FitzRoy's "Weather Book" can hardly be termed a text-book of the subject, and, moreover, it was written at a date at which weather telegraphy was in its infancy. The books which have appeared during the last two decades have been either manuals mainly for the use of seamen, like the Barometer Manuals of the Meteorological Office; or explanations of the interpretation of weather charts, like Mr. Scott's "Weather Charts and Storm Warnings," of which the third edition was lately noticed in these pages. The idea of telling an isolated observer how to employ local weather signs and the manifold modifications of clouds in aiding his own judgment of local weather has not hitherto been adequately carried out.

Mr. Abercromby is peculiarly well qualified for the task of preparing a weather text-book, for not only is he gifted with an unusual faculty of observing weather phenomena, and especially clouds and their changes in this country, as is proved by the papers he has read on various occasions; but he has had more leisure to travel to "foreign parts" than falls to the lot of most meteorologists. The book relates to weather in general, as distinguished from storms, and not merely to the weather of the British Isles; for, though the latter subject occupies most of the work, the information given as to the weather over more extensive areas, such as those of the North Atlantic and the United States, is most instructive and valuable. The

work is divided into two sections, elementary and advanced, of which the former is about one-fourth of the bulk of the latter. The reader must not go away with the idea that the volume contains no original views, for, as Mr. Abercromby says in his preface, "the results of many of the author's original and unpublished researches are included in its pages, such, for instance, as the explanation of many popular prognostics; the elucidation of the general principles of reading the import of cloud-forms; the classification of those cases in which the motion of the barometer fails to foretell correctly the coming weather; and the character of that kind of rainfall which is not indicated in any way by isobaric maps."

Mr. Abercromby's pages convey small consolation to adventurous weather prophets, such as Mr. Wiggins or the framers of the *New York Herald* announcements. At p. 433 we read: "From eight to twelve hours seems to be the furthest time for which forecasts can be issued in advance, and even then many local details cannot be given." Again, at p. 426 he says: "On the whole, we see that the crude notion of forecasting European storms from the United States contains some elements of truth, but that still, from the nature of cyclone motion, the idea can never be used in practical forecasting." His statements as to the impossibility of practically predicting weather by observations of sun-spots are also made with great care.

The most interesting chapters, at least to the ordinary reader, are those which relate to weather prediction, for isolated observers. As regards the formation of clouds and their indications, Mr. Abercromby sets forth the results of much research, but in our opinion he speaks somewhat too decidedly on points which are still *sub judice*.

We welcome the book most cordially, and anticipate a considerable demand for it. We may say, however, that in several places we have noticed slips in the wording, and that the orthography of some of the foreign names is not quite "according to Cocker." In some cases the author's phraseology is not quite clear, and paragraphs have to be read repeatedly before their precise meaning is taken in.

OUR BOOK SHELF.

Class-book of Algebra Examples for Middle and High Schools. Part II., for High Schools. By John Cook, M.A., Principal, Central College, Bangalore. (Madras: printed at the Lawrence Asylum Press, Mount Road. 1887.)

THIS book contains, in addition to the examples which form the main part of the volume, an "Introductory Summary of Rules and Formulæ," extending to about one-third of the whole contents. Although Mr. Cook in his preface lays special stress on this summary, we are by no means sure that its introduction into the volume is an improvement. It is insufficient to allow the student to dispense with the use of a text-book; and a student, who desired to refresh his memory about some particular method or formula, would do better to read it up in his text-book, than to refer to a set of stereotyped rules. Such a summary has the positive disadvantage that it inclines the student to conceive of algebra as consisting entirely of a set of rules, proceeding he knows not whence and leading he knows not whither—a conception which it is one of the chief duties of a teacher of algebra steadily to combat. In parts of this introduction, moreover, there is a looseness of method which is apt to prove very misleading to the student. To refer to only one or two cases in point, we would mention in the first place a confusion between an integral or a rational number and an integral or a rational function. This confusion is shown in the case of division (p. 10) and in the case of root-extraction (pp. 46 and 51). Again, Mr. Cook defines (p. 43) the G.C.M. of two or more fractions, a conception which is perfectly useless in algebra, and only tends to confuse the mind of the learner as to the real meaning of the algebraical G.C.M.

As to the main part of the volume, we are able to compliment Mr. Cook on having brought together a number of examples which are likely to prove useful, especially to teachers. The examples show very considerable variety, those on identities being particularly noteworthy. At the end of each exercise stands a "model solution" which will no doubt prove useful to the student; but what does Mr. Cook mean by saying in one such solution (p. 143) that any three numbers that satisfy the relation $a^2+b^2=c^2$ may be expressed in the form $3n$, $4n$, $5n$? We trust that, should the book reach a second edition, as it doubtless will, Mr. Cook will either dispense with the introduction altogether, which we should be inclined to consider the better plan, or at least remove from it the faults in method to which we have made objection. The good qualities possessed by the main part of the book—the examples themselves—would then render the volume one of undoubted value alike to students and teachers of elementary algebra.

R. E. A.

The Student's Hand-book to the Microscope: a Practical Guide to its Selection and Management. By A Quekett Club Man. (London: Roper and Drowley, 1887.)

ALTHOUGH hand-books and practical guides to the use of the microscope are by no means scarce, this little volume will be welcome to many. It more completely, and in a much smaller compass, meets the precise wants of the beginner who intends to acquire a practical knowledge of the use of the microscope, than the majority of kindred treatises. But it aims only at elementary instruction in the use of the instrument and its accessories. The author does not burden the eager and ambitious amateur who has just become possessed of, or is just about to obtain, a microscope, with the complexities of collecting, preserving, dissecting, preparing, and mounting. There can be no doubt that to obtain a fair initial mastery of a good instrument, with powers up to a quarter-inch objective, and to become facile in the use of all the apparatus which these may involve, for illuminating, polarizing, &c., and, in short, in putting to its best and highest use such a microscope, is by far the better course. To become hastily acquainted with the microscope and its adjuncts, and then to be diverted by elaborate processes for preparing and mounting, is not the surest way to increase the number of skilled and competent masters of the modern microscope. The Quekett Club Man is evidently practical, and sees this. He confines himself to a concise and useful statement, aided by illustrations, of what the microscope is and how its various accessories may be employed.

The author does not claim to take the student into any of the intricacies of high-power work, nor, save in an incidental way, to call attention to the newest microscopy. This is consistent; but we regret that the new and only accurate terminology is not employed. "Numerical aperture," briefly explained, would have been wiser than "angle of aperture," with no comment of any moment as to its relatively unscientific nature. Nor are we quite convinced that, although the author did not hold it to be within his scope to discuss, or even indicate the existence of, "apochromatic lenses," he was as helpful to the uninitiated as he might have been, by not indicating the existence of "compensating eye-pieces"; for both in English and German microscopes, with *any* good objec-

tives, they give better results than the majority of Huyghenian eye-pieces.

We close the book, nevertheless, feeling that it will be an acquisition to many who are without information, and want it, as to how to use the microscope.

A Sketch of Geological History, being the Natural History of the Earth and of its Pre-Human Inhabitants. By Edward Hull, M.A., LL.D., F.R.S. (London: C. W. Deacon and Co., 1887.)

In a prefatory note the publishers of this little book inform the readers that it constitutes the first of a series of volumes devoted to a "Sketch of Universal History." We must congratulate the publishers on having discovered an author with sufficient knowledge, and at the same time with the necessary courage, for coping with such an undertaking. In 148 small pages we have a description of the "original condition of the globe" when it first assumed its present form, followed by sketches of the Archæan and succeeding periods of the earth's history; the whole concluding with a retrospect, which reads like the moral of a fable. The work, it is believed, will form an appropriate introduction to three similar volumes in which the modern history of the world is sketched. The book before us is a marvel of condensation; but in reading it we feel like the unfortunate individuals who are compelled to support life on lozenges composed of "Liebig's Extract."

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.

Politics and the Presidency of the Royal Society.

I THINK that you have done the scientific world a great service in pointing out, in language to which it seems to me no one can take exception, the inconveniences which may arise from the President of the Royal Society occupying a seat in Parliament.

No one will, I think, contest the fact that the Royal Society occupies a unique place in our social organization. It differs from all other Societies in constitution, temperament, and tradition. To persons unacquainted with its working, its method of procedure often seems deliberate and formal to a fault. To those who take part in its work it is obvious that its intellectual freedom is absolutely unrestrained, and that, subject to such mistakes as no human institution can claim exemption from, its impartiality and independence of judgment are absolutely unfettered. This arises from the fact that it is a picked body of men of the most diverse mental attitudes, who owe their association to nothing but their own exertions, and who are in the habit of expressing themselves with the utmost frankness on subjects of common interest discussed amongst themselves.

With the general body of Fellows the Council, from the rapidity with which it is changed, is in constant touch. It is no great assumption, then, to conclude that the Council when it speaks will have behind it the approval of the Fellows—that is, in point of fact, the sanction of the general scientific opinion of the Empire.

Now, the President of the Royal Society, when he speaks officially, is something more than the President of a learned Society: he is virtually the Speaker of the English scientific world. This being so, his position appears to me to be no small one. It is one which in emergencies may become of paramount importance. And it is this view of his position which disposes me to think that it is desirable that the occupant of such a post should be politically unfettered. I apprehend that this view is shared by Prof. Balfour Stewart when he says: "I grant freely that under ordinary circumstances it is undesirable that the President of the Royal Society should enter the House of Commons." And it is not difficult to see why it is undesirable.

Successive Governments, as is well known, are in the habit of consulting the Royal Society on scientific questions, the solution of which may possibly influence or determine a public policy. To such appeals the Royal Society has hitherto replied to the best of its ability without fear or favour. Will it always have the same freedom when its President is amenable to party discipline? It is only necessary to point to the last session of Parliament to see that there were many occasions when the position of the President on the Government benches would have been a not wholly pleasant one. Much bebadgered Ministers would perhaps have come up to him and have said, You must really make some concession, and the man would be made of iron who would not sometimes yield. Then, having been squeezed himself, he would return to his Council with:—"In the House of Commons the other night a very strong opinion was expressed to me," &c., and the process of squeezing would be transferred to the Council. It is no use saying that these things would not happen; because everyone knows that in actual political life they do. If the President descends from the dignified reserve which hedges him in at Burlington House, he will have to take his chance with the disabilities of the ordinary Parliamentary rank and file.

I cannot therefore resist the conclusion that a President of the Royal Society owes it to himself and to his position to hold aloof from all influences that would impair his freedom, and, as a consequence, that of the Society. His position is one of the few in the country which is unique not merely from its absolute independence of external public influence, but from the sanction which is given to the action of its occupant by internal support. The impossibility of allowing the Judges to sit in the House of Commons is, I suppose, apparent to everyone, and, in my view, every disability in that respect which attaches to them attaches with equal force to the President.

I will only trespass on your space with two further observations.

Prof. Balfour Stewart's last argument is, of course, purely political, and, being so, appears to me to be the one thing needed to demonstrate the unadvisability of any exception to the general principle to which he adheres. He says that the President "has chosen to be an Englishman first and a man of science afterwards." Yes. But—and I trust that no shade of impropriety may be thought to attach to the argument—would he have been as equally acquiescent had the President chosen the political *rôle* of Irishman as his first duty?

Lastly, Prof. Williamson remarks that our President cannot "be supposed to have entered the House as the political representative of the Royal Society." But unfortunately he cannot help himself. He cannot sink his official status. The House of Commons will take note of it just as it does of that of the Lord Mayor and of the Chairman of the Metropolitan Board of Works, who do not sit in Parliament by virtue of their official positions. Yet, being there, they are liable to interpellations with respect to the business of the bodies over which they preside. I do not see why the President of the Royal Society should expect immunity from the same discipline, and the result, it is easy to see, might be extremely embarrassing to the Royal Society, which has other, and in my opinion more constitutional, modes of communicating with the Government, and, if need be, with Parliament.

I say these things not because I like saying them, but because, feeling as I do, I do not think I ought to abstain from saying them. No one has a higher admiration for our President than I have, and no one would less willingly utter a syllable that would give him pain. I rejoice in one aspect of the case, that the University of Cambridge has crowned a great scientific career by a signal honour. But I cannot but feel that the authority and position of the Presidency of the Royal Society belong to a sphere of action infinitely above the conflict of parties, and that they will run a serious risk of impairment when the honoured name of its occupant appears for the first time in modern scientific history in the lists of a party division.

W. T. THISELTON DYER.

Royal Gardens, Kew, November 26, 1887.

As a Fellow of the Royal Society who has sat for many years continuously in the House of Commons, I have read with much interest your article on the above subject, which, from a Royal Society point of view (but not in any sense from a Parliamentary stand-point) is one of very great importance. No reasonable person would for a moment object, I presume, to Prof. Stokes entering Parliament as a politician, if he be one, provided he be very careful to doff at the door of the House of Parliament every vestige of Royal Society representation, and appear there as a private politician to be taken for just what he is worth in that capacity, and no more. Do not let me be misunderstood: as a man of science he will, even in the House of Commons, receive the personal consideration due to his distinguished personal attainments; and few public assemblies are more ready than that House to give the full value to personal qualities and achievements. But the President of the Royal Society will put that distinguished body, no less than himself, in a thoroughly false position if he presumes to utter there a single sentence in its name. Should I be present—and the same may be said, I trust, of other Fellows—I shall not hesitate to rise instantly and disclaim his pretensions, and declare that he has no more authority than one of the doorkeepers to speak in a political assembly in the name of the Society over which in a purely scientific capacity he presides.

Having a most careful regard to the purity of your columns in respect of everything merely political, I find it very difficult to say much of what I think and feel on this question; but when I consider the depths to which a certain ex-Professor has descended since he seated himself upon the steep and slippery slope of politics, I must very earnestly deprecate any similar proceeding on the part of the highest officer of the Royal Society, in that capacity. In the political arena, I fear, we are on both sides daily getting a lower and lower opinion of our opponents, and I must confess that it is rapidly becoming hard to reconcile with the scientific spirit the rancorous abuse and unreasoning misrepresentation with which we are now too familiar.

But I must not be drawn into either polemics or personality. I must content myself with saying, that, if Conservatives think meanly of Liberal politicians just now, their sentiment is thoroughly reciprocated, and probably more than reciprocated, by those who, like myself, believe we have at heart the true greatness, the lasting tranquillity and the intellectual and social progress of the country. For Heaven's sake let us keep the Royal Society, if not above, at least most distinctly apart from, all political contentions; and, in order that we may do this, let its President, who has now become a professed party politician, either vacate the chair, or make it absolutely clear that on the floor of Parliament he will not presume to speak with any kind or degree of authority in the name of the Society.

I have no idea, Sir, of your political views, but I appreciate your desire to keep the Royal Society politically neutral—aye, politically non-existent—and I hope your timely and courageous warning will not have been given in vain.

I have no care to conceal my name, but the end in view may be best promoted, perhaps, by my merely signing myself,

F.R.S. AND M.P.

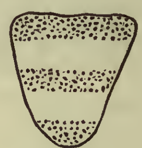
Library of House of Commons, November 21.

The Vitreous State of Water.

TO-DAY, between 2 and 3 p.m., with the barometer standing at 29 inches, the thermometer a little below 0° C., and the wind north-east, we had for the space of about twenty minutes an interesting fall of hail in this neighbourhood. The stones varied in size from that of a mustard-seed to that of a hemp-seed or thereabouts. Some rain accompanied them, and this became frozen in part on cold exposed surfaces. The stone sill of my study window, which faces nearly north-west, was soon covered in this way with a thin pellicle of ice, which served as a convenient resting-place for the hailstones at a low temperature. I was struck at once with their glassy appearance, and examined a number of them with a pocket lens as they lay on the cold surface of the stone, not having at hand any refrigerating arrangement adjustable to the stage of a microscope. Nor was the latter necessary. The lens showed most distinctly the clear transparency of the glass of which these hailstones consisted, and the vitreous fracture of some which had been broken by impact. Watching them as they lay, one saw minute nests of crystals form, in some cases in a peripheral zone, extending gradually inwards; but in the majority of instances the crystallization began in the centre of the ice, and gradually extended in a beautiful crystal growth more or less through the mass.

There would seem to be no room left for doubt that this crystal-building process (sometimes in bands, sometimes in confused nests of crystals) was a simple case of devitrification—as distinct a case, one may almost say, as the well-known devitrification on a larger scale which is clearly exhibited by some glassy slags. The fact of lying on a surface below 0° C., and undergoing devitrification instead of liquefaction, seems to lend direct support to the theory of *latent heat of the vitreous state*, which I have ventured elsewhere to propound (see NATURE, vol. xxxvi. p. 77).

I may add that last July, in a much heavier hailstorm in the Trent Valley, I noticed a very great number of hailstones, many of them as large as a moderate-sized hazel-nut, and peg-top shaped, with a zonal or banded structure thus:—



The layers or zones were alternately transparent and opaque (apparently crystalline), but in this case the temperature caused them to melt away without allowing a good opportunity for observation of any devitrification of the glassy portions. To-day Nature has performed the experiment suggested in my previous letter, and the result is found to accord with the theory.

A. IRVING.

Wellington College, Berks, November 18.

The Bagshot Beds.

IT may interest some of your readers to know that I recently obtained some casts of fossils from the Bagshot Sands of the Newbury district, from which, with one doubtful exception ("Survey Memoir," vol. iv. p. 330), they have not, I believe, hitherto been recorded. The fossils are of the nature of ferruginous casts, and were found in a sand-pit about one-third of a mile south-east of the London lodge of Highclere Park, mapped by the Survey as Lower Bagshot. They consist both of univalves and bivalves, and four or five genera are represented. They resemble, both in appearance and mode of occurrence, the fossils found in the Upper Bagshot of the Bagshot district; and the sands in which they occur have a strong resemblance to the

sands of that division. To whatever division, however, of the Bagshots these beds may be assigned eventually, the occurrence of fossils in them is, I think, worthy of record.

53 Warwick Square, November 25. R. S. HERRIES.

The Ffynnon Beuno and Cae Gwyn Caves.

SINCE writing my note, as published in NATURE of November 3, p. 7, I have paid another visit to the British Museum, and seen a second implement from the Denbighshire caves, presented by Dr. Hicks and Mr. Luxmore. It is a small and highly-finished scraper, exactly agreeing with the Neolithic scrapers of Icklingham and Mildenhall, and with small scrapers found in caves of confessedly very late date. This scraper is quite sufficient to condemn any pre-Glacial theory, and it enables me to emphasize my former remark that the cave contents, instead of belonging to the earliest Palæolithic class, belong to the *very latest.* I do not believe that a similar scraper has ever been found in any *really old, or even moderately old, Palæolithic river gravel.* Such scrapers were only made in the most recent of Palæolithic times.

Mr. G. H. Morton is not justified in his remark (Nov. 10, p. 32) that my former letter afforded "a remarkable instance of rushing into print and giving an opinion on a subject with which the writer was unacquainted," for I have studied the drifts of Wales for twenty years, and during that time I have never failed to make one or two visits a year to Wales. I have also examined nearly every cave in North and South Wales, and handled the shovel and pickaxe myself. From the experience I have obtained during this time, I say the drift in front of the Denbighshire caves is *not in its original position,* but *distinctly and obviously relaid;* and I even doubt whether before it was relaid it was a true Glacial gravel at all.

I will "read up the literature of the subject" if I get time: in the meantime there is no great harm done in expressing an opinion from a study of some of the real objects, even if that opinion is "not worth anything" and "of no consequence," as Mr. Morton concludes. WORTHINGTON G. SMITH.

Meteor.

ON Tuesday night, November 15, a wonderfully fine meteor was seen at Falmouth, and being out star-gazing at the time, I was fortunate enough to see it. I was looking towards that part of the Milky Way between Auriga, Perseus, and Cassiopeia, when suddenly a curved train of light flashed out; but, instead of just going away, it remained visible for quite eight seconds; meanwhile the lower extremity burst into a brilliant mauve "cone" of light, about a quarter the size of the full moon. So bright was it that it lit up the roadway, quite overpowering the lamps.

It was a grand sight, and I sincerely hope other eyes than mine saw it. B. TRUSCOTT.

4 Alma Crescent, Falmouth.

MODERN VIEWS OF ELECTRICITY.[1]

PART III.—MAGNETISM.

V.

WE next proceed to consider electricity in a state of *rotation.* What happens if we make a whirlpool of electricity? Coil up a wire conveying a current, and try. The result is it behaves like a magnet: compass-needles near it are affected, steel put near it gets magnetized, and iron nails or filings get attracted by it—sucked up into it if the current be strong enough. In short, it *is* a magnet. Not of course a permanent one, but a temporary one, lasting as long as the current flows. It is thus suggested that magnetism may perhaps be simply electricity in rotation. Let us work out this idea more fully.

First of all, one may notice that everything that can be done with a permanent magnet can be imitated by a coiled wire conveying a current. (It would not do altogether to make the converse statement.) Float a coil attached to a battery vertically on water, and you have a compass-needle: it sets itself with its axis north and south. Suspend two coils, and they will attract or repel or turn each other round just like two magnets.

As long as one only considers the action of a coil at some distance from itself, there is no need to trouble about the shape of the particular magnet which it most closely simulates; but as soon as one begins to consider the action of a coil on things close to it, it is necessary to specify the shape of the corresponding magnet.

If the coil be a long cylindrical helix like a close-spired corkscrew, as in Fig. 16, it behaves like a cylindrical magnet filling the same space. But if the coil be a short wide hank, like a curtain-ring, it behaves again like a cylindrical magnet, but one so short that it is more easily thought of as a disk. A disk or plate of steel magnetized with one face all north and the other face all south can be cut to imitate any thin hank of wire conveying a current. It will be round if the coil be round, square if it be square, and irregular in outline if the coil be irregular.

There is no need for the coil to have a great number of turns of wire except to increase its power: one is sufficient, and it may be of any shape or size. So when we come to remember that every current of electricity must necessarily flow in a closed circuit, one perceives that *every current of electricity is virtually a coil of more or*

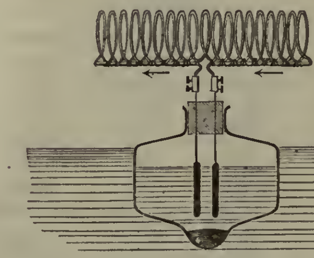


FIG. 16.—Floating battery and helix acting as a compass-needle.

less fantastic shape, and accordingly imitates some magnet or other which can be specified. Thus we learn that every current of electricity must exhibit magnetic phenomena: the two are inseparable—a very important truth.

There is one detail in which the magnetized disk and the coil are not equivalent, and the advantage lies on the side of the coil: it has a property beyond that possessed by any ordinary magnet. It has a penetrable interior, which the magnet has not. For space outside both, they simulate each other exactly; for space inside either, they behave differently. The coil can be made to do all that the magnet can do; but the magnet cannot in every respect imitate and replace the coil: else would perpetual motion be an every-day occurrence.

Now I want to illustrate and bring home forcibly the fact that there is something rotatory about magnetism—something in its nature which makes rotation an easy and natural effect to obtain if one goes about it properly. One will not observe this by taking two magnets: one will see it better by taking a current and a magnet, and studying their mutual action.

A magnet involves, as you know, two poles—a north and a south pole—of precisely opposite properties: it may be considered as composed of these two poles for many purposes; and the action of a current on a magnet may be discussed as compounded of its action on each pole separately. Now how does a current act on a magnetic pole? Two currents attract or repel each other; two poles attract or repel each other; but a current and a pole exert a mutual force which is neither attraction nor repulsion: it is a rotatory force. They tend neither to approach nor to recede; they tend to revolve

[1] This Part is an expansion of a lecture delivered at the London Institution on January 5, 1885. Continued from p. 13.

round each other. A singular action this, and at first sight unique. All ordinary actions and reactions between two bodies take place in the line joining them: the forces acting between a current and a pole act exactly at right angles to the line joining them.

Helmholtz long ago (in 1847) showed that the conservation of energy could only be true if forces between bodies varied in some way with distance and acted in the line joining them. Now here is a case where the forces are not in the line joining the bodies, and accordingly the conservation of energy is defied: the two things will revolve round each other for ever. This affords and has afforded a fine field for the perpetual motionist; and if only the current would maintain itself without sustaining power, a perpetual motion would in fact be attained. But this after all is scarcely remarkable, for the same may be said of a sewing-machine or any other piece of mechanism: if only it would continue to go without sustaining power it would be a perpetual motion. Attend to pole and current only, and the energy is *not* conserved, it is perpetually being wasted; but include the battery as an essential part of the complete system, and the mystery disappears: everything is perfectly regular.

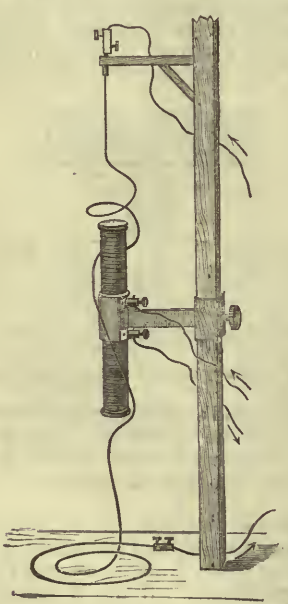


Fig. 17.—A long flexible conductor twisting itself into a spiral round a powerful bar-magnet.

The easiest way perhaps of showing the rotation of a conductor conveying a current round a magnetic pole is to take an 8-feet-long piece of gold thread, such as military officers stitch upon their garments, and hanging it vertically supply it with as strong a current as it will stand. Then bring near it a vertical bar-magnet, and instantly you will see the thread coil itself into a spiral, half of it twisting round the north end of the bar, and half twisting the other way round the south end (Fig. 17).

If the magnet were flexible and the conductor rigid, the magnet would in like manner coil itself in a spiral round the current: the force is strictly mutual. A rigid magnet put near a stiff conductor shows only the last remnants of this action: it sets itself at right angles to the wire, and approaches its middle to touch it, but that is all it can do.

The experiment with the flexible gold thread is simple, satisfactory, and striking, but the rotatory properties connected with a magnet may be illustrated in numbers of other ways. Thus, pivot a disk at its centre, and arrange some light contact to touch its edge, either at one point or all round, it matters not; then supply a current to disk from centre to circumference, and bringing a bar-magnet near it along its axis, or, better, two bar-magnets, with

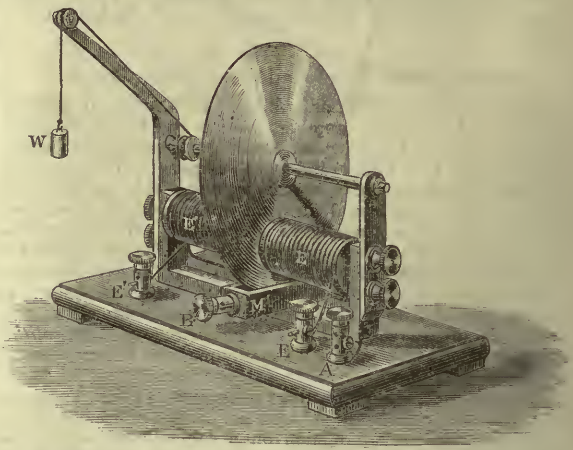


Fig. 18.—Pivoted disk with radial current, revolving in a magnetic field and winding up a weight. The current is supplied to the axle by screw A, and leaves the rim by mercury trough M. The same apparatus obviously serves to demonstrate currents induced by motion; both directly and by the damping effect.

opposite poles one on each side, near the contact place of the rim, the disk at once begins to rotate (Figs. 18 and 19).

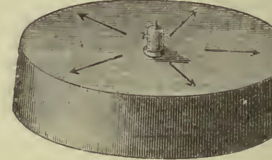


Fig. 19.—Another pivoted disk with flange to dip into liquid so as to make contact all round its rim. It rotates when a magnet is brought above or below; or even in the field of the earth.

Instead of a disk one may use a single radius of it, viz. a pivoted arm (Fig. 20) dipping into a circular trough of mercury; or we may use a light sphere rolling on two

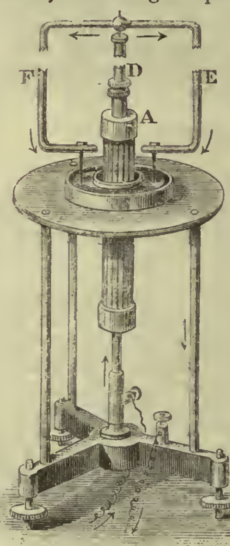


Fig. 20.—A couple of radii of the above disk provided with points to dip into mercury, and rotating constantly under the influence of the steel magnet A.

concentric circular lines of railway (Gore's arrangement, Fig. 21). In every case rotation begins as soon as a magnet is brought near.

Nor is the revolving action confined to metallic conductors and to true conduction. Liquids and gases, although they convey electricity by something of the

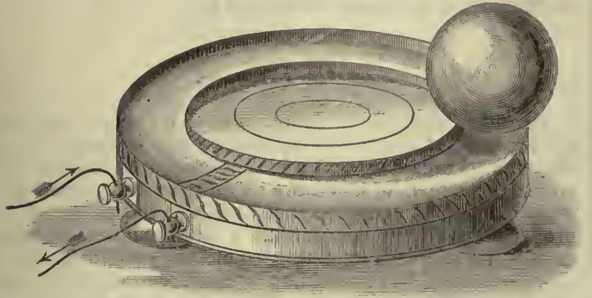


Fig. 21.—Gore's circular railway. The light spherical metal ball revolves round the two concentric metal hoops or rails whenever it is made to convey a current between them in a vertical magnetic field.

nature of convection, are susceptible to rotation in a precisely similar manner.

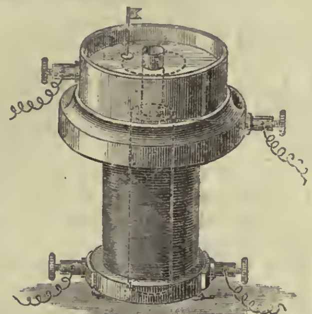


Fig. 22.—Rotation of a liquid disk conveying a radial current in a vertical magnetic field.

To show the rotation of liquid conductors under the influence of a magnet, take a circular shallow trough of

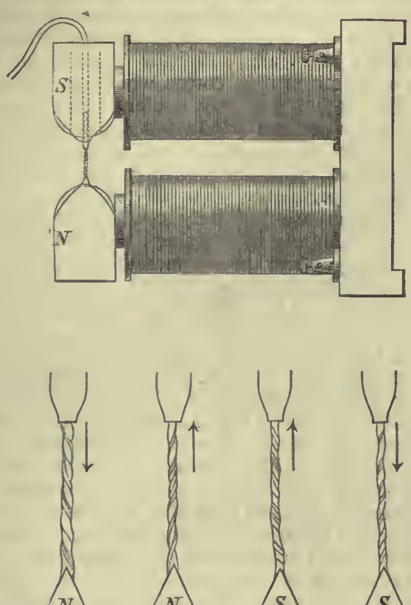


Fig. 23.—A falling stream of liquid conveying a current between two magnetic poles, and being thereby twisted into a spiral. (Copied from a paper in Phil. Mag. by Dr. Silvanus Thompson).

liquid, supply it with electrodes at centre and circumference, and put the pole of a magnet below it. The liquid at once begins to rotate, and by using a magnet and current of fair strength can easily be made to whirl so fast as to fly over the edge of the trough (Fig. 22). The experiment is plainly the same as Fig. 19, except that a liquid disk is used in place of a solid one. Or, again, it may be considered the same as Fig. 20. Reverse the magnet, and the rotation is rapidly reversed.

Another method is to send a current along a jet of mercury near a magnet and note the behaviour of the jet. It twists itself into a flat spiral as shown in Fig. 23.

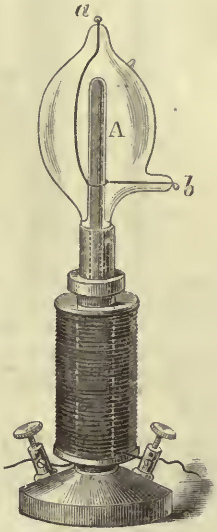


Fig. 24.—Induction coil discharge from *a* to *b* through rarefied gas, rotating round a glass-protected magnetized iron rod.

The rotation of a gas discharge is most commonly illustrated by an arrangement like Fig. 24, where the terminals of the induction coil are connected to the rarefied gas respectively above one pole and round the middle of a magnetized bar. If the discharge can be got to concentrate itself principally down one side, the line of light so formed is seen to revolve.

Action between a Magnet and an Electric Charge in Relative Motion.

From all this it is not to be doubted that a charged pith ball moving in the neighbourhood of a magnet is subject to the same action. There is no known action between a magnet and a *stationary* charged body, but directly either begins to move there is an action between them tending to cause one to rotate round the other. It is true that for ordinary speeds of motion this force is extremely small; but still it is not to be doubted that if a shower of charged pith balls or Lycopodium granules are dropped on to a magnet pole, they will fall, not perfectly straight, but slightly corkscrew fashion. And again, if a set of charged particles were projected horizontally and radially from the top of a magnet, their paths would revolve like the beams of a lighthouse. And if by any means their paths were kept straight, or deflected the other way, they would exert on the magnet an infinitesimal "couple," tending to make it spin on its own axis.

Conversely, if a magnet were spun on its axis rapidly by mechanical means, there is very little doubt but that it would act on charged bodies in its neighbourhood, tending to make them move radially either to or from it. This, however, is an experiment that ought to be tried; and the easiest way of trying it would be to suspend a sort of electrometer needle, electrified positive at one end and negative at the other, near the spinning magnet, and to look for a trace of deflection—to be reversed when the spin is reversed. A magnet of varying strength might be easier to try than a spinning one.

Rotation of a Magnet by a Current.

The easiest way to show the actual rotation of a magnet is to send a current half way along it and back

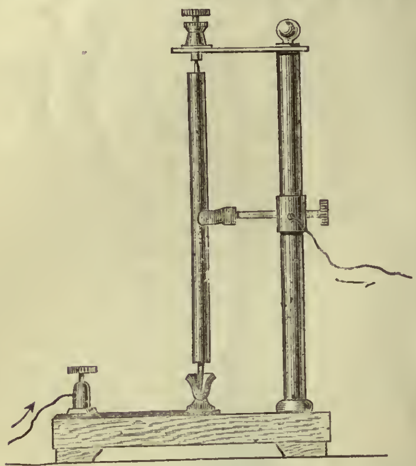


FIG. 25.—Round bright steel bar-magnet pivoted at its ends, spinning rapidly on its axis under the influence of a current supplied to either the bottom or top pivot, or both, and removed near the middle by a scrap of tinfoil lightly touching it.

outside. Thus, take a small, round, polished steel bar-magnet with pointed ends, pivot it vertically, and touch it steadily with two flakes or light pads of tin-foil, one

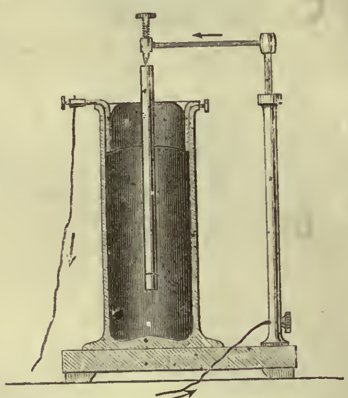


FIG. 26.—Another mode of exhibiting the same thing as Fig. 25. The magnet is loaded so as to float upright in mercury.

near either end and one near the middle; supply a current by these contact pieces, and the magnet spins with great rapidity. Reverse the current, and it rotates

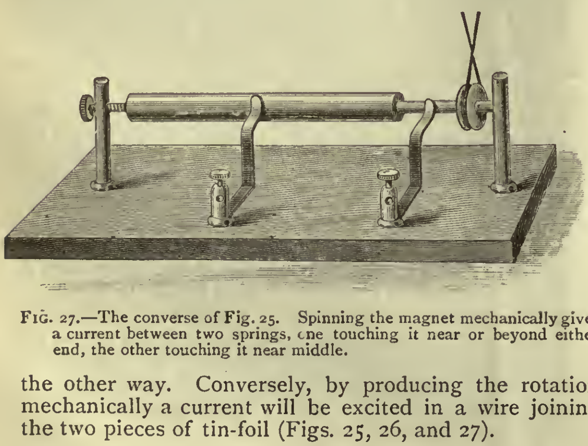


FIG. 27.—The converse of Fig. 25. Spinning the magnet mechanically give a current between two springs, one touching it near or beyond eithe end, the other touching it near middle.

the other way. Conversely, by producing the rotation mechanically a current will be excited in a wire joining the two pieces of tin-foil (Figs. 25, 26, and 27).

Many more variations of the experiment could be shown, but these are typical ones, and will suffice. They all call attention to the fact that a magnet, considered electrically, is a rotatory phenomenon.

Ampère's Theory.

The idea that magnetism was nothing more nor less than a whirl of electricity is no new one—it is as old as Ampère. Perceiving that a magnet could be imitated by an electric whirl, he made the hypothesis that an electric whirl existed in every magnet and was the cause of its properties. Not of course that a steel magnet contains an electric current circulating round and round it, as an electro-magnet has: nothing is more certain than the fact that a magnet is not magnetized as a whole, but that each particle of it is magnetized, and that the actual magnet is merely an assemblage of polarized particles. The old and familiar experiment of breaking a magnet into pieces proves this. Each particle or molecule of the bar must have its circulating electric current, and then the properties of the whole are explained.

There is only one little difficulty which suggests itself in Ampère's theory—How are these molecular currents maintained? Long ago a similar difficulty was felt in astronomy—What maintains the motions of the planets? Spirits, vortices, and other contrivances were invented to keep them going.

But in the light of Galileo's mechanics the difficulty vanishes. Things continue in motion of themselves until they are stopped. Postulate no resistance, and motion is essentially perpetual.

What stops an ordinary current? Resistance. Start a current in a curtain ring, by any means, and leave it alone. It will run its energy down into heat in the space of half a second or so. But if the metal conducted infinitely well there would be no such dissipation of energy, and the current would be permanent.

In a metal rod, electricity has to pass from atom to atom, and it meets with resistance in so doing; but who is to say that the atoms themselves do not conduct perfectly? They are known to have various infinite properties already: they are infinitely elastic, for instance. Pack up a box of gas in cotton-wool for a century, and see whether it has got any cooler. The experiment, if practicable, should be tried; but our present experience warrants us in assuming no loss of motion among the colliding atoms until the contrary has been definitely proved by experiment. To all intents and purposes *certainly* atoms are infinitely elastic: why should they not also be infinitely conducting? Why should dissipation of energy occur in respect of an electric current circulating wholly inside an atom? There is no known reason why it should. There are many analogies against it.

How did these currents originate? We may as well ask, How did any of their properties originate? How did their motion originate? These questions are unanswerable. Suffice it for us, there they are. The atoms of a particular substance—iron for instance, or zinc—have an electric whirl of certain strength circulating in them as one of their specific physical properties.

This much is certain, that the Ampèrian currents are not producible by magnetic experiments. When a piece of steel or iron is magnetized, the act of magnetization is not an excitation of Ampèrian current in each molecule—is not in any sense a magnetization of each molecule. The molecules were all fully magnetized to begin with: the act of magnetization consists merely in facing them round so as to look mainly one way—in polarizing them, in fact. This was proved by Beetz long ago; I will not stop to explain it further, but will refer students to Maxwell.

Ampère's Theory extended by Weber to explain Diamagnetism also.

Let us see how far we have got. We have made the following assertions:—

(1) That a magnet consists of an assemblage of polarized molecules.

(2) That these molecules are each of them permanent magnets, whether the substance be in its ordinary or in its magnetized condition, and that the act of magnetization consists in turning them round so as to face more or less one way.

(3) That when all the molecules are faced in the same direction the substance is magnetically completely saturated.

(4) That if each molecule of a definite substance contains an electric current of definite strength circulating in a channel of infinite conductivity the magnetic behaviour of the substance is completely explained.

But now, supposing all this granted, how comes it that the molecular currents are not capable of being generated by magnetic induction? And if we cannot excite them, are we able to vary their strength?

The answer to these questions is included in the following propositions, which I will now for convenience state, and then proceed to explain and justify.

(5) If a substance possessing these molecular currents be immersed in a magnetic field, all those molecules which are able to turn and look along the lines of force in the right direction will have their currents weakened; but on withdrawal from the field they will regain their normal strength.

(6) If the currents flowing in the conducting channels be feeble or *nil*, the act of immersion of the substance in a magnetic field will reverse them or excite *opposite* currents, which will last so long as the body remains in the field, but will be destroyed by its removal.

(7) The molecular currents so magnetically induced are sufficient to explain the phenomena of *diamagnetism*.

Let us first just recall to mind the well-known elementary facts of current induction. A conducting circuit, such as a ring or a coil of wire, suddenly brought near a current-conveying coil or a magnet, has a momentary current induced in it in the opposite direction to the inducing current—in other words, such as to cause momentary repulsion between the two. So long as it remains steady, nothing further happens; but on withdrawing it another momentary current is induced in it in the contrary direction to that first excited. The shortest way of expressing the facts quite generally is to say that while from any cause the magnetic field through a conductor is increasing in strength a current is excited in it tending to drive it out of the field: the disturbance is only temporary, but whenever the magnetic field decreases again to its old value a reverse flow of precisely the same quantity of electricity occurs. Fig. 28 shows a mode of illustrating the facts. A copper disk is supported at the end of a torsion arm and brought close to the face of an unexcited bar electro-magnet. On exciting the magnet the disk is driven violently away: to be sucked back again, however, whenever the magnetism ceases.

Now, why are all these effects so momentary? What makes the induced current cease so soon after excitation? Nothing but dissipation of energy: only the friction of imperfect conductivity. There is nothing to maintain the current: it meets with resistance in its flow through the metal, and so it soon stops.

But in a perfect conductor like a molecule no such dissipation would occur. Electricity in such a body will obey the first law of motion, and continue to flow till stopped. Destroying the magnetic field will stop an induced molecular current, but nothing else will stop it. Hence it follows that the repulsion experienced is no transitory effect like that in Fig. 28, but is as permanent as the magnetic field which excites and exhibits it.

Thus, then, a body whose molecules are perfectly conducting, but without specific current circulating in them, will behave diamagnetically, *i.e.* will move away from strong parts of the field towards weak ones, and thereby set its length equatorially, just as bismuth is known to do.

Whether this be the true explanation of diamagnetism or not, it is at least a possible one. It is known as Weber's theory.

It does not necessarily follow that the specific molecular currents of a diamagnetic substance are really *nil*; all that is needful is that they shall be weaker than those induced by an ordinary magnetic field. By using an extremely weak field, however, the specific currents need not be quite neutralized, and in such a field the body ought to behave as a very feebly magnetic substance. Such an effect has been looked for (see NATURE, vol. xxxv. p. 484).

One loop-hole there is, however, viz. that every molecule may be so jammed as to be unable to turn round, and such a substance could hardly exhibit any noticeable magnetic properties. The molecules would have got themselves into a state of minimum potential energy, and if jammed therein nothing could be got out of them. The induced currents of diamagnetism would be superposed

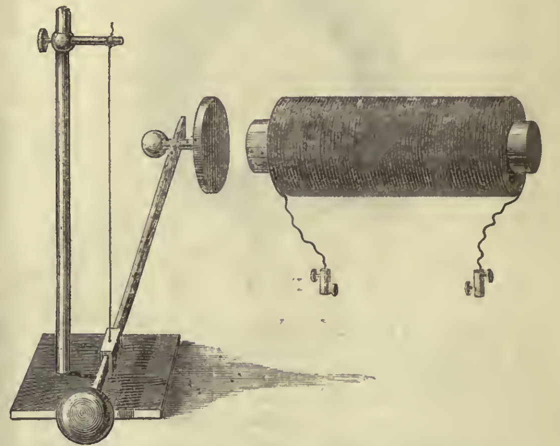


FIG. 28.—Stout disk of copper supported on a horizontal arm near one pole of a bar electro-magnet. The disk is repelled every time the magnet is excited, and is attracted while the magnetism is destroyed.

upon them just as if no initial molecular currents existed. By varying the temperature of such a substance, however, one might expect to alter their arrangement, and so develop magnetic properties in it, just as electrical properties are developed in crystals like tourmaline by heat or by cold.

We are now able clearly to appreciate this much—that the molecular currents needful to explain magnetism are not conceivably excited by the act of magnetization, for they are in the wrong direction. *Induced* molecular currents will be such as to cause repulsion: those which cause attraction must have existed there before, and be merely rotated into fresh positions by the magnetizing force.

Function of the Iron in a Magnet. Two Modes of expressing it.

We can now explain the function of iron, or other magnetic substance, in strengthening a magnetic field. Take a circular coil of wire, Fig. 29, and send a current round it: there is a certain field—a certain number of lines of force—between its faces. Fill the coil with iron, so as to make it a common electro-magnet, and the strength of the field is greatly increased. Why? The common mode of statement likens the magnetic circuit to a voltaic circuit; there is a certain magneto-motive

force, and a certain resistance: the quotient gives the resulting magnetic induction, or total number of lines of force. Iron is more permeable than air—say, 300 times more permeable—and accordingly the resistance of the iron part of the circuit is almost negligible in comparison with that of the air-gap between the poles. Thus a good approximation to the total intensity of field is obtained by dividing the magneto-motive force by the width of the air-gap; or more completely and generally by treating the varying material and section of a magnetic circuit just as the varying material and section of a voltaic circuit is treated, and so obtaining its total resistance. Iron is thus to be regarded as a magnetic conductor some 300 times better than air.

This mode of regarding the case is undoubtedly simple and convenient, but it is not the fundamental mode. If we look at it less with a view to practical simplicity than with the aim of seeing what is really going on, we shall express it thus:—

Before the iron was inserted in the coil there were a certain number of circular lines of force inside it due to the current alone. A piece of common iron, although

FIG. 29.

full of polarized molecules, has no external or serviceable lines of force: they are all shut up, as it were, into little closed circuits inside the iron. But directly the iron finds itself in a magnetic field some of these open out, a chain of polarized molecules is formed, and the lines due to its molecular currents add themselves to those belonging to the current of the magnetizing helix.

Thus our ring electro-magnet has now not only its own old lines of force, but a great many of those belonging to the iron which have sympathetically laid themselves alongside the first.

The end result of either mode of regarding the matter is of course the same—the lines of force between the poles are increased in number by the presence of iron; but whereas, in the first-mentioned mode of treatment, the fact of permeability had to be accepted unexplained, in the second nothing is unexplained except the fundamental facts of the subject, such as the reason why currents tend to set themselves with their axes parallel, and other matters of that sort.

Electrical Momentum once more.

There is just one point which I must stop here to call attention to. The theories of magnetism and diamagnetism, which I have given according to Ampère, Weber, and Maxwell, require as their foundation that in a perfect conductor electricity shall obey the first law of motion—shall continue to flow until stopped by force. But the property of matter which enables it to do this is called *inertia*; the law is called the law of inertia; and anything which behaves in this way must be granted to possess inertia.

It would not do to deduce so important a fact from a yet unverified theory; but at least one must notice that it is essentially involved in Ampère's theory of magnetism. It is the only theory of magnetism yet formulated, and it breaks down unless electricity possesses inertia.

Nevertheless it is a fact that an electro-magnet does not behave in the least like a fly-wheel or spinning-top: there is no momentum mechanically discoverable. Supposing this should turn out to be strictly and finally true, we must admit that a molecular electric current consists of two equal opposite streams of the two kinds of electricity: one must begin to regard negative electricity not as merely the negation or defect of positive, but as a separate entity. Its relation to positive may turn out to be something more like that of sodium to chlorine than that of cold to heat.

I said that no effect due to electric inertia was *mechanically* discoverable, but that is perhaps too sweeping a statement. Think of a couple of india-rubber pipes tied together so as to form a double tube, and through each propel a current of water, one in an opposite direction to the other. Although the double current has no gyrostatic properties, yet the water exhibits momentum, even when the current is quite steady, by its effect on kinks and bends and curves in the tube: these all tend to straighten or smooth themselves out, and the tube if quite free would become a perfect circle.

Precisely the same effect can be observed with a flexible conducting wire or gold thread. Throw a loop of very light flexible thinly-covered stranded wire at random on a glass slab, and pass a strong current through it: it will tend to round off its sharp corners, open out its tangled loops, and do its best to become a perfect circle; and this quite independently of the earth's field, in accordance with the principle numbered 3 on page 8. It will be at once objected that this effect, in the case of the wire, is due to something going on in the medium surrounded by it, and not simply to the inertia of anything in the conducting channel itself, as in the water case. The objection is, of course, perfectly valid, but nevertheless the effect is one worth bearing in mind; and its ultimate explanation may lead us to postulate inertia quite as essentially though not so superficially as the crude hydraulic analogy suggests.

So long as one considered the flow of electricity in ordinary conductors, we could partially avoid the question of inertia by considering it urged forward at every point with a force sufficient to overcome the resistance there and no more; but though this explained the shape of the stream-lines (Fig. 15) yet it did not suffice to render clear the phenomena of self-induction—the lag of the interior electricity in a wire behind the outside until definitely pushed; and still more its temporary persistence in motion after the pushing force has ceased.

But, now that we are dealing with perfect conductors with no pushing force at all, the persistence of molecular currents without inertia, or an equivalent property so like it as to be rightly called by the same name at present, becomes inexplicable. True, the molecular currents are as yet an hypothesis; and that is the only loop-hole out of a definite conclusion.

OLIVER J. LODGE.

(To be continued.)

DISCOVERY OF DIAMONDS IN A METEORIC STONE.

IN a Russian paper of October 22 last appears a preliminary report of the examination by Latschinof and Jerofeief, Professors of Mineralogy and Chemistry respectively, of a meteoric stone weighing 4 lbs., which fell in the district of Krasnoslobodsk, Government of Pensa, Russia, on September 4, 1886.

In the insoluble residue small corpuscles showing traces of polarization were observed; they are harder than corundum, and have the density and other characters

of the diamond. The corpuscles are said to amount to 1 per cent. of the meteoric stone.

Carbon, in its amorphous graphitic form, has been long known as a constituent of meteoric irons and stones; lately, small but well-defined crystals of graphitic carbon having forms often presented by the diamond, were described in our columns as having been found in a meteoric iron from Western Australia. If this supplementary discovery be confirmed, we may at last be placed on the track of the artificial production of the precious stone.

NOTES.

On Tuesday afternoon an important meeting was held in the Town Hall, Manchester, in support of the National Association for the Promotion of Technical Education. A powerful and most interesting address was delivered by Prof. Huxley. Afterwards, in accordance with a resolution moved by Sir H. E. Roscoe, and seconded by Sir W. H. Houldsworth, the meeting appointed an influential Committee to consider the proposals communicated by the National Association for the Promotion of Technical Education, and to take action thereon. Now that the vital importance of the subject is beginning to be understood in the district, there can be little doubt that Manchester will soon be supplied with a thoroughly sound and adequate system of technical instruction. The residuary legatees under the will of the late Sir Joseph Whitworth have just presented to the town a plot of land, called Potter's Park, which they have bought for £47,000. On a part of this land it is proposed that the following institutions shall be erected: (1) an appropriate Institute of Art, with galleries for paintings, for sculpture and moulded form, and for architectural illustration; (2) a comprehensive Museum of Commercial Materials and Products; (3) a Technical School on a complete scientific and practical scale. Much money will have to be provided before this scheme can be fully carried out, but in so great a centre of manufacturing and commercial energy the necessary funds should be raised without serious difficulty. The managers of the late Manchester Exhibition, like the Whitworth legatees, are vigorously supporting the movement, and their example will certainly be extensively followed. The progress made at Manchester is most satisfactory, and there are also many signs of an advance in the right direction at Liverpool and Newcastle.

The latest news from Mr. John Whitehead is that he has returned from Palawan with a rich collection, especially in birds, of which he believes that he has obtained over eighty species not previously recorded from the island, and a large number of migrants. Palawan is an interesting place for a naturalist, as it lies so near the Philippine Archipelago, and yet contains a very strong Bornean element. Mr. Whitehead proposes shortly to make another ascent of Kina Balu Mountain, where last spring he obtained nineteen new species of birds, described by Mr. Bowdler Sharpe in the *Ibis* for October.

Letters have recently been received from Mr. H. O. Forbes, who is now at Granville in British New Guinea. He has not recovered from the overwhelming disaster at Batavia, when the whole of the *matériel* for his explorations was lost by the upsetting of a boat in the surf, but his spirits and those of his brave wife appear indomitable, and he hopes yet to proceed into the interior of New Guinea. He remarks that the Horse-shoe Range of the Astrolabe Mountains is unkown to residents in the island. This is the place whence Mr. Hunstein sent the wonderful birds of Paradise described by Dr. Finsch and Dr. Meyer, but Mr. Forbes says that he cannot find out the position of the range to which Hunstein attached the name. Mr. Forbes states that he has penetrated further inland than any other explorer, but that "no European foot has yet trod any portion of the real Owen Stanley Range." Surely some assistance can be rendered to this good naturalist, who has expended £2000 of his own money in the cause of science, to enable him to prosecute further researches. It only requires a glance at Mr. Forbes's work on the Malay Archipelago to show that he is a worthy follower in the footsteps of Wallace.

At a dinner given by the Library Committee of the Corporation of London on Monday, Prof. Stokes responded for "Science." He said men of science knew how fascinating the pursuit of science was, even apart from its applications. It differed from art, however, in this respect, that when the scientific man had arrived at his result it was in very many cases of such a nature that only comparatively few men, who themselves had been trained more or less in science, could enter into and derive pleasure from it.

The discussion on Sir Frederick Abel's paper on "Accidents in Mines," at the Institute of Civil Engineers, came to a conclusion on Tuesday evening, the debate having extended over four meetings, a number of well-known colliery owners and managers coming up from the country to take part in it. Safety-lamps, gas, coal-dust, winding-gear, and other topics were exhaustively discussed, and it was evident that amongst practical men a considerable difference of opinion exists on many of the questions raised. Sir Frederick having directed attention to the communication in Nature, vol. xxxvi. pp. 437 and 438, Mr. Harries gave further particulars bearing upon the meteorology of colliery explosions. He showed how the popular belief that disasters are always accompanied by a low barometer is fostered by English and foreign newspaper reporters and writers habitually making statements on the subject which cannot be justified by the facts. Very few of the explosions of 1886 and 1887 have been coincident with a low barometer, and out of the list of disasters in the eleven years 1875–85 given by Sir Frederick Abel only 18·75 per cent. of the accidents, and 17·4 per cent. of the deaths, occurred when the mercury was at 29½ inches or below. One half of this small percentage of explosions took place with a low but rapidly rising barometer, and at a time when gas is shown by careful observations to have commenced issuing from the strata. The importance of studying the influence of anticyclones in connection with mining was still further emphasized, as coal-dust is more inflammable and more difficult to moisten when the air is cold and dry than in the time of cyclones, when the air is warm and damp. In the new rooms recently added to the Institute, an interesting series of appliances for use in mines was on exhibition, a number of safety-lamps of different patterns, oil and electric, the Fleuss apparatus, Loeb's respirator, safety winding-gear, anemometers, and a collection of photographs of miners actually at work, hewing, timbering, &c., from Mr. Sopwith, Cannock Chase.

Mr. Goschen will deliver his inaugural address as President of the Royal Statistical Society on Tuesday, December 6, when the first ordinary meeting of the present session will be held. The Statistical Society usually holds its meetings at the Royal School of Mines in Jermyn Street; but on the present occasion, as the Council have reason to expect an extra large attendance of the Fellows and their friends, it has been arranged that the meeting shall take place at Willis's Rooms, King Street, St. James's, at the usual hour, 7.45 p.m.

We regret to have to record the sudden death of Dr. Max Schuster, Privat-docent and Assistant in the University of Vienna. His laborious researches on the optical characters of the Felspars are known to every petrologist; and his treatise on the features of Danburite, in its almost painful minuteness of observation and calculation, is one of the seven wonders of modern crystallography. His kindliness of manner, and his enthusiasm,

won him the affection and esteem of all who had the good fortune to know him: by his death, at the early age of thirty, Mineralogy is deprived of the most promising of its investigators.

Dr. Gustav Theodor Fachner, the well-known physicist, died at Leipzig on November 18. He was born near Moscow, on April 19, 1801.

Naturalists have learned with much satisfaction that Mr. William Davison has been appointed to the Curatorship of the Singapore Museum. Mr. Davison's appointment has been objected to on the score that he is a "mere collector," but, even if this were the case, it would scarcely be denied that he is one of the best collectors ever known. Certainly he is without a rival in the present day, and only Wallace or Bates or Clarence Buckley could be named along with him. Such objectors, however, are singularly ignorant of Mr. Davison's career. For thirteen years he was Curator to Mr. Allan Hume, whose private museum was one of the best managed in the world, and he has conducted some of the most important scientific expeditions of modern times. At Singapore he will have the opportunity of completing his explorations in Malacca, which he commenced some ten years ago, when he traversed the whole of the western half of the peninsula, but was not able to penetrate to the mountainous regions of the eastern half. A rich field of discovery awaits him, if we may judge from the collections sent by Mr. Wray to the British Museum from the Larut Range behind Perak. Every naturalist may depend upon the hearty co-operation of Mr. Davison in any branch of science, and we shall expect to see that, in the course of a few years, Singapore possesses one of the most famous natural history collections in the East.

On Saturday last Mr. Francis Galton gave at the South Kensington Museum the first of three lectures on heredity and nurture. Towards the close of the lecture Mr. Galton spoke of the advantages which might be derived from the establishment of a permanent anthropometric laboratory. An anthropometric laboratory is a place where a person may have any of his various faculties measured in the best possible way, at a small cost, and where duplicates of his measurements may be preserved, as private documents for his own future use and reference. Such an institution would contain apparatus both of the simpler kind used for weighings and measurings, and for determinations of chest capacity, muscular strength, and swiftness, and that of a more delicate description, used in what is technically called psycho-physical research, for determining the efficiency of each of the various senses and certain mental constants. Instruction might be afforded to those who wished to make measurements at home, together with information about instruments and the registration of results. An attached library would contain works relating to the respective influences of heredity and nurture. These would include statistical, medical, hygienic, and other memoirs in various languages, that are now either scattered through our different scientific libraries or do not exist in any of them. Duplicates of the measurements, but without the names attached, would form a growing mass of material accessible to statisticians. From conversation with friends, Mr. Galton gathers that the library might fulfil a welcome purpose in becoming a receptacle for biographies and family records, which would be in two classes—the one to be preserved as private documents, accessible only to persons authorized by the depositor; and the other as ordinary books, whether they were in manuscript or in print. Mr. Galton will be grateful for any communications that may show whether sufficient interest really exists to justify a serious attempt to found an Anthropometric Laboratory and Family Record Office, as well as for any helpful suggestions towards the better carrying out of the idea.

Comparing the proceedings of the Anthropological Sections of the British and American Associations for the Advancement of Science, the American journal *Science* decides that the anthropological work done in the English institution is superior to that of the Americans. "We do not mean to say," it states, "that there are no vague theories held by British men of science, or that no eminent work is done by Americans; but the favourite studies of ethnologists as a whole, and as expressed in the subjects of papers presented to the English Association, seem to be of a more general and of a higher scientific character than they are here."

In a recent number of the *Korrespondenzblatt* of the German Society for Anthropology and Ethnology, there is a good account of the archæological explorations which have been carried on near Reichenhall, in the south-eastern part of Bavaria. An ancient cemetery was discovered here some time ago, and no fewer than eighty-five skulls have been found, with some well-preserved skeletons, and a great quantity of weapons and ornaments. The skulls are of the primitive Germanic type, and the skeletons show that the people must have been about the size of the existing population of the Bavarian highlands. Among the treasures which have been recovered is a thin gold coin, evidently an imitation of a Roman coin. This coin probably belongs to the fifth century, and it may have found its way to this part of Germany in consequence of the intimate relations which are known to have existed between the ancient inhabitants of Bavaria and the Langobardi.

An interesting account of a series of experiments upon the so-called alloy between the metals sodium and potassium is given by M. Joannis in the current number of the *Annales de Chimie et Physique*. For some years it has been known that, although in many respects so similar, the e two metals possess a certain affinity for each other, and unite under suitable circumstances to form a liquid amalgam-like substance. M. Joannis has at length shown that a definite compound, NaK_2, is formed with considerable evolution of heat when the fused metals are brought together in the right proportion. In order to prove this fact, thermo-chemical methods were resorted to, liquid mixtures of the composition Na_2K, NaK, NaK_2, and NaK_3 being successively introduced into the calorimeter. The hydrogen liberated by decomposition of the water in the calorimeter was caused to pass first through a perforated platinum plate, and afterwards through a long thin-walled glass spiral, eventually escaping in minute bubbles through the water itself, after becoming reduced to the temperature of the calorimeter. The liquid mixture of metals was gradually introduced by means of an ingenious apparatus consisting of a drawn-out delivery-tube containing the alloy between two layers of protecting naphtha, and which, by means of a valve, could be placed in communication with a reservoir of compressed air, so that, by regulating the valve, a gentle stream of the liquid could be forced out as required. When the calorimetrical experiments were concluded, the amount of alkali was determined in an aliquot part of the water in the calorimeter, and thus the amount of metal used could be arrived at. From the data afforded by these experiments, M. Joannis appears to have conclusively shown that the only stable compound is NaK_2, all others being mixtures of this with excess of one or other of the two metals. It is very satisfactory that a reliable method has at last been found of distinguishing between true compounds and physical mixtures of metals, and rather remarkable that one of the earlier analyses of the most stable combination of sodium and potassium gave as the percentage of potassium 76·5, a number which closely approximates to that required for NaK_2.

Since the Ben Nevis Observatory was opened four years ago, eleven cases of St. Elmo's fire have been recorded. These case

have been examined by Mr. Rankin, first assistant, and compared with the other observations made from thirty hours previous to eighteen hours subsequent to the times of occurrence. It would appear that the phenomenon has almost invariably occurred when the temperature, after having been for some time distinctly above the mean of the season, has been falling for about twenty-four hours. During this time, while the temperature fell, the barometer also continued to fall till within three hours of the time of St. Elmo's fire, and thereafter rose steadily. The wind is west-south-westerly till the barometer falls to the minimum, and then shifts to north-west. The accompanying weather is fog, squalls, and unusually large-sized soft hail. Mr. Rankin further compared the phenomena with the weather charts of the Meteorological Office, with the result that pressure was in all cases highest over the south-west and south of Europe, diminishing, however, gradually towards North-Western Europe, where pressure was comparatively low, with several satellite cyclones skirting the northern coasts of the British Islands. Of the eleven cases, two occurred in September, three in October, four in November, one in January, and one in February.

The Meteorological Report of the Straits Settlements for the year 1886 has been issued. Charts are appended, showing the mean annual elements from 1870 to 1886.

Dr. A. Müttrich has published the twelfth Annual Report of the forest meteorological observations of Germany. The stations now number sixteen, and the observations of temperature, &c., are made in the open, in the forests, and in the crowns of the trees. Monthly and yearly *résumés* are given, but there is no discussion of the results. Special attention is paid to evaporation and rainfall.

Several earthquakes are reported from Carinthia and Styria. On November 14 a shock was felt at Klagenfurt at 10 p.m., which lasted for four seconds. At Bleiburg, as well as over the whole of Carinthia, severe oscillations were noticed. Reports state that shocks occurred at 11 p.m. at Graz and Saldenhofen, and at 4 p.m. at Ostrau-Witkowitz. At Cavaillon and St. Saturnin-lès-Avignon (Vaucluse) oscillations were felt on November 14. At Cavaillon eleven houses were damaged. On November 17, at 8.55 a.m., two severe shocks occurred at Zafferana, near Etna. A severe earthquake, lasting for ten minutes, occurred in Iceland on October 28; at Reikianaes large chasms appeared in the ground.

The second session of the Liverpool Biological Society was opened on October 29, when Dr. J. J. Drysdale, the President, delivered an address on the definition of life as affected by the protoplasmic theory. The Council's Report showed the affairs of the Society to be in a very satisfactory condition, the number of members amounting to 121. At the second meeting of the session, held on November 12, the following papers, dealing with the history of the foundation of the Zoological Station on Puffin Island, Anglesey, were read: account of the foundation of the Station, and of the general work done during the past summer, by Prof. Herdman; report on the land Mollusca, by Alfred Leicester; report on the higher Crustacea, by A. O. Walker; report on the Actiniaria, by J. W. Ellis; report on the Copepoda, by J. C. Thompson; report on the Polyzoa, by J. Lomas.

At a meeting of the Aristotelian Society on November 21, Dr J. McK. Cattell, of the University of Pennsylvania, read a paper on "The Psychological Laboratory of Leipzig." He explained how experimental psychology undertakes to analyze and measure mental phenomena, and advocated the systematic work of the laboratory, both for the education of students and for the advancement of knowledge. An account was then given of the psychological laboratory at Leipzig, founded by Prof. Wundt in 1879, and of the researches which have been undertaken in it, including experiments on the measurement of sensation, the duration of mental processes, attention, memory, and other subjects. The paper was followed by a discussion in which Prof. Bain, Prof. Dunstan, and others took part.

An address on the Army Medical School, delivered some months ago by Sir Henry W. Acland, at Netley Hospital, at the distribution of prizes, has now been published. The author explains that he issues the address because of an opinion recently expressed before a Committee of the House of Commons by the Accountant-General of the Army, to the effect that the Army Medical School might be advantageously dispensed with. Sir Henry hopes that the Accountant-General of the Army may revise his opinion, and propose hereafter to increase the grant and to enlarge the scope and means of the school.

The American Industrial Education Association is about to issue leaflets, giving concise information on points of its work regarding which questions are continually asked. The first leaflet will state compactly what the argument for manual training is.

In a Report just published by the Foreign Office, on the trade of the Nyassa Territories, Mr. Hawes, the newly-appointed Consul, describes the Strophanthus, a climbing plant from which the natives extract a strong poison, and which is beginning to find its way into the London market. It is called by the natives *kombe*, and is found at a low level, and not apparently on high land. The supplies hitherto obtained have been drawn from the right bank of the Shire River below the Murchison Rapids. There is apparently more than one species, or at least variety, the distinguishing feature being a much smaller pod and fewer seeds. At present, information relative to the varieties is scant. It is a strong climbing plant, and is always found in the vicinity of high trees, on which it supports itself. The stem varies in diameter, but has an average of a few inches. It lies on the ground in folds, the branches supporting themselves on the nearest trees. The young branches are in appearance not unlike the elder. The fruit grows in pairs, and has a peculiar appearance, very like a pair of immense horns hanging to a slender twig. It begins to ripen in July, and lasts till the end of September. The native method of preparing the poison is very simple. They first clean the seeds of their hairy appendages, and then pound them up in a mortar until they have reduced them to a pulp. A little water is then added. This is done by using the bark of a tree containing a gummy substance, which helps to keep the poison on the arrow, in the event of its striking against a bone. The poison thus prepared is spread upon the arrow, and allowed to dry; game wounded by arrows poisoned with Strophanthus die quickly. The flesh is eaten without evil effect. The only precaution taken is to squeeze the juice of the baobab bark on the wound made by the arrow, and this counteracts the evil effects of the poison. Buffalo and all smaller game are killed by this poison.

The additions to the Zoological Society's Gardens during the past week include a Cheetah (*Cynælurus jubatus*) from East Africa, presented by Mr. Frederick Holmwood; two White-backed Piping Crows (*Gymnorhina leuconota*) from Tasmania, presented by Mr. C. Sadler; a Crowned Hawk Eagle (*Spizætus coronatus*) from South Africa, presented by Mr. E. A. Hart; two Cereopsis Geese (*Cereopsis novæ-hollandiæ*) from Australia, presented by His Grace the Duke of Northumberland, P.C., K.G.; a Common Crossbill (*Loxia curvirostra*) British, presented by Mr. S. R. Armord; a Knot (*Tringa canutus*) British, presented by Mr. Howard Bunn; two Thunder Fish (*Misgurnus fossilis*) from the Baltic Sea; four Chub (*Leuciscus cephalus*) from British fresh waters, presented by Messrs. Paul and Co.; two Cape Crowned Cranes (*Balearica chrysopelargus*) from East Africa, a Mealy Amazon (*Chrysotis farinosa*) from South America, deposited.

OUR ASTRONOMICAL COLUMN.

Probable New Variables.—Mr. John Tebbutt calls attention in the *Observatory* for November to the double star OΣ 256, one of the components of which appears to be variable; for during the occultation of the star on August 22 the preceding component appeared very distinctly the brighter of the two, whilst Crossley and Gledhill, in their "Hand-book of Double Stars," regard this star as the companion. Struve was apparently the first to draw attention to the probable variability of this star, for whilst he usually estimated the preceding star as the brighter by half a magnitude, Dembowski recorded it as being the fainter by that amount.

Dr. Bauschinger (*Astr. Nach.* No. 2810), finds that a star in Libra, Lam_3 1875, Munich Zones 695—place for 1855·0, R.A. 15h. 4m. 1·5s., Decl. 5° 27′·6 S.—is also probably variable. Lamont gives the star as of the eighth magnitude; Dr. Bauschinger finds it 9·2 m.; it is wanting in the southern Durchmusterung. Dr. Schönfeld writes that he observed the star on two, if not three occasions; once as 10 m. and once as 12 m. It should therefore be added in the *Bonn. Beob.* vol. viii., after −5° No. 4028, as:—

"Var. 15h. 4m. 2·5s., 5° 27′·5 M."

Names of Minor Planets.—Minor Planet No. 268 has received the name of Adorea; No. 270 that of Anahita.

The Spectra of Oxygen and Carbon compared with that of the Sun.—Prof. Trowbridge and Hutchins have presented to the American Academy of Arts and Sciences a paper on the spectra of oxygen and carbon as compared with that of the sun. In the case of the former element, Dr. Henry Draper had convinced himself that there were bright lines in the solar spectrum corresponding to the bright lines of oxygen, whilst his brother, Prof. J. C. Draper, had identified the oxygen with faint dark lines, but the present experimenters conclude that "so far as concerns the spark spectrum in air and the solar spectrum from wave-lengths 3749·8 to 5033·85 they can safely affirm that there is no physical connection between them." They "have photographed the sun's spectrum every day that the sun has shone for nearly five months, without finding a line that could with certainty be pronounced brighter than its neighbours"; the powerful dispersion given by the large concave Rowland grating employed by Messrs. Trowbridge and Hutchins causing the "bright bands to vanish," which Dr. H. Draper thought he had discovered, and which seemed conspicuous with the dispersion he used, whilst it showed at the same time that there was no real correspondence between the oxygen lines and the dark lines Prof. J. C. Draper had identified with them. Lack of sufficient instrumental power had led both of the two earlier observers astray.

With regard to carbon, Messrs. Trowbridge and Hutchins are of opinion "that the fluted spectrum of carbon is an example of the reversal of the lines of a vapour in its own vapour," and they find a striking coincidence in many cases between the spaces separating the fine bright lines of the flutings and dark lines in the solar spectrum, twenty-eight such coincidences being traced within the limit of ten wave-lengths in the fluting at wave-length 3883·7. Their hypothesis as to the origin of the flutings leads them "to conclude that, at the point of the sun's atmosphere where the carbon is volatilized so as to produce the peculiar arrangement of reversals observed, the temperature of the sun approximates to that of the voltaic arc."

Olbers' Comet, 1887.—The following ephemeris for Berlin midnight for this object is in continuation of that given in Nature, vol. xxxvi. p. 588, and vol. xxxvii. p. 37, and is by Herr Tetens (*Astr. Nach.*, No. 2813):—

1887.	R.A. h. m. s.	Decl. ° ′	Log r.	Log Δ.	Brightness.
Dec. 1	15 26 36	7 10·9 N.	0·1594	0·3354	0·84
3	15 32 7	6 34·7			
5	15 37 31	5 59·5	0·1692	0·3417	0·78
7	15 42 49	5 25·1			
9	15 48 0	4 51·6	0·1790	0·3478	0·73
11	15 53 4	4 19·1			
13	15 58 2	3 47·5	0·1890	0·3537	0·68
15	16 2 54	3 16·9			
17	16 7 41	2 47·2 N.	0·1990	0·3593	0·63

The brightness on August 27 is taken as unity.

A Vienna observation of October 21 gives the error of the ephemeris as R.A. + 3s. and Decl. + 0′·2, and this will probably slowly increase.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 DECEMBER 4–10.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on December 4

Sun rises, 7h. 50m.; souths, 11h. 50m. 19·6s.; sets, 15h. 51m.: right asc. on meridian, 16h. 42·2m.; decl. 22° 15′ S. Sidereal Time at Sunset, 20h. 44m.

Moon (at Last Quarter on December 8, 3h.) rises, 18h. 39m.*; souths, 2h. 44m.; sets, 10h. 44m.: right asc. on meridian, 7h. 34·0m.; decl. 20° 0′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	5 46	10 27	15 8	15 18·9	15 49 S.
Venus	3 20	8 45	14 10	13 36·7	7 36 S.
Mars	0 53	7 8	13 23	11 58·9	2 8 N.
Jupiter	5 56	10 27	14 58	15 18·9	17 25 S.
Saturn	19 57*	3 45	11 33	8 35·1	19 6 N.
Uranus	2 35	8 9	13 43	13 0·6	5 46 S.
Neptune..	15 11	22 52	6 33*	3 45·8	18 5 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultation of Star by the Moon (visible at Greenwich).

Dec.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
6	7 Leonis	6½	5 18	6 27	59 300

Dec.	h.	
4	11	Mercury in conjunction with and 1° 35′ north of Jupiter.
5	5	Saturn in conjunction with and 0° 51′ north of the Moon.
5	8	Mercury at greatest elongation from the Sun, 21° west.
9	5	Mars in conjunction with and 2° 10′ south of the Moon.

Saturn, December 4.—Outer major axis of outer ring = 44″·3; outer minor axis of outer ring = 14″·2; southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Dec. 7,	0 46	*m*
R Sculptoris	1 21·8	33 8 S.	,, 10,		*M*
Algol	3 0·8	40 31 N.	,, 6,	2 46	*m*
			,, 8,	23 35	*m*
λ Tauri	3 54·4	12 10 N.	,, 7,	2 17	*m*
S Orionis	5 23·4	4 47 S.	,, 7,		*M*
ζ Geminorum	6 57·4	20 44 N.	,, 9,	19 0	*m*
R Canis Majoris	7 14·3	16 11 S.	,, 8,	20 56	*m*
			,, 10,	0 12	*m*
T Cancri	8 50·2	20 17 N.	,, 6,		*M*
S Virginis	13 27·1	6 37 S.	,, 9,		*M*
U Coronæ	15 13·6	32 4 N.	,, 7,	23 4	*m*
β Lyræ	18 45·9	33 14 N.	,, 5,	4 0	*m*
S Vulpeculæ	19 43·8	27 0 N.	,, 7,		*M*
Y Cygni	20 46·8	34 10 N.	,, 5,	22 23	*m*
			,, 8,	22 17	*m*
R Vulpeculæ	20 59·4	23 22 N.	,, 5,		*m*
δ Cygni	22 25·0	57 50 N.	,, 6,	23 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near γ Persei	44	56 N.	Very slow; faint.
The Taurids II.	80	23 N.	Slow; bright.
The Geminids	107	33 N.	Swift; short.
Near δ Geminorum	110	24 N.	Rather swift.
Near π Leonis	145	8 N.	Swift; streaks.
Near β Ursæ Majoris.	162	58 N.	Very swift; streaks.

GEOGRAPHICAL NOTES.

In the *Bulletin* of the California Academy of Sciences for June, Mr. C. M. Richter re-examines all the data relating to the ocean currents contiguous to the coast of California, with the result that existing charts are in many cases found to be wrong, and that great diversity of opinion exists as to the real character and origin of these currents.

In the new number of the *Mouvement Géographique* the various rumours that have been afloat as to disasters which have happened to Mr. Stanley's Expedition are examined, and, when tested by known facts and the latest trustworthy information from Mr. Stanley himself and his officers, are shown to be without justification.

Mr. Montagu Kerr sailed from London last Thursday for Zanzibar, for the purpose of attempting to cross Africa by a new route. It is a mistake to refer to Mr. Kerr's private expedition as intended for the further "relief" of Emin Pasha. It has nothing whatever to do with Emin Pasha; though, no doubt, Mr. Kerr will shape his course through Masai Land towards Wadelai as his first stage, and may be guided by Emin's advice as to his further course. His main object after reaching Wadelai will be to proceed in a north-westerly direction towards Lake Chad, solving as far as possible by the way the hydrography of the Welle and Shari regions. After exploring around Lake Chad, Mr. Kerr may make for the Niger, though it is possible enough he will go on northwards in the direction of Tripoli. Since his return from his South African journey, Mr. Kerr has been diligently qualifying himself for scientific observation.

The paper on Monday at the Royal Geographical Society was one of unusual originality; it described Mr. A. D. Carey's two years' journey around and across Turkistan and into the north of Tibet. Mr. Carey, who was accompanied by the well-known Central Asiatic traveller, Mr. Dalgleish, describes so many new features that it is impossible to follow his route throughout on any map. Although his route coincided to some extent with those of Prejevalsky, he has been able to supplement the Russian traveller's observations in many directions. Mr. Carey, starting from Leh in Ladak, crossed the western part of Tibet and the western continuation of the Altyn Tagh, to Kiria in the south-west corner of the great Tarim Desert. Thence along the Khoten River he reached the Tarim, the course of which he followed, with excursions to various places on the route, as far as Lob Nor. The hydrography of this interesting river Mr. Carey has helped considerably to clear up. Some time was spent about the Lob Nor region, and then Mr. Carey, amid many difficulties, endeavoured to penetrate as far as possible into Tibet; but as his time was limited he did not succeed in getting further than the Ma Chu, about half-way between the Kuen Lun and the Tangla Range. But in his wanderings to and fro in the great marshy and desert plain that lies between the Altyn Tagh Mountains and the Kuen Lun, he has added something to our knowledge of one of the most interesting regions of Central Asia. From the Ma Chu, Mr. Carey struck almost direct northwards by Sachu to Hami, across the Gobi Desert. Then by a great sweep he traversed the northern border of Turkistan, by Turfan, Karashahr, Kuchir, Aksu, and Yarkand, back to Leh, two years after he left it. As he says, he thus completed the circuit of Chinese Turkistan, and, Kashgar excepted, visited every important place in it. The chief characteristic of the country is its extreme poverty. It may be described as a huge desert fringed by a few small patches of cultivation. The only really good strip of country of considerable size is the western portion, comprising Kargalik, Yarkand, and Kashgar. To the north a succession of very small oases extends along the foot of the Tian Shan Mountains, the stretches of intervening desert becoming longer as the traveller goes further to the east. The eastern extremity of the province is desert pure and simple, and so is the southern extremity as far west as Kiria, with the exception of the small oases of Charchand and Chaklik. The central portion is chiefly desert, except along the Tarim and in the Lob Nor region. Mr. Carey gives some useful notes on the different classes of people he met with, and occasionally a jotting on the natural history of the region. But the chief scientific result of Mr. Carey's journey is the excellent map which Mr. Dalgleish carefully plotted every day, and which covers many sheets; it is being reduced, and will be published by the Royal Geographical Society.

The correspondence from Major Barttlet, Mr. Stanley's second in command, from his station on the Aruwimi, shows that all is going well, and that if there are any dangers they will be due to the Arabs, and not to the natives. For the many rumours of disaster to the Expedition there is no foundation in fact; there is positively no news from Mr. Stanley since he left the Aruwimi, and in this case no news is good news, for bad news travels as rapidly in Africa as elsewhere.

THE ANNIVERSARY MEETING OF THE ROYAL SOCIETY.

The Royal Society held its Anniversary Meeting yesterday for the purpose of electing officers and presenting medals. The President delivered the address which we print below. After the meeting the Fellows dined together at Willis's Rooms, and the attendance was larger than on any previous occasion, nearly 200 Fellows being present.

During the past year death has removed from us fifteen of our Fellows and one foreign Member. It is remarkable that no less than six of these had reached the age which the Psalmist takes for the extreme duration of human life, while the average age of the whole exceeds seventy-five years. Within two months after our last anniversary Sir Joseph Whitworth died, at the age of eighty-four. Starting from a humble beginning, he attained, through his talent and steady application, a commanding position among constructors of machinery and heavy ordnance, and the truth of surface and accuracy of dimensions of what came from his workshop are probably unrivalled.

Sir Walter Elliot, who was still older, combined a high official position in India with the pursuit of natural history, and was the author of several papers in scientific serials. John Hymers and Thomas Gaskin were mathematicians well known to Cambridge men of some standing, and were both elected Fellows of our Society nearly half a century ago. The former was the author of various mathematical text-books, which for a long time were those chiefly used in their respective subjects by Cambridge students for mathematical honours. The latter, once a colleague of my own in a mathematical honour examination, was famed for his skill in the solution of problems, though he has not left much behind him in the way of mathematical writings, beyond a book containing the solution of a variety of problems. In Robert Hunt we have lost an aged Fellow whose name is well known in connection with the study of the action of light in producing chemical changes, and on vegetation. In Joseph Baxendell we had a man who during a long life was a diligent observer of astronomical and meteorological phenomena. John Arthur Phillips, a geologist who attended most particularly to the chemical origin of mineralogical and geological phenomena, was the author of several papers, some of which appeared in our own Proceedings. It is not long since Sir Julius von Haast was among us, apparently in full vigour, having come to England in connection with the Colonial Exhibition, and now this distinguished geologist and naturalist is no more. The Earl of Iddesleigh was suddenly carried off in the midst of the duties belonging to an important office in the State, whilst Beresford-Hope has succumbed to an illness of some duration. These two joined us under the statute which enables the Council to recommend to the Society for election, in addition to the fifteen who are selected in the ordinary way, and nearly always on account of their scientific claims, persons who are members of Her Majesty's Most Honourable Privy Council, and whose ability is thus attested, though they are not usually men of science. From the list of foreign Members, one name has disappeared which has become a household word among the physicists of all civilized nations. The name of Kirchhoff will ever be remembered as that of the introducer, conjointly with Bunsen, of spectral analysis into the regular work of the chemical laboratory, a step which has been so fertile in results. To him too we owe the reference of the dark lines of the solar spectrum to the absorption of portions of light coming from deeper portions of the sun by the vapours of substances which in the condition of incandescent vapour themselves emit bright lines in corresponding positions; and to him therefore we are indebted for the detection of chemical elements in the sun and stars, though partial anticipations of these discoveries had been made by others. The fertility of these researches, and the attention which they consequently excited, should not make us

forget the many important investigations in mathematical physics of which Kirchhoff was the author.

The present year is memorable as the Jubilee of the reign of Her Most Gracious Majesty our beloved Sovereign, and the Patron of our Society. An address of congratulation on this auspicious event was prepared by the Council, and was graciously received by Her Majesty in Windsor Castle at the hands of your President, who was accompanied on that occasion by the senior Secretary.

It happens that this same year is also the Jubilee of the Electric Telegraph, if we date from the first construction of a telegraph on an actually working scale, as distinguished from preparatory experiments made only in the laboratory. The Jubilee was duly celebrated by the Society of Telegraph Engineers. The name of our former Fellow Wheatstone will go down to posterity as having occupied a foremost place in this great practical application of Oersted's fertile discovery.

I will just briefly allude to another outcome of scientific research. The last half-century was well advanced when our Fellow Dr. Perkin, by utilizing a colour reaction which had been employed by chemists as a test for aniline, laid the foundation of the industry of the coal-tar colours, which has now attained such great proportions, and the investigation of the chemical theory of which has occupied the attention of so many eminent chemists from our own Fellow Dr. Hofmann onwards.

There is yet another Jubilee connected with this same year in which our Society is if possible still more closely connected: it is now just 200 years since the publication of the first edition of that immortal work, "The Principia" of Newton. Some of the important results embodied in the Principia had previously been communicated to the Royal Society.

But, restricting our view to the last half-century alone, we can hardly help casting a glance at the progress of science, and of the practical applications of science, within that period. In electricity, I have already referred to the electric telegraph, now passed into the management of a department of the State, and inwoven in our daily life, with its wires stretching all round the earth like the nerves in the body, and placing us in immediate connection with distant countries. Much more recent than the invention of the electric telegraph is that, in some respects, still more wonderful apparatus for communication at a distance afforded by the telephone. The application of electricity to lighting purposes, of which we have availed ourselves for the lighting of the apartments of our own Society, is an industrial outcome of Faraday's discovery of magneto-electric induction which could not have been thought of when the account of that discovery first appeared in our Transactions. It is true that what I have just been mentioning with respect to electricity consists of industrial applications rather than the discovery of new scientific principles; but these industrial applications react upon abstract science beneficially in more ways than one. The possibility of useful applications induces theorists to engage in investigations which they might not otherwise have thought of, the result of which is oftentimes to lead them to a clearer apprehension of fundamental principles, and to induce them to undertake exact quantitative determinations of fundamental constants. Moreover, the grand scale on which apparatus for actual commercial use has to be constructed renders it possible for scientific men, through the courtesy of those who direct the construction, to make interesting experiments on a scale the cost of which would be quite prohibitory if it were a matter of science pure and simple. Take for example the experiments made by Faraday on the first cable prepared for the attempt to span over the Atlantic Ocean.

When we think of the progress of science, both abstract and applied, during the last half-century, we can hardly help speculating as to the possible increase of scientific knowledge half a century hence. Perhaps we might be tempted to think that the mine must have been so far worked that no great quantity of precious ore can still be left, except what lies too deep for human power to extract. Yet surely the progress of knowledge in the past warns us against any hasty conclusion of the kind. How often have accessions to our knowledge been made which were quite unforeseen and quite unexpected, and how can we say what great discovery may not be made at any moment, and what a flood of light may not result from it?

In what direction such discoveries may be made, it would be rash indeed to attempt to predict. Yet one cannot help thinking of one or two cases in which we seem almost in touch of what if we could reach it would probably give us an insight into the processes of Nature of which we have little idea at present. Take for example the theory of electricity as contrasted with the theory of light. In the latter we have the laws of reflection and refraction, which have long been known, the remarkable phenomenon of interference, the curious appearances which we designate by phenomena of diffraction. But all these fall in the most simple and natural way into their places when we have arrived at the answer to the question, What is light? which is furnished by the statement, Light consists in the undulations of an elastic medium. But we are not at present able to give a similar answer to the question, What is electricity? The appropriate idea has yet to be found. We know a great deal about its laws, and its connection with magnetism and chemical action; we are able to measure accurately physical constants relating to it; we make it subservient to the wants of daily life; and yet we are unable to answer the question what is it? Could we only give a definite answer to this question, it seems likely that the production of electricity by friction, electrostatic attractions and repulsions, the laws of electrodynamics, those of thermodynamics, the nature of magnetism, and magneto-electric phenomena would prove to be all simple deductions from the one fundamental idea. Nay more: so closely is electricity related to chemical action, that could we only clearly apprehend the nature of electricity, it seems not unlikely that an unexpected flood of light might be shed on chemical combination.

Let me refer to one other instance in which a large accession to our present knowledge seems not altogether hopeless. We know that when an electric discharge is passed through a given gas, or between electrodes formed of a given substance, an analysis of the spark reveals a usually complicated spectrum of bright lines characteristic of the chemical substances present. The arrangement of the lines in most cases seems capricious, while in other instances we have repetitions of lines, or else rhythmical flutings, indicative of law, though one of no simple character. There can be no reasonable doubt that the periodic times indicated by the bright lines seen in the spectrum are those belonging to the component vibrations of the chemical molecules themselves; and the appearance is just such as would be produced by a tolerably complex dynamical system vibrating under the action of internal forces of restitution. Now such a system may really be composed of two or more simpler systems, held together less firmly than the parts of one of the simpler systems; and the complex vibrations of the whole may be made up of those of the several simpler systems, modified, however, by their mutual connection, together it may be with others due to the mutual connection of the simpler systems regarded each as a whole. It is conceivable that relations of chemical composition may thus be pointed out even between substances which we deem elementary, and which from their great stability we may, perhaps, never be able actually to decompose.

But I must apologize for having taken up your time with speculations as to the future; I will turn now to some mention of the action of your Council during the past year, and of the progress made by Committees appointed by the Council.

In response to an invitation received from the Academy of Sciences of Paris, that the Society should be represented at the International Conference of Astronomers, which it was proposed should assemble in Paris, in the spring, for the purpose of deliberating about concerted action for obtaining a complete map of the starry heavens by means of photography, your Council requested the Astronomer Royal to represent the Society on that occasion. The Conference met, as it was proposed, last spring; and I believe that the English astronomers at least think that a good foundation has been laid for concerted action in that great undertaking.

As the Fellows are already aware from a circular which has been issued, the Council has decided to make a change in the mode of publication of the Philosophical Transactions. The average yearly volume is a good deal more bulky now than it was at the beginning of the century, and its size is such as not unfrequently to make it desirable to bind one volume in two. The sciences, moreover, which are represented in the Philosophical Transactions, divide themselves very naturally into two groups: mathematics, physics, and chemistry forming one, and the biological sciences the other. The Council has decided to issue the Transactions from henceforth in two series, corresponding to these two divisions, and a yearly volume will appear in each series. It is hoped that this arrangement will be conducive to an earlier publication, as the numeration of the pages in the two series can go on independently. The indi-

vidual papers will also be issued separately, so that Fellows who prefer receiving them in this way can have them as soon as they are printed. Moreover, the issue of the Transactions in two series will enable institutions that are concerned with one only of the two groups of subjects, and that are not on our list for free presentation, to purchase for their libraries the series devoted to that group, instead of going to the expense of procuring the whole Transactions.

I am happy to be able to announce that the publication of the *Challenger* Report is now nearly finished. Twenty-eight volumes, some in two parts, have now been published, and these are all in the Society's library.

The Krakatão Committee have now all but completed their labours. A vast amount of information on the phenomena related to that most remarkable volcanic explosion has been collected and digested, different branches of the inquiry having been taken up by different members of the Committee. An estimate has been made of the cost of publication of the Report, and the Council has decided that it should be published as a separate work, and has voted the sum required for publication. The printing of the volume is now far advanced, and in a very few weeks it will in all probability be in the hands of the public.

The reports of the observers of the total solar eclipse of August last year are now coming in. From inquiries I have made I am in hopes that they will all be in by the end of the year. It is obviously convenient that they should all be dealt with together, rather than appear in a scattered form for the sake of a slightly earlier publication of those which happen to be read first.

I mentioned in my last address that with respect to this eclipse the Council, acting in accordance with the recommendations of the Eclipse Committee, had decided to confine themselves to an expedition to Grenada, without attempting another to Benguela on the Western Coast of Africa, which if sent out from this country would have been a good deal more costly, and of which the success, judging by such accounts of the climate of Benguela and its neighbourhood as we could procure, seemed very doubtful. The Committee guaranteed, however, £100 towards the expense of a small expedition from the Cape in case Her Majesty's Astronomer at that place should be in a condition to organize one. Sir W. J. Hunt-Grubbe, the Admiral in command at that station, was prepared to render every assistance in his power. Ultimately, however, it was not found practicable to organize an expedition from the Cape, and so the English observations of the eclipse were confined to those taken at Grenada. I have heard that the day of the eclipse was fine at Benguela, but there were no astronomers of any nation there to take advantage of it. It may be doubted, however, whether, in spite of the fineness, the haze which is said to prevail so much on that coast at that time of year, might not materially have interfered with the observations.

The boring in the Delta of the Nile has been continued, by the favour of the War Office, under the able and zealous superintendence of Captain Dickinson, R.E. As I mentioned last year, the Committee thought it best to concentrate their efforts on a single boring until rock should be reached, or else a stratum of such a character as to show that the alluvial or drifted deposit had been got through. This result has not at present been obtained. The boring at Zagazig reached the depth of 324 feet, when the tube broke, and stopped for the time further progress. It is, however, a matter of interest and importance to know that the drift or deposit extends to so great a depth. Geologists attach so much importance to the prosecution of the inquiry that at the suggestion of the Delta Committee an application was made to the Government Grant Committee for a grant of £500, which was acceded to by the Committee. This sum would not suffice for the prosecution of the inquiry to the extent contemplated; but it was thought that with such a sum as a nucleus extraneous pecuniary assistance might be obtained from Societies or individuals specially interested in the inquiry, and the Council have authorized the Delta Committee to avail themselves of such aid.

The meetings of Council and Committees continue to be very numerous, and no less than twenty-two Committees and Sub-Committees have been at work during the session.

The number of papers communicated to the Society continues to increase. In 1884-85 the number was 93; in 1885-86 it was 113; and in the past session, 129.

Since the last Anniversary one complete part of the Philosophical Transactions, and thirty-two papers towards the new volume have been published; the whole comprising no less than 1482 pages of letterpress and seventy-six plates. In the same period twelve numbers of the Proceedings, containing 984 pages, have appeared.

The task of preparing the MS. of the Catalogue of Scientific Papers, decade 1874 to 1883, has proved far heavier than was anticipated, and the matter very far exceeds in bulk that of the previous decade. The cataloguing of papers from the volumes in our own library has long been finished, but the work of gleaning stray papers from works in other libraries which we do not possess has proved more arduous than was expected, and even now is not quite completed. It is confidently hoped, however, that the MS. will be completed for the press during the coming session.

The distribution and exchange of duplicates from our library commenced last session has been continued, and several defective series among the periodicals on our shelves have been made good. The general work of the library has received careful attention at the hands of Mr. Alfred White, who shortly before the last Anniversary was appointed to the office of Assistant Librarian.

The Copley Medal for the year has been awarded to the eminent botanist, your former President, Sir Joseph Dalton Hooker. It is impossible, within the limits to which I must confine myself on the present occasion, to do more than briefly refer to some of the more salient features of his scientific career, extending as it does over nearly half a century of unceasing intellectual activity; and I need hardly say that in attempting to give some idea of important labours which lie outside my own studies, I am dependent on the kindness of scientific friends.

As a traveller, he can perhaps only compare with Humboldt in the extent to which he has used travel as an instrument of research. To quote a remark by Prof. Asa Gray, "No botanist of the present century, perhaps of any time, has seen more of the earth's vegetation under natural conditions." His Antarctic voyage in 1839–43 supplied the material for a series of well-known works of first-rate importance on the vegetation of the southern hemisphere; and these in their turn formed the basis of important general discussions. The journey to India in 1847–51 yielded, in the Himalayan journals, as Humboldt has remarked, "a perfect treasure of important observations." The maps made of the passes into Tibet are even still unsuperseded. The fine work on the "Sikkim Rhododendrons" was at once a revelation to the botanist and to the horticulturist. His account of the glacial phenomena of the Himalayas supplied facts both to Darwin and to Lyell. A journey to Morocco in 1871 and a later visit to North America led to important conclusions on plant distribution.

Perhaps Sir Joseph Hooker's most important place in scientific history will be found in the rational basis upon which he placed geographical botany. De Candolle, while admitting the continuity of existing floras with those preceding them in time, still adhered in principle to the multiple origin of species. To quote a remark by Prof. Asa Gray, "De Candolle's great work closed one epoch in the history of the subject, and Hooker's name is the first that appears in the ensuing one." According to Lyell, "the abandonment of the old received doctrine of the 'immutability of species' was accelerated in England by the appearance in 1859 of Dr. Hooker's 'Essay on the Flora of Australia.'" This essay effected a revolution. It was quickly followed in 1860 by the classical essay on the "Distribution of Arctic Plants," and in 1886 by the Nottingham lecture on insular floras. The fact of widely *dissevered* localities for species, which De Candolle found an insuperable obstacle to abandoning the doctrine of multiple origin, has, in the hands of Hooker and A. Gray (as stated by Bentham), afforded the most convincing proof of the genetic relationship of the floras of which such species are components.

In systematic botany, Hooker has perhaps had no rival since Robert Brown. The "Genera Plantarum," the joint work of himself and his friend Bentham, and the "Flora Indica," to the completion of which our colleague is devoting the leisure of a well-earned retirement, form only as it were the head of an immense body of taxonomic memoirs.

Nor have his services to botanical science been confined to geographical botany and to taxonomy. His researches on various groups, such as *Welwitschia* and others, deal in a masterly way with morphological problems of the highest interest and of extreme difficulty.

While no one would attempt to minimize the commanding

and unique position of Mr. Darwin, the scientific historian of the future will recognize how much the development of the modern theory of evolution, from its first conception in the mind of Mr. Darwin, was facilitated by the interaction upon one another of the work and minds of Darwin, Hooker, and Lyell. It was due to the earnest efforts of his two friends that Mr. Darwin was induced to publish the first sketch of the origin of species at all. And no one, had he been alive, would have more cordially recognized than Mr. Darwin how vast an armoury of facts the wide botanical experience of Hooker constantly placed at his disposal in fortifying and supporting his main position.

Of the two Royal Medals, it is customary, though it is not an invariable rule, to award one for mathematics or physics, and the other for biological science.

The medal, which, in accordance with the usual rule, has been devoted to mathematics and physics, has this year been awarded to Colonel A. Clarke for his comparison of standards of length, and determination of the figure of the earth.

Colonel Clarke was for some twenty-five years the scientific and mathematical adviser for the Ordnance Survey, and whilst acting in that capacity he became known to the whole scientific world as possessing a unique knowledge and power in dealing with the complex questions which arise in the science of geodesy.

His laborious comparison of the standards of length, carried out under General Sir Henry James, R.E., are universally regarded as models of scientific precision.

His determination of the ellipticity and dimensions of the earth from the great arcs of meridian and longitude involved a very high mathematical ability and an enormous amount of labour. The conclusion at which he arrived removed an apparent discrepancy between the results of pendulum experiments and those derived from geodesy, and is generally accepted as the best approximation hitherto attained as to the figure of the earth.

The accounts of these investigations have been published in a number of memoirs, several of which have been communicated to the Royal Society.

In 1880 he published a book on geodesy, which, besides giving an accurate account of that science, embodies the main results of the work of his life.

In the biological division of the sciences the Royal Medal has this year been awarded to Prof. Henry N. Moseley for his numerous researches in animal morphology, and especially his investigations on Corals and on Peripatus.

The result of his elaborate investigations on Corals, an account of which has been published in the Philosophical Transactions, was to show that the Milleporidæ and the Stylasteridæ were not, as had been thought, Anthrozoan in nature, but were composite coral-forming hydroids. Many new genera and species were described by him in these memoirs, and in fact a new group of organisms, the Hydrocorallinæ, was not merely indicated, but the complete morphology and systematic subdivisions of that order were worked out.

Moseley's memoir on Peripatus is not less remarkable. He was the first to point out the true nature of this remarkable animal, and to demonstrate that it was in reality an archaic Arthropod. The subsequent investigations of Balfour and Sedgwick have further increased the importance of Moseley's discovery.

Moseley's memoir on the Land Planarians of Ceylon (Phil. Trans., 1872) is an important contribution to the anatomy of the Turbellaria. He was the first to apply the method of section-cutting to the Planarians, and his paper is full of new facts of great importance, which have stood the test of subsequent work over the same ground.

Besides these three great memoirs published in the Philosophical Transactions, Moseley has published numerous minor discoveries, and his spectroscopic observations on the colouring matters of marine organisms have proved the starting-point of valuable investigations.

Mention must not be omitted of Moseley's admirable book, "Notes of a Naturalist on the *Challenger*," which has been justly compared, for the varied ability, interest, and activity which it evinces on the part of the author, to Darwin's "Voyage of the *Beagle*."

Since the date of the works above referred to, Moseley has been chiefly active in the discharge of his duties as Linacre Professor, and the success with which he has directed the work of his pupils is evinced by the important memoirs on zoological subjects which several of them have produced whilst working under his direction. He has himself also published a remarkable discovery with regard to the Chitons. In the shells of many genera and species of these mollusks he has detected highly developed eyes, of which he has described the minute structure.

The Davy Medal for the year 1882 was awarded by the Council to Profs. Mendelejeff and Lothar Meyer conjointly, for their discovery of the periodic relations of the atomic weights. This relation, now known as "the Periodic Law," has attracted great attention on the part of chemists, and has even enabled Prof. Mendelejeff to predict the properties of elements at the time unknown, but since discovered, such as gallium for instance.

But while recognizing the merits of chemists of other nations, we are not to forget our own countrymen; and accordingly the Davy Medal for the present year has been awarded to Mr. John A. R. Newlands, for his discovery of the Periodic Law of the chemical elements. Though, in the somewhat less complete form in which the law was enunciated by him, it did not at the time attract the attention of chemists, still, in so far as the work of the foreign chemists above mentioned was anticipated, the priority belongs to Mr. Newlands.

SCIENTIFIC SERIALS.

Rivista Scientifico-Industriale, October.—On the crepuscular phenomena of 1883–84, by Prof. Annibale Riccò. These remarks are made in connection with the author's comprehensive work, now nearly ready for the press, on the remarkable after-glows of the years 1883–84. One of the chief conclusions arrived at in this work, after a careful consideration of all the evidence, is that the volcanic theory, first advanced by Mr. Norman Lockyer, is the only one that can be now accepted. The light-effects appeared soon after the great eruption of Krakatão on August 27, 1883, were propagated from the neighbourhood of the volcano to the most distant parts, and then gradually died out, precisely in the same way that similar manifestations were made immediately after the eruption of the island of Ferdinandea (Julia) in 1831. It is further concluded that the after-glows were due, not to the ashes or scoriæ ejected by Krakatão, but to the condensation of the aqueous vapours caused by the volcano, which condensation increased the quantity of solar light reflected by the atmosphere.

Bulletin de l'Académie Royale de Belgique, October.—On the mass of the planet Saturn, by L. de Ball. By a comparative study of its satellites, made at the Observatory of Cointe during the winter of 1885–86, the author finds the mass of Saturn to be 1/3492·8 that of the sun, which is rather less than the values obtained by Meyer, Hall, and Struve, which are 1/3482·5, 1/3481·3 and 1/3490·8 respectively.—Experimental researches on the sense of vision in the Arthropods, by Felix Plateau. Of this elaborate memoir the first part only appears in this issue, dealing first with the work already accomplished down to the year 1887 on the structure and functions of simple eyes; secondly, with the eyes of Myriapods. The four remaining parts, to be published in subsequent numbers of the *Bulletin*, will treat of vision in the spiders, and in larvæ generally; of the part played by the frontal eyes in perfect insects; of compound eyes and the perception of movements; with an anatomico-physiological summary, and experiments with insects.—Remarks on the total solar eclipse of August 19, 1887, by L. Niesten. A comparative study of the photographs obtained by MM. Niesten and Karelin at the station of Jurjewetz, shows that with Van Monckhoven's sensitive plates an almost instantaneous image is obtained not only of the protuberances but also of the corona; and further that a pose of thirty seconds gives no more detailed images of the corona than those obtained at the end of eight seconds. Hence it would appear that photographs of the corona obtained after an exposure of over a minute should be attributed to physical phenomena due to the atmospheric conditions, or to light-effects produced in the photographic apparatus itself.

SOCIETIES AND ACADEMIES.

LONDON.

Linnean Society, November 3.—W. Carruthers, F.R.S., President, in the chair.—Mr. J. H. Hart, of Trinidad, was elected a Fellow of the Society.—The President called attention

to the death-roll since last June meeting, specially deploring the loss of Prof. Julius von Haast, N.Z., Dr. Spencer Baird, U.S., and Prof. Caspary, of Königsberg.—Mr. H. N. Ridley gave an account of his natural history collection in Fernando Noronha. The group of islands in question is in the South Atlantic, 194 miles east of Cape San Roque. The largest is about five miles long and two miles across at broadest part. Although chiefly basaltic, phonolite rocks crop up here and there. The indigenous fauna and flora seem to have been much modified, and in some cases extirpated, by human agency. Of mammals, the cat is reported to have become feral, and rats and mice swarm; Cetacea occasionally frequent the coast. The land-birds comprise a dove, a tyrant, and a greenlet (*Virio*). Sea-birds are numerous, though apparently less so than in the time of the early voyagers. Among reptiles occurs an Amphisbæna, a Skink, and a Gecko; turtles also haunt the bays. The absence of batrachians and fresh-water fish is noteworthy. A well-known Brazilian species of butterfly is plentiful. Though insects generally are abundant, there are, notwithstanding, but few species. Two shells (*Trochus*) show a southern distribution, though other marine forms indicate West Indian relationship. Several interesting plants were got, a *Solanum* with medicinal properties, a new *Erythrinm*, and flower of the "Burra," a Euphorbiaceous tree. Of ferns, mosses and hepatics, lichens and fungi, several interesting sorts were collected.—Mr. Geo. Murray exhibited *Vallonia ovalis* from Bermuda and Grenada; the former sort consisting of a balloon-shaped cell an inch long and two wide. He explained by diagrams the development of *V. utricularis*, incidentally comparing this with *Sciadium*.—Prof. Marshall Ward showed specimens and made remarks on the peculiar development of *Agaricus* (*Amillaria*) *melleus*.—Mr. E. A. Heath exhibited examples of fruits of two species of *Solanum* from Barbados.—A paper was read on the scars occurring on the stem of *Dammara robusta*, by Mr. S. G. Shattock. He says that the process of disarticulation of the branches is like that by which a leaf or other organ is shed. The parenchymatous cells across the whole zone of articulation multiply by transverse division, a layer of cork resulting from the formation of this secondary meristem, and through the distal limits of this, solution of continuity occurs. After this the slender connecting bond of wood is broken across by the weight of the branch or the first trivial violence; this completion of the process being aided, perhaps, by the tension made upon the wood in consequence of the cell-division of the surrounding parenchyma which occurs across its axis. It thus happens that the whole of the parenchymatous system of the stem is closed by cork *before* the branch is actually shed.—A communication followed, by Messrs. J. G. Baker and C. B. Clarke, on the Ferns of Northern India; it being a supplement to a memoir published in the Society's Transactions.

Physical Society, November 12.—Prof. W. E. Ayrton, F.R.S., Vice-President, in the chair.—Lieut. Bacon, R.N., was elected a member of the Society.—Owing to the illness of Dr. Shettle, the paper announced to be read by him was postponed. —The following communication was read:—On a geometrical method of determining the conditions of maximum efficiency in the transmission of power by alternating currents, by Mr. T. H. Blakesley. In this paper the author confines himself to the consideration of a simple circuit containing generating, conveying, and recipient parts, in which the E.M.F. follows the law of sines. The maximum E.M.F.'s of both machines are supposed known, together with the resistance and coefficient of self-induction of the complete circuit. The variable on which the efficiency of transmission depends is the difference of phase of generator and receiver. A geometrical construction is given by which the phase which gives maximum efficiency can be determined. Mr. Kapp thought the construction would not apply where the receiver does mechanical work, owing to the E.M.F. not being a true sine function of the time. He also mentioned an experiment performed on a motor driven successively by alternating and direct currents, in which the *apparent* power $\left(\sqrt{\bar{e}^2}\sqrt{\bar{c}^2}\right)$ supplied by alternating currents was about five times that required when direct currents were used, the motor giving out the same power in the two cases. From this he inferred that the ratio of power to weight is much greater for a direct than for an alternating current motor. This he considered a serious drawback to the use of alternate currents for transmitting power. After some remarks by Prof. Ayrton and Prof. S. P. Thompson, Mr. Blakesley said that by placing a condenser between the terminals of the recipient machine a greater current could be passed through the receiver than that in the generator and line.—Prof. A. W. Rücker exhibited and described a lecture experiment for determining the velocity of sound. The principle of the arrangement is that used by Fizeau in determining the velocity of light. A vibrating reed is used as the source of sound and a sensitive flame as receiver. A long U-shaped tube has its two ends placed near and parallel to the plane of a perforated disk, which is capable of rotating about an axis perpendicular to its own plane. The reed and sensitive flame occupy similar positions on the opposite side of the disk. On rotating the disk, the sensitive flame flares or is quiescent according as the time taken to travel the length of the tube is an even or an odd multiple of $\frac{T}{2n}$, where T is the time of one revolution and n the number of holes in the disk.—Mr. Bosanquet exhibited a form of polariscope he had made some time ago for researches on the polarization of the sky. Its chief feature is a compound prism of right- and left-handed quartz which shows coloured bands with polarized light, whatever be the direction of the plane of polarization. It also forms a very sensitive object for polarimeters.

Zoological Society, November 15.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the months of June, July, August, September, and October, 1887, and called attention to certain interesting accessions which had been received during that period.—A communication was read from Herr W. von Nathusius, of Königsborn, on *Symbiotes equi*, a parasite of the horse, causing what is called "greesy-foot," of which he sent specimens for exhibition.—The Secretary read a letter addressed to him by Dr. Emin Pacha, dated Wadelai, April 15, 1887, referring to some communications which he was proposing to offer to the Society. —A letter was read from Surgeon-General George Bidie, referring to a case of the breeding of the Elephant in captivity.—Prof. Bell made some observations on the "British Marine Area," as proposed to be defined by the Committee of the British Association. Prof. Bell opposed the idea of omitting the Channel Islands from the British area.—Prof. A. Newton, F.R.S., exhibited (on behalf of Mr. W. Eagle Clarke) a specimen of Bulwer's Petrel (*Bulweria columbina*), believed to have been picked up dead in Yorkshire.—Mr. H. E. Dresser exhibited (on behalf of Lord Lilford) specimens of a new species of Titmouse allied to the Marsh-Tit (*Parus ater*), obtained by Dr. Guillemard in Cyprus, which he proposed to designate *Parus cypriotes*.—Mr. Boulenger exhibited a living specimen of a rare African Batrachian (*Xenopus lævis*), which had been sent to him by Mr. Leslie, of Port Elizabeth.—Prof. Flower exhibited a photograph of a specimen of Rudolphi's Whale (*Balænoptra borealis*), taken in October last, in the Thames near Tilbury.—Mr. G. A. Boulenger, read an account of the Reptiles and Batrachians collected by Mr. H. H. Johnston on the Rio del Rey, West Africa. Amongst these were examples of two species of Batrachians new to science. —Mr. Edgar A. Smith read some notes on three species of shells obtained by Mr. H. H. Johnston at the Rio del Rey, Cameroons. —Mr. A. G. Butler read a paper containing an account of two small collections of African Lepidoptera obtained by Mr. H. H. Johnston at the Cameroons and the Rio del Rey.—A communication was read from Mr. G. E. Dobson, F.R.S., on the genus *Myosorex*. The paper contained the description of a new species from the Rio del Rey (Cameroons) district, which he proposed to call *Myosorex johnstoni*, after Mr. H. H. Johnston, who had sent home the specimens.—Mr. G. A. Boulenger gave the description of a new species of *Hyla* from Port Hamilton, Corea, living in the Society's Gardens, which he proposed to name *Hyla stepheni*, after its discoverer.

Institution of Civil Engineers, November 8.—Mr. G. B. Bruce, the new President, after presenting the medals and premiums announced at the annual meeting in May last, delivered his address on assuming the chair for the first time. Having entered upon his apprenticeship in the locomotive works of Robert Stephenson within a few months of the beginning of the present reign, the President chose the state of engineering then and in the Queen's Jubilee year as the subject of his remarks. Starting with the workshop, in 1837 machine-tools were practically unknown, reliance being placed upon the skill of the workmen, who could chip and file by hand almost as truly as the machine. It was scarcely credible, but it was a fact, that there

was not a single crane in Robert Stephenson's shops in 1837; and the only steam-engine, in that which was the most important locomotive shop in the world of that day, was a vibrating pillar-engine, with a single 16-inch cylinder and 3-feet stroke. About the only machine-tool, properly so called, in the works was a planing-machine, which probably weighed about 3 tons. At the present time there were lathes 75 feet long, weighing 100 tons, giving a yield of steel-turnings at the rate of 10 and 20 tons a day, and planing-machines weighing 90 tons and operating over surfaces of 20 feet by 15 feet. Having spoken of the changes in the position of the workmen, the President referred to the progress of railways, the development of the iron and steel industries, and sanitary engineering. Reference was made to the electric telegraph, which had developed from the 5-needle instrument of Cooke and Wheatstone, employing six wires and working at about the rate of four words a minute, to the system of multiplex and automatic telegraphy, by means of which six messages could be sent at once on one wire with a speed of, say, 600 words per minute. Touching successively on the telephone, electric light, and the application of electricity as a motive power, the President hazarded the opinion that when some way should have been discovered of storing up in a more efficient and financially successful manner the unemployed forces of Nature, such as the winds and tides, then would electricity become a factor in the world's life compared with which it was at present as nothing.

Anthropological Institute, November 22.—Prof. Flower, C.B., Vice-President, in the chair.—Canon Isaac Taylor read a paper on "The Primitive Seat of the Aryans," in which he urged the view that the Finns are the nearest representatives of the ancient Aryan stock, and that the race took its origin in North Germany.

Edinburgh.

Royal Physical Society, November 16.—Prof. Duns delivered the introductory address for the session 1887–88. At the outset obituary notices of several deceased Fellows were given, notably of Mr. Robert Gray, the late Secretary of the Society. After some remarks upon the history and progress of the Society, he passed on to consider the claims of Scotland upon Government aid for scientific purposes, and advocated the union of the various scientific corporations of Edinburgh to form an Academy of Science for dealing with general questions of this nature.

Paris.

Academy of Sciences, November 21.—M. Janssen in the chair.—On the nervous system of the Gasteropods (Aplysia type, *A. depilans* and *A. fasciata*), by M. H. de Lacaze-Duthiers. The Aplysia, a large mollusk, abounding especially in the Mediterranean seaports, is here studied for the purpose of determining the type of its nervous system in order to compare it with those of Gadinia, Testacella, and other Gasteropods already described by the author.—Remarks in connection with M. Colladon's recent note on waterspouts and tornadoes, by M. H. Faye. It is again shown that M. Colladon's illustration, as published in the *Comptes rendus*, has only a very remote connection with true waterspouts and whirlwinds. Reference is also made to the statement, in W. Ferrel's new work on meteorology, that much sea-water is carried up by the ascending current of waterspouts, the fish and other animals in small ponds being even in this way borne aloft and wafted to great distances. On the contrary, M. Faye insists with Lieutenant Finley, of the United States Signal Service, that no appreciable quantity of water is pumped up in this way, although much is driven horizontally to the right and left by the gyratory velocity of the air, which has always a descending, and never an ascending motion.—On the crystalline form of cinchonamine, by M. C. Friedel. Some crystals of the alkaloid discovered by Arnaud in certain varieties of quinquinas are described as hexagonal prisms terminating in a rhombohedron and of the true orthorhombic type.—On a meteorite which fell on August 18/30, 1887, at Taborg, in the Government of Perm, Russia, by M. Daubrée. This meteorite, which has but slight cohesion, with density 3·620, appears to closely resemble those which fell on April 1, 1857, at Heredia (Costa Rica); on May 14, 1861, at Canellas, Province of Barcelona (Spain); on January 19, 1867, at Khethree, Rajputana (India); and on August 17, 1875, at Feid Shair (Algeria).—On a simple relation between the wave-lengths of spectra, by M. A. E. Nordenskjöld. The researches here described tend to confirm the author's previous view that, at least in the spectra of certain simple bodies, the differences between the logarithms of the wave-lengths of each element are simple multiples of the same number. The universality of this law, as applicable to the spectra of all bodies, is still far from being established. But further investigation will probably show, either that the spectra of all simple bodies conform absolutely to this law, or else that they are disposed in more or less independent groups, to which the law may still be applicable.—On the volcanoes of Hawaii, by Mr. James Dana. Reserving for the *American Journal of Science* a detailed account of a recent visit to these volcanoes, the author here remarks chiefly on the remarkable fluidity of the lavas, and on the fact that the eruptions show no sign of being in any way associated with the surrounding marine waters. The salts deposited in the hottest recesses, and those of solfataras, do not appear to have hitherto yielded any chloride, while the sulphate of soda is very common.—Researches on meteorites: general conclusions, by Mr. J. Norman Lockyer.—Observations of Olbers' comet (1815 I.), at its return in 1887, made with the 0.38 m. equatorial of the Bordeaux Observatory, by MM. G. Rayet and F. Courty. The observations cover the period from September 8 to September 25.—On sidereal evolution, by M. Stanislas Meunier.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Die Welt in Ihren Spiegelungen unter dem Wandel des Völkergedankens. Prolegomena zu Einer Gedankenstatistik; Ethnologisches Bilderbuch mit Erklärendem Text: A. Bastian (Mittler, Berlin).—Sound, Light, and Heat: M. R. Wright (Longmans).—A Primary Geometry: S. E. Warren (Trübner).—Quantitative Chemical Analysis: Classen and Herrick (Trübner).—Myth, Ritual, and Religion: A. Lang (Longmans).—Translations of Foreign Biological Memoirs, I. Memoirs on the Physiology of Nerve, of Muscle, and of the Electrical Organ, edited by J. Burdon-Sanderson (Clarendon Press).—Earth Knowledge: Harrison and Wakefield (Blackie).—Colour: Prof. A. H. Church (Cassell).—Elementary Microscopical Manipulation: T. C. White (Roper and Drowley).—Quarterly Journal of Microscopical Science, November (Churchill).—Annales de la Faculté des Sciences de Toulouse, tome i., 1887, 4 parts (Gauthier-Villars, Paris).

CONTENTS.

THURSDAY, DECEMBER 8, 1887.

TECHNICAL EDUCATION IN MANCHESTER.

PROFESSOR HUXLEY could scarcely have anticipated the ready response Manchester has given to the challenge he threw down at the close of his most able address at the Town Hall on the 29th ult. In speaking of one of the great problems of the day, that of meeting ever-increasing competition and yet maintaining the proper social condition of the workers, he said:—"I have ventured to put this before you in a bare and almost cynical fashion because it will justify the strong appeal which I make to all concerned in this work of promoting industrial education to have a care at the same time that the conditions of industrial life remain those in which the physical energies of the population may be maintained at a proper level, in which their moral state may be cared for, in which there may be some days of hope and pleasure in their lives, and in which the sole prospect of a life of labour may not be an old age of penury. . . . I therefore confidently appeal to you to let those impulses have full sway, and not to rest until you have done something better and greater than has yet been done in this country in the direction in which we are now going."

Only a few hours before the utterance of these words the trustees of the late Sir Joseph Whitworth—who during his life-time did so much to encourage and promote the higher education of working engineers—made a munificent offer to the city of Manchester; an offer which was only made public by Mr. Darbishire after Prof. Huxley had finished his address, and which goes some way towards realizing what Prof. Huxley remarks may by some be looked upon as the Utopian dream of a student. The gift consists of a plot of land of about twenty-five acres in one of the best situations in the city, which the trustees have purchased for the sum of £47,000. They propose to offer the whole of this to the Corporation of Manchester upon trust, two-thirds to be maintained as a public park, and one-third as a site for the following institutions: (1) an appropriate Institute of Art, with galleries for paintings, for sculpture and moulded form, and for architectural illustration; (2) a comprehensive Museum of Commercial Materials and Products; (3) a Technical School on a complete scientific and practical scale. The necessary buildings are to be raised by the Corporation and by public-spirited inhabitants of the great district which owns Manchester as its metropolis; and the Whitworth Trustees add that, if this work be heartily undertaken, their own further contributions may be looked for.

Such a result of the movement for the Extension of Technical Education and for the higher culture of our toiling thousands may indeed be welcomed, and the National Association, under whose auspices this meeting was held, may well be congratulated on this outcome of its autumn work. But this is not all, for it is not unlikely that the surplus, amounting, it is believed, to about £50,000, now placed at the disposal of the guarantors of the Jubilee Exhibition, may be applied to furthering this enterprise. Manchester has thus before it the prospect of showing England what can be done to promote educational progress in this direction, and to inaugurate a movement which ought to be followed by all the great cities in the country. Can we doubt that the sons of those whose energy and clear-sightedness have in times past placed Manchester in the van of the most important social movements of the day will prove themselves equal to the task which they have now a splendid opportunity of accomplishing? It is by caring thus for the well-being of our workers that the stable condition of society, referred to by Prof. Huxley, can be best secured; for truer words were never spoken than those in which he stated his belief that, in order to succeed in the competition which is every day becoming more keen, it is not sufficient that our people shall have the knowledge and the skill which are required, but that they must also have the will and energy and the honesty without which neither knowledge nor skill can be of any permanent avail. Mere technical instruction in handicraft or science must, in short, be based on a sound preliminary education. We need to train our workers to be not only clever artisans, but honest men who take pride in the quality no less than in the quantity of their work. It is because these were the views upheld by Sir Joseph Whitworth, and acted upon by him during his lifetime, that his trustees have felt that in no better way could they carry out the important ends for which he laboured than by starting a movement having for its object not merely the technical training of the artisan, but his moral, intellectual, and physical advancement. H. E. R.

TRIDIMENSIONAL FORMULÆ IN ORGANIC CHEMISTRY.

Dix Années dans l'Histoire d'une Théorie. Deuxième Édition de "La Chimie dans L'Espace." Par J. H. Van 't Hoff. (Rotterdam: P. M. Bazendijk, 1887.)

THIS interesting monograph gives an account, partly historical, partly expository, of what in our opinion is the most important theoretical contribution towards solving the problem of the constitution of organic compounds that has been made since the idea of a definite union of atoms within the molecule was first introduced into chemistry, of which idea, indeed, the new theory is an expansion. The work has the advantage of being written by one of the originators of the theory.

The linked-atom formulæ, which have so powerfully aided the development of organic chemistry, never professed to give any information as to the relative positions of the atoms in space. All that the "links" or "bonds" denoted was the existence of a closer relation of attraction (of a kind not further specified) between atoms represented as directly linked than between atoms represented as not directly linked. The question of the actual position of these atoms was left entirely open. If, therefore, anyone gathered from the graphic formula of methane,

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   H
   |
H—C—H,
   |
   H
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for example, that the five atoms of this compound were necessarily situated in one plane, that person was merely

permitting the geometrical properties of paper (or black-boards) to influence his conceptions unduly.

In the great majority of cases the ordinary graphic formulæ fulfilled the purpose for which they were primarily devised: they enabled chemists to predict the number and constitution of the isomerides possible for any given combination of atoms. But there were cases, on the other hand, in which the number of isomerides discovered by experiment exceeded that predicted by the theory. This was especially noticeable in the case of those compounds which in the liquid form or in solution produce rotation of the plane of polarized light. Thus, of three compounds to which chemists, from a study of their modes of formation and decomposition, were obliged to ascribe identical atomic linkage, one would be found to cause rotation of the polarized ray to the right, another to the left, whilst a third was optically inactive. As this optical difference was frequently the only difference discoverable, the phenomenon was described as "physical" isomerism—a name which appeared to suggest that the investigation of it lay outside the province of chemistry. Wislicenus, however, in 1873, in discussing an isomerism of the foregoing type occurring in the case of fermentation-lactic acid and paralactic acid, suggested that this was really a "geometrical" isomerism; that, although the mode of linkage of the atoms was undoubtedly the same in the two compounds, the positions of the atoms in space were different.

The demand thus made for a system of tridimensional formulæ was speedily responded to. In the following year, Van 't Hoff, in Holland, and Le Bel, in France, independently, and almost simultaneously,[1] suggested a very simple hypothesis with regard to the distribution of the four affinities of the atom of carbon. From this hypothesis they developed a system of formulæ under which not only the old anomalies of isomerism disappeared, but new lines of experiment in the preparation of unknown isomerides were indicated.

Van 't Hoff and Le Bel called attention to the hitherto unnoticed fact that all organic compounds which in the liquid state or in solution exhibit optical activity, contain in their constitutional formulæ at least one carbon atom, the four affinities of which are satisfied by four *different* atoms or groups. Such a carbon atom they termed, for reasons to be explained presently, "asymmetric" (Van 't Hoff), or "dissymmetric" (Le Bel).

With regard to the distribution of the affinities of an atom of carbon, Van 't Hoff and Le Bel made the assumption that the four monad atoms or groups satisfying the four affinities of such a tetrad atom are situated at the solid angles of a tetrahedron, the centre of which is occupied by the carbon atom. If, now, the four monad atoms or groups are dissimilar, as in the case of optically active compounds, it is possible to arrange them about the angles of the tetrahedron in two different ways, so as to produce two *asymmetric* tetrahedra (considered with regard to the positions of these atoms or groups)—two non-superposable tetrahedra, one of which is the mirror-image of the other. A continuous curve, passing through the four atoms or groups in the same order, will in the one case describe a right-handed, in the other a left-handed, screw-line. Two compounds thus differing in atomic structure only as regards conditions of symmetry might be expected to possess the same chemical and physical properties, save where dissymmetry or polarity is concerned. As a fact, this is found to be the case with optically active compounds. When a compound contains an asymmetric carbon atom, this compound, provided that it has been adequately investigated, can always be shown to exist in two modifications, possessing the same chemical properties and displaying the same chemical reactions, and, as regards physical properties, agreeing in melting-point, boiling-point, solubility, specific gravity, and all other properties not involving the operation of polar forces. But let dissymmetry or polarity in any form intervene, and the non-identity of the two compounds is at once manifested. Thus, as regards the action of the compounds upon polarized light, the one compound turns the polarized ray through a given angle to the right, the other through the same angle to the left. Again, if the two compounds crystallize, although they do so in forms belonging to the same system and having the same angles, yet the crystals exhibit hemihedral faces which are situated to the right in the one case, and in the other to the left. The one crystal is thus the mirror-image of the other—a relation corresponding with that which is supposed to prevail between the asymmetric carbon atoms themselves within the molecule. These two hemihedral crystals also display opposite pyro-electricity. Even the otherwise identical chemical action of the two compounds may be modified by the dissymmetry of a third compound with which they combine; thus, for example, a dextro-rotatory and a lævo-rotatory acid differ in the degree of their affinity for a dextro-rotatory base, and the two resulting salts are generally quite distinct in their properties.

In all artificial syntheses of compounds containing an asymmetric carbon atom the substance obtained is optically inactive. This was to be expected. The chances in favour of the formation of each of the two modifications of opposite rotatory power are equal: both are therefore formed in equal quantity; and the resulting mixture is inactive. There are three known methods of separating the optically active constituents of such a mixture. These methods, due to M. Pasteur, were discovered, it should be mentioned, many years ago, before the Van 't Hoff-Le Bel hypothesis was put forward. The separation is effected: (1) by the greater ease with which one of the two modifications is attacked by some particular micro-organism, it being thus possible to destroy the whole of one modification leaving the other almost intact, and by properly selecting the organism even to destroy at will either the dextro-rotatory or the lævo-rotatory modification; (2) by the different degree of affinity which the two modifications exhibit towards some other optically active compound; and (3) by means of the factthat under certain conditions of temperature and concentration it is sometimes possible to separate the two modifications by ordinary crystallization. By these means Pasteur succeeded in breaking up racemic acid into dextro-tartaric and lævo-tartaric acids.

[1] Van 't Hoff's views were first published in a pamphlet in the Dutch language, in September 1874. Le Bel's original memoir appeared in the *Bulletin de la Société Chimique*, in November of the same year. In May 1875, Van 't Hoff published his pamphlet, "La Chimie dans l'Espace." which, however, did not attract much notice until the appearance of the German translation by Herrmann in 1877.

Optical inactivity may also be due to mutual compensation between two asymmetric carbon atoms, of equal and opposite asymmetry, within the molecule itself. This is the case with inactive tartaric acid.

At the present moment there is no case known which contradicts the foregoing hypothesis. A few substances, which, at the time when the hypothesis was first put forward, were believed to be optically active, and yet contained in their molecule no asymmetric carbon atom, have since been shown to owe their supposed optical activity to impurities. On the other hand, the presence of an asymmetric carbon atom in the formula of an apparently inactive compound has been an indication to chemists that the resolution of the compound into two isomerides of opposite optical activity might be profitably attempted; and in the long list of such attempts that have been made within the last few years there appears to be no record of failure.

We have already alluded to M. Pasteur's classical researches on the tartaric acids, in which he not only rendered the Van 't Hoff-Le Bel hypothesis possible by elucidating all the various modes of optical activity and inactivity which it contemplates, but also devised the methods which have so facilitated its experimental development. It only remains to show how near this great investigator came to anticipating the entire hypothesis. In a passage written in 1860, quoted by Prof. Van 't Hoff, M. Pasteur says, referring to the tartaric acids:—

"Les atomes de l'acide droit sont-ils groupés suivant les spires d'un hélice dextrorsum, ou *placés aux sommets d'un tétraèdre irrégulier*, ou disposés suivant tel ou tel assemblage dissymétrique déterminé? Nous ne saurions répondre à ces questions. Mais ce qui ne peut être l'objet d'un doute, c'est qu'il y a groupement des atomes suivant un ordre dissymétrique à l'image non-superposable. Ce qui n'est pas moins certain, c'est que les atomes de l'acide gauche réalisent précisément le groupement dissymétrique inverse de celui-ci."

This is divination indeed!]

We must content ourselves with merely referring to another portion of the subject—the application of the carbon tetrahedron to the explanation of anomalous cases of isomerism occurring among unsaturated compounds; of which *allo-isomerism*, as Prof. Michael has termed it, fumaric and maleic acids may be taken as illustrations. This application, first made by Van 't Hoff in 1874, and accepted later by Le Bel, has undergone within the past year an extension of extraordinary importance at the hands of Prof. Wislicenus in his elaborate memoir "Ueber die räumliche Anordnung der Atome in organischen Molekulen und ihre Bestimmung in geometrisch-isomeren ungesättigten Verbindungen" (*Abhandl. der Königl. Sächs. Gesellsch.*, 1887), of which a very full and appreciative summary is given by Prof. Van 't Hoff in the present work.

The tridimensional formulæ of organic chemistry are thus an accomplished fact. The treatment of the subject is still, of course, only statical; but, taking care not to lose sight of the limitations thus imposed, the method is a perfectly legitimate one.

F. R. JAPP.

THE MAMMOTH AND THE FLOOD.

The Mammoth and the Flood: an Attempt to Confront the Theory of Uniformity with the Facts of Recent Geology. By Henry H. Howorth, M.P., F.S.A. (London: Sampson Low and Co., 1887.)

MR. HOWORTH'S book is not disproportionate to its subject. But even as the mammoth it had a small beginning. It saw light as letters in NATURE. It cast its swaddling-clothes at the British Association. Grown larger, it took passage on board the *Geological Magazine*, and, as some thought, threatened to swamp that useful but far from bulky periodical. Now, with body and tusks alike full-grown, it comes forth to champion cataclysm and scatter the uniformitarians.

The book consists partly of facts, partly of theories. The one part is separable from the other, though of course sometimes the facts are regarded in the light of the theories. We will endeavour in our notice to keep them apart. The first chapter of the work a little reminds us of the *hors d'œuvre* which sometimes precedes a banquet. Appetizing bits, dainty but miscellaneous—the etymology of mammoth, and its identity with behemoth; griffons and their claws; fossil unicorns; dragons' bones; Indian fabulous beasts; stories of giants, and their bones: with such subjects is the reader's palate stimulated. The next chapter gives a history of opinion on the subject of the remains of the mammoth and the woolly rhinoceros. The author then discusses the abode and range of the mammoth in Asia. He considers it to have been limited to the tundras, which must at that time have enjoyed a climate far more temperate than at the present. Then comes an account of the various discoveries of carcasses, either of the mammoth or of the woolly rhinoceros, in Siberia; followed by the history of the same animals and their associates in Europe. The climate of Europe, when frequented by them, is next discussed, and the facts bearing on the extinction of the mammoth are enumerated, particular stress being laid on the evidence of caves and fissures. Palæolithic man is next called into the witness-box, and cross-questioned as to the cause of his disappearance. That he was exterminated by Neolithic invaders does not, to the author, seem a satisfactory theory. That he was a victim of the Deluge is a simple explanation. The Old World is now quitted for the New—the two Americas are examined. In each, at no distant time, huge mammals flourished; their remains are found under circumstances not materially different from those of similar quadrupeds in the Old World. So they also must have perished in like manner: the Deluge was not limited to Siberia nor to the Old World; it swept alike over tundra and morass, over prairie and pampa; it inundated the New. Of course the West Indies could not escape; apparently no corner of the earth eluded the devastating waves, for Australia, Tasmania, and New Zealand tell the like tale of extinguished life, and sudden devastation. Lastly, there is the citation of historical evidence, in the form of brief summaries of the many variations of the widespread tradition of a universal deluge.

The facts, as indicated by the above statement—which is only a concise summary of the table of contents—cannot wholly be disentangled from the theories, in the light of which they are viewed and in proof of which

they are ranged. Still, their value is independent of the theories: for the author has dealt with them in the spirit of an advocate, but of an honest advocate. If, indeed, Mr. Howorth can be accused of any forensic art, it is in this very pardonable respect—that the most is made of the opinions of geologists who have held views generally favourable to his own. Thus the unwary and but slightly scientific reader almost trembles before such a weight of authority, and is afraid to question an opinion favoured by so many lights of the heroic age of geology. But in citing authorities it must always be remembered that, unless it can be shown that all the important facts on which an induction is now founded were before them also, the value of their opinion is greatly affected, and it may even be comparatively small. Further, if satisfied on this point, we must inquire whether any, and, if so, what, alternative hypotheses had been presented to them. These preliminary considerations are often overlooked in quoting authorities, yet their importance cannot be disputed. The mind is greatly influenced by early impressions and by the hypotheses which it has accepted. In the multitude of facts we to some extent find what we seek, miss those of whose value we are ignorant, and without any conscious unfairness select those things which support the accepted view. Anyone who has had in the course of his life to reconsider and to modify an induction formerly maintained must be conscious that in this respect he has innocently erred. Probably, only a cantankerous-minded investigator wholly escapes this infirmity, and for him other snares are laid. Hence in this matter the testimony of even such men as Buckland, or Cuvier, or D'Archiac, is of small value, because not only has a vast store of new facts been acquired since their time, which have influenced or modified almost every branch of geology, but also because the widespread belief in a universal deluge and the virulent attacks made on geology by well-meaning but unthinking theologians had produced a natural readiness to welcome everything which seemed to harmonize with the Biblical narrative.

Mr. Howorth urges that a catastrophic occurrence is not excluded by a rational view of uniformitarianism—which position, we imagine, few would dispute in the abstract; but issue would often be joined as to which explanation were the more probable. He points out also that it is quite possible for a particular form of a tradition to be unhistorical, and yet for the tradition itself to have a true foundation, a remark which is certainly just, and which is sometimes forgotten. But, admitting these axioms, the asserted occurrence of any particular cataclysm is a question of evidence; and it is not enough for Mr. Howorth to show that his hypothesis explains some difficulties which exist in the other, unless he further prove that it is not only in accordance with a larger number of facts, but also does not create a new class of difficulties still more formidable.

Mr. Howorth's preface sounds no uncertain note, as the following extract will show:—

"The coral-insect (*sic*) raises the islands of the Pacific, and the fall of leaves in a tropical forest piles up deep black soil. These cases are no doubt cases of continuous change; but if we turn elsewhere we have to explain a very different state of things. The great gaping cliffs and sheer precipices of the Alps, the splintered pyramids of the Sierra Nevada, the cañons of Colorado, the huge dislocations of the strata, involving faults of hundreds of fathoms in extent, so near us as Durham. These have not the look of gradual changes."

We rub our eyes, and wonder whether the last fifty years have been all a dream. Here are dead and gone geological ideas in full vigour. We had thought that if there was one spot on earth in which catastrophe could not be invoked, where the uniformitarian could be in peace, it was the Colorado cañons; and we cannot help thinking that if Mr. Howorth were a member of the English Alpine Club he would by this time have convinced himself that, whatever signs of ruin the Alps may afford, there are none of any vast catastrophe. It is therefore evident that Mr. Howorth's method of interpretation differs from that of geologists in general, and this must throughout the book be borne in mind by the reader. But Mr. Howorth is always rather a special pleader, ingenious sometimes, but generally inconclusive. Granting that occasionally he contrives to give a smart rap to the irrational uniformitarian (for such a person does exist) and hits upon a defect in an hypothesis, he straightway goes on to propose a solution involving greater difficulties. In a brief notice it is impossible to deal with particular instances, but some general indications may be given The carcasses of mammoths are found embedded in ice in the north of Siberia. It is admitted that, from their state of preservation, they must have been frozen up very shortly after death, and have so remained ever since. There are no doubt considerable difficulties in attributing their transport to a river flood, as Mr. Howorth points out; nevertheless, when we remember the peculiarities of the Siberian rivers, and that in a cold region a carcass would be slow to decompose, for the flesh might freeze before it ceased to drift, these do not seem insuperable. Mr. Howorth, as an alternative, offers the hypothesis of a deluge, followed by a sudden change of temperature, but, apart from the difficulties attending the former part of this, by what physical or astronomical catastrophe does he account for the latter? Wisely, he makes no attempt to indicate this.

Again, in speaking of the contents of caves, Mr. Howorth constantly lays stress upon the indications of the action of running waters, and upon the absence of any such disturbing agent at the present time. But he forgets that even followers of Lyell would admit that at no very remote epoch the climate of England was different, the rainfall was heavier, the streams were all bigger, nay, that a cave itself is symptomatic of running water, which in most cases would gradually forsake its old course. The stream which made Clapham Cave still runs concealed, hard at hand, through the limestone rock, and not so long ago, after a downpour on Ingleborough, welled up into its ancient channel. We wonder whether Mr. Howorth has ever seen what the fall of 4 inches of rain in a single night—no unprecedented case—can do even in our English lowlands. Such a downfall would turn many a dry fissure, small as its drainage-area might be, into a running stream. Mr. Howorth, in combating uniformitarians, seems to overlook the variations and catastrophes on a small scale (compared with the bulk of the earth) which everyone who has sat at the feet of Lyell accepts as axiomatic.

It would have been more politic had Mr. Howorth contented himself with local deluges ; but no, his destroying waves must pass over the whole earth. What is to generate these destructive waves, what multiplication of a Krakatão catastrophe is needed, how many cubic miles of mountain summit must fall into the sea, or of ocean bed leap up into the air, he forbears to tell us. Here, after a laborious scrutiny of facts, the reader is refreshed by a use of the imagination.

We leave a host of minor difficulties unnoticed for want of space, such as the occurrence of erratic blocks in positions of unstable equilibrium, the relation of drifts, supposed cataclysmal by the author, to the valleys in which they occur, the escape of apterous birds like the moa and the dodo, and the like. We must part from the book by saying that it exhibits great industry in the collection of materials—so that it will long be valuable as a work of reference—with a curious want of mental perspective, and a misapplied ingenuity of reasoning.

NEW ZEALAND SCALE INSECTS.

An Account of New Zealand Scale Insects. By W. M. Maskell, F.R.M.S. (Wellington : Geo. Didsbury, 1887.)

THIS book shows that the valuable work which is being done in South Australia by Mr. Frazer Crawford, Inspector under the Vine-Protection Act, is being done on a still greater scale in New Zealand. It affords an example of the great service which may be rendered by plain and sound publications on the subject of injurious insect attacks in the colonies. In the mother country the works which have been prepared for the Agricultural Department of the Privy Council by Mr. Whitehead, and Miss E. A. Ormerod's constant valuable publications on economic entomology, show what can be achieved in this field.

It is eminently satisfactory to find such an important subject taken up in New Zealand by an observer so well known as Mr. Maskell. The work extends to 116 pages, and includes exhaustive information on the Coccididæ affecting the crops of the island. The life-history of the Coccididæ (which are divided by the author as follows: I. Diaspidinæ; II. Lecanidinæ; III. Hemicoccidinæ; IV. Coccidinæ) is given in all its stages, a whole chapter being devoted to it, with descriptions of the male and female perfect insects in detail, and another to the natural checks to their increase, and parasites, &c.

The remedies against Coccididæ are fully treated of in Chapter V. The author gives a list of washes, of which he says: "Some of the substances here given are manifestly unsuitable for general use on account of their expense, at any rate in the open air; yet it is well to include them, as they are all suggested in some work or other, or in the replies of gardeners and fruit-growers to Parliamentary inquiries, and the objections to them ought to be known."

Chapter VI. is devoted to "A Catalogue of Insects"—that is, of the Coccididæ—and "A Diagnosis of Species," and will be found to be of great service to all students of entomology. Particular attention is paid to the cottony-cushion scale, the *Icerya purchasi*, whose ravages in South Africa have been so ably treated of in the pamphlet lately published by the Consulting Entomologist of the Royal Agricultural Society of England, also by the State Inspector of the Fruit Pests of California, and more recently by Prof. Riley, the well-known Entomologist of the Department of Agriculture of the United States. "Tree-growers should especially beware of this insect, and the best plan to adopt would be to burn at once any tree found infested with it."

This chapter, which occupies almost two-thirds of the book, is succeeded by an index of plants and the Coccididæ attacking them, with the useful reminder that in hot-houses and green-houses all sorts of plants are liable to attack.

The work also contains twenty-three well-drawn plates, which convey a good idea of the Coccididæ to those who have not the opportunity of studying them. Plates I., II., and III. deal with anatomical points or structural details; Plates IV. to XX. give a large selection of insects, with specimens of the various trees and plants they infest; Plate XXI. is especially valuable as giving the male insects *Cœlostoma zœlandicum* and *C. wairoense*, the antenna of the former and the head of the latter being especially well marked. Plate XXII. gives the honey-dew and resulting fungi, and Plate XXIII. parasites of Coccididæ. "Fig. 1, *a*, pupa of Hymenopterous parasites; *b*, the same pupa under the waxy test of *Ctenochiton perforatus*; *c*, imago. Fig. 2, *a*, brown and yellow fungi on *Ctenochiton viridis*; *b*, upper side of brown fungus; *c*, under side of the same, with attached fungoid sheet; *d*, *Ctenochiton viridis* (test removed), filled with yellow fungus, and with globular mass of the same above it."

From the above brief sketch of the contents of Mr. Maskell's book it will be seen that it is a welcome addition to entomological literature. It is written in plain and forcible language, and there is no padding or beating about the bush for the reviewer to find fault with. There is an excellent tabulated explanation of terms used, and students will be much pleased with the author's classification, or rather division or arrangement, of the Coccididæ, based upon a plan most useful for economic entomology:—

"Neglecting entomological distinctions, we may divide the Coccididæ roughly into

"(*a*) Insects attacking deciduous plants;
"(*b*) Insects attacking evergreen plants;

or again:

"(*c*) Insects living usually on the bark;
"(*d*) Insects living usually on the leaves;
"(*e*) Insects living on both bark and leaves;

or lastly:

"(*f*) Insects covered with hard shields or 'scales';
"(*g*) Insects covered with cotton;
"(*h*) Insects naked."

Among other salient points the importance of destroying the eggs is frequently urged upon those who wish to extirpate coccids, and attention is wisely drawn to the fact that "it is a fallacy to imagine that rule-of-thumb methods, not founded upon any knowledge of the nature, habits, and life-history of the insects, are likely to be really efficacious."

We agree with the author that an increase in works on

economic entomology is always of good service in any country, and New Zealand may be congratulated on having Mr. Maskell at hand to supply a demand generated by the improved intelligence of the agricultural community.

OUR BOOK SHELF.

Pen and Pencil in Asia Minor; or, Notes from the Levant. By William Cochran. Illustrated with eighty-nine engravings, made chiefly from water-colour sketches by the Author. (London: Sampson Low and Co., 1887.)

THIS well-printed volume of over 450 pages is one of a class that we had thought had become extinct. The notes begin with the arrival of the author at the Alexandra Docks in Liverpool, and are continued almost daily, in some instances hourly, until the close of a five-months' tour through the Mediterranean to Smyrna, Constantinople, and then, with some slight journeys inland, back again by the same route to Liverpool.

No doubt the journey was pleasant, and we feel sure that the note-taking and the water-colour sketching were very agreeable occupations for the tourist; but probably even the author's friends would admit that as now laid before the world the text contains nothing either very novel or attractive, while of the many scenes sketched, omitting the sketches from photographs, we may say that it would be hardly fair to criticize them from an art point of view. The volume is not, however, without its merits. The author deserves credit for the earnest way in which he has called attention to the importance of encouraging the tea and silk industries, and we sincerely hope for the good of our colonies that his efforts in the direction of silk culture in Australia and New Zealand may eventually be as successful as tea-farming has been in Ceylon.

One chief object of the voyage to Smyrna was to see the result of Mr. John Griffitt's silk-farming in Asia Minor. At one time the silk industry was one of great importance in and about Smyrna, but owing to the silk-worm disease it became almost extinct, so that even the very mulberry-trees were used for firewood. Now, through the philanthropic zeal of Mr. Griffitts in supplying silkworm eggs not only free from disease but raised from carefully-selected varieties, the industry is being restored, and large numbers of mulberry-trees are being planted.

Several chapters in this volume are devoted to the subjects of the rearing of silkworms, and of the treatment of the mulberry-trees. From the hatching out of the larval forms to the reeling off of the silk, only some forty to forty-five days elapse, but though the labour be short, the care and attention required are very great, and the successful silk rearer learns various lessons of method and cleanliness which are of permanent value.

In chapter ix. we have a summary of Mr. Griffitt's valuable report on the silk trade, furnished to the Department of State, Washington. From it we learn that at one time in Smyrna there were three large silk-reeling factories, driven by steam, where hundreds of female hands were employed. When, on the failure of the indigenous worms, Japanese worms were introduced, it was found that it required double the number of cocoons to yield the same weight of silk. With Mr. Griffitt's improved native race of silkworms, the quality of the silk is better, and the produce much heavier than before. To those interested in silk culture we can recommend the perusal of this volume, which, indeed, would be better described as "Notes on Silk Culture in Smyrna."

A Catalogue of the Flora of Matheran and Mahableshwar. By the Hon. H. M. Birdwood, M.A., LL.M. With a Note by Dr. Theodore Cooke, LL.D., F.G.S. (1887.)

THIS little botanical work is a reprint from the Journal of the Bombay Natural History Society. It will be useful to persons visiting the localities botanized; and the records of the upper limits of various plants are interesting to botanists at a distance.

Mahableshwar is in the Ghauts, about a hundred miles south of Bombay, and the highest part of this healthy resort is nearly 5000 feet above sea-level, so that there are considerable changes in the vegetation in the ascent. The present catalogue contains the names of less than 500 species of plants, a number which future investigations will doubtless double. As the Bombay Natural History Society is still in its infancy, some singular slips in the classification of the plants are perhaps excusable; and we hope the members will not feel discouraged at our pointing out that ferns are not "plants with cellular tissue only," nor are mosses "leafless plants."

The Bombay Natural History Society possesses a herbarium of Mahableshwar plants, presented to it by Dr. Cooke, and it may be hoped that this will form the nucleus of a collection adequately representing the whole flora of the entire Presidency. Up to the present time the Bombay Government has shown but little interest in botanical work, and possesses none of the appliances for its prosecution to be found at Calcutta, Saharunpore, Madras, or Peradeniya. Yet for the Forest Department alone some kind of herbarium and botanical library is indispensable, unless its officers are to grope in the dark as to a large proportion of the plants they come across in their duties.

However, this is by the way. It is a sign of the development of a healthier interest when a hard-worked official like a judge of the High Court is found to take the lead in so creditable a way in the study of the local flora.

L'Homme avant l'Histoire. Par Ch. Debierre. (Paris: J. B. Baillière et Fils, 1888.)

IN this book M. Debierre gives a clear and interesting account of some of the results of anthropological research. In dealing with disputed points, however, he is apt to arrive at conclusions somewhat hastily. The doctrine of the unity of the human race he rejects, but he contents himself with a very slight and inadequate consideration of the arguments which may be advanced on the other side. Again, he assumes that there can be no doubt whatever as to the Asiatic origin of the Aryan or Indo-European race. That the original home of the Aryans was in Europe cannot be held to have been proved, but the theory has been accepted by so many investigators, and so much may be said in favour of it, that in a work of this nature it ought at least to have been explained and discussed.

Philips' Handy Volume Atlas of the British Empire, with Statistical Notes and Index. (London: Philip and Son, 1887.)

THIS little book is among the first British work of its sort that we have seen. It is extremely neatly put together and is well edited throughout. It contains 64 plates and on them 110 maps, showing the British possessions in the various parts of the globe. After each map is a short analysis of position, extent, population, climate, industries, government, orography and hydrography, &c., &c. In addition to the maps there are plans of various towns. Just before the index are given "Comparative Diagrams of the British Empire," comparing area, population, trade, and revenue of the British possessions of the different quarters of the globe. This is followed by the

index itself, with a list of abbreviations, consisting of twenty pages closely filled in with places in three columns. The colouring of the maps is excellent, and it is obvious that no attempt has been spared to make the book as complete as possible in every way. A. L.

The Young Collector's Hand-book of Ants, Bees, Dragon-Flies, Earwigs, Crickets, and Flies. By W. Harcourt Bath. (London: Swan Sonnenschein, 1888.)

ANY boy who may wish to form a collection of insects will find in this little hand-book all the information he will be likely to need at first for his guidance. The author does not pretend to go deeply into the subject, but he has brought together a sufficient number of facts to show beginners that the study of entomology will well reward any labour that may be devoted to it. His explanations are simple and clear, and the value of the manual is much increased by a large number of good illustrations.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

An Earthquake in England.

AS no account has been given in NATURE of a recent earthquake, perhaps room may be found for the following. I was standing near my garden door at 8.20 a.m. on Sunday, November 20, when the quiet was suddenly broken by a heavy smothered crash, followed by reverberations as in a clap of thunder of rather short duration. I felt no shaking of the ground, but many persons here felt it, and the shaking is stated to have been very marked near Dagnall, between here and Hemel Hempstead. The sound was like the falling in of an immense mass of rock—followed by echoes—in a cavern.

Some persons say they heard a second, but much less loud, crash later in the morning, but this was not heard by me.

At Ampthill, near Bedford, persons left the town to meet the first train from London to inquire of the passengers as to a possible explosion having occurred in London.

The crash was heard in Bucks, Beds, Herts, Suffolk, Essex, Cambridge, and possibly in other counties. I have seen reports from Newmarket, Hitchin, Cambridge, Wimpole, Heydon, Royston, and Saffron Walden, in addition to accounts from many positions close to this place.

It is curious that Stow records, under A.D. 1250, the thirty-fourth year of the reign of Henry III.:—"Upon St. Lucie's Day, there was a great earthquake in this town (St. Albans) and the parts thereabouts, with a noise underground as tho' it thundered, which was the more strange for that the ground is chalky and sound, nor hollow or loose as those are where earthquakes often happen; and this noise did so fright the daws, rooks, and other birds which sat upon houses or trees, that they flew to and fro, as if they had been frighted by a gosshawk." WORTHINGTON G. SMITH.

Dunstable.

On the Constant P in Observations of Terrestrial Magnetism.

THE formula for P given by Mr. Rücker (NATURE, vol. xxxvi. p. 508) has evidently been obtained by expanding the usual expression rigorously to terms of the second order; but as the usual expression differs from Gauss's theory by terms of the second order, Mr. Rücker's expansion is necessarily inexact to the same extent, and in fact his second order term has no existence in Gauss's theory.

Going only to terms involving r^{-5}, Gauss's equations may be written—

$$f(u) = \mathrm{L}r^{-3} + \mathrm{L}^1 r^{-5} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$f(u_1) = \mathrm{L}r_1^{-3} + \mathrm{L}^1 r_1^{-5} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\frac{m}{\mathrm{H}} = \tfrac{1}{2}\mathrm{L}\left(1 + \frac{\mathrm{S}}{\mathrm{F}}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where $f(u)$ signifies either sin u or tan u according to the form of instrument employed.

By putting

$$\mathrm{A} = \tfrac{1}{2}r^3 f(u) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$\mathrm{A}_1 = \tfrac{1}{2}r_1^3 f(u_1). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$\mathrm{B} = \frac{r_1^2 r^2}{r_1^2 - r^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

we find from (1) and (2) respectively

$$\tfrac{1}{2}\mathrm{L} = \mathrm{A}\left\{1 - \mathrm{B}\left(\frac{\mathrm{A} - \mathrm{A}_1}{\mathrm{A}}\right)r^{-2}\right\} = \mathrm{A}\,(1 - \mathrm{P}r^{-2}). \quad (7)$$

$$\tfrac{1}{2}\mathrm{L} = \mathrm{A}_1\left\{1 - \mathrm{B}\left(\frac{\mathrm{A} - \mathrm{A}_1}{\mathrm{A}_1}\right)r_1^{-2}\right\} = \mathrm{A}_1(1 - \mathrm{P}_1 r_1^{-2}) \quad (8)$$

Whence, by inspection,

$$\mathrm{P} = \mathrm{B}\left(\frac{\mathrm{A} - \mathrm{A}_1}{\mathrm{A}}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$\mathrm{P}_1 = \mathrm{B}\left(\frac{\mathrm{A} - \mathrm{A}_1}{\mathrm{A}_1}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

To find $\tfrac{1}{2}$L we may use either (4) and (9), or (5) and (10); and in either case the result will be as accurate as our fundamental expressions.

Expanding (10) to terms of the second order,

$$\mathrm{P}_1 = \mathrm{B}\left(\frac{\mathrm{A} - \mathrm{A}_1}{\mathrm{A}}\right) + \mathrm{B}\left(\frac{\mathrm{A} - \mathrm{A}_1}{\mathrm{A}}\right)^2 \quad . \quad . \quad . \quad (11)$$

and therefore the mean of (9) and (10) is

$$\mathrm{P}_0 = \mathrm{B}\left\{\left(\frac{\mathrm{A} - \mathrm{A}_1}{\mathrm{A}}\right) + \frac{1}{2}\left(\frac{\mathrm{A} - \mathrm{A}_1}{\mathrm{A}}\right)^2\right\} \quad . \quad (12)$$

whence, by putting

$$\mathrm{C} = \log \mathrm{A} - \log \mathrm{A}_1$$

and remembering that

$$\frac{\mathrm{A} - \mathrm{A}_1}{\mathrm{A}} = \frac{\mathrm{C}}{\mathrm{M}} - \frac{\mathrm{C}^2}{\lfloor 2\mathrm{M}^2} + \frac{\mathrm{C}^3}{\lfloor 3\mathrm{M}^3}, \text{ \&c.} \quad . \quad . \quad . \quad (13)$$

in which M is the modulus of the common system of logarithms, we have to terms of the second order—

$$\mathrm{P}_0 = \frac{r_1^2 r^2}{r_1^2 - r^2}\left\{\frac{\log \mathrm{A} - \log \mathrm{A}_1}{\mathrm{M}}\right\} \quad . \quad . \quad . \quad (14)$$

Equation (9) is what I gave in my letter on p. 366 of the last volume of NATURE, where I was careful to say that it was derived from Gauss's original equations. When properly used it is as accurate as equations (1) and (2). Equation (14) was given by Mr. Ellis in his letter on p. 436. It is slightly easier to compute than (9), and differs from that expression by a term of the second order which is less than the accidental error of observation. The second order term added by Mr. Rücker renders his expression less accurate than either (9) or (14), if Gauss's theory is accepted as correct. WM. HARKNESS.

Washington, D.C., November 4.

I THINK that on reconsideration Prof. Harkness will admit that it is not I who have fallen into error. If only two observations are made, equations (7) and (8) are identical, and there is no need for the introduction of P_0. In like manner if numerous measurements were available in which the error of observation was *nil*, any pair would give the same value of L, and P_0 would again be unnecessary. If, however, the equations are affected by errors of observation, and it be agreed that in combining them we may replace the P's by a single quantity, P_0, it must not be arbitrarily defined. Prof. Harkness assumes that in the case of two observations it must be the mean of P and P_1, but he gives no reasons, and he does not state what value he would adopt if

the measurements were numerous. The proper course is to determine it by the method of least squares.

Writing f for $f(u)$ and omitting $1 + \frac{S}{F}$, (3) may by the aid of (7) be written in the form—

$$\frac{1}{f} = \frac{H}{2m}(r^3 - P_0 r).$$

This is exactly analogous to the equations used by Maxwell in the determination of the quantity A_2, which in his notation and method of development corresponds to P_0 ("Electricity and Magnetism," second edition, vol. ii. p. 100). It is unnecessary to occupy the pages of NATURE with a reproduction *mutatis mutandis* of his formulæ. We can get, as he does, a general expression for P_0 when we have n equations at our disposal, and when $n = 2$ this reduces (in the notation of Prof. Harkness) to—

$$(\alpha) \qquad P_0 = (A - A_1)/(A/r^2 - A_1/r_1^2).$$

If then in a magnetic survey observations are made at two distances at a number of stations, we should take as the final value of P_0 the mean of the most probable values found at each station. As this would be unduly laborious, we approximate. By an obvious transformation (α) becomes—

$$\log\left(1 - \frac{P_0}{r_1^2}\right) - \log\left(1 - \frac{P_0}{r^2}\right) = \log A - \log A_1$$

$$\therefore P_0(r^{-2} - r_1^{-2}) + \frac{P_0^2}{2}(r^{-4} - r_1^{-4}) + \&c. = \frac{\log A - \log A_1}{M}.$$

Thus to a first approximation—

$$(\beta) \qquad P_0 = \frac{r^2 r_1^2}{r_1^2 - r^2} \cdot \frac{\log A - \log A_1}{M}.$$

And if we substitute this value in the small term—

$$(\gamma) \qquad P_0 = \frac{r^2 r_1^2}{r_1^2 - r^2} \cdot \frac{\log A - \log A_1}{M}$$

$$- \frac{r_1^2 r^2}{2} \cdot \frac{r_1^2 + r^2}{(r_1^2 - r^2)^2} \left(\frac{\log A - \log A_1}{M}^2\right).$$

This is the expression I gave. The effect of the small term in (γ) is, as I pointed out, less than the error of experiment, but it diminishes the difference between the rigorous and approximate values of P_0 given in (α) and (β), and it is useful in indicating the magnitude of the difference between them.

Fortunately all methods lead to (β) as a first approximation which we are agreed is close enough for practical purposes. If, however, we regard the observations as fallible, (α) gives a better value of P_0 than (14), and equation (γ) gives a closer approximation to it than (β) does. ARTHUR W. RÜCKER.

Science Schools, South Kensington, November 24.

P.S.—It may be well to add that, although the formula for A_2 is correctly given by Maxwell in line 17, p. 101, the value of A_2 deduced below is incorrect, being really that of $2MA_2/H$. There is another misprint immediately below, δD being substituted for δQ in the second edition.

Instability of Freshly Magnetized Needles.

I SHOULD like to be permitted to support Prof. Rücker in his reply to Prof. Nipher (NATURE, vol. xxxvii. p. 77), with a few remarks on the subject of observations of magnetic dip.

The question of the degree of accuracy of dip observations is one that has been repeatedly raised and discussed. In 1864 in his report to the Board of Visitors, the Astronomer-Royal, Sir G. B. Airy, referred to the matter, and a correspondance between him and the Chairman of the Kew Committee (Mr. L. P. Gassiot) ensued, which is printed *in extenso* in the Report of the British Association for 1864, pp. xxxiv.-xlvii.

In reply to an inquiry by Mr. Gassiot as to whether the paragraph in the Report was intended to apply to dip observations made at the Kew Observatory, Sir G. B. Airy quoted the following statement by Sir E. Sabine :—"The probable error of a *single observation of the dip* with reliable instruments of easy procurement is known to be ± 1'·5. It has been shown to be so by a series of 282 observations made at Kew, employing twelve circles and twenty-four needles, all of the pattern which has been in use at Kew for several years past. The observations were made by seven different observers; the results are published in the Proceedings of the Royal Society, March 1861, vol. xi. p. 156, from entries in the Kew Observatory books, not a single observation having been omitted. The probable error ± 1'·5 may be regarded as including *constant* errors, considering the number of different circles and needles which were employed, as well as the peculiarities of different observers, of whom there were seven" (the italics are General Sabine's). The Astronomer-Royal then concluded by stating "these are the probable errors which I cannot accept as accurate."

As a result of the correspondence, a series of observations was made at both the Greenwich and Kew Observatories by the observers of both institutions, with the same Kew pattern instruments, and then Sir G. Airy wrote, in a letter dated November 15, as follows: "As regards the results of observations, those made with the Kew instruments are consistent to a degree which I never saw before; and the results for dip obtainable with the Kew dip instruments are undoubtedly more consistent and more certain than I had supposed them to be."

A similar inquiry was set on foot by Dr. H. Wild, of St. Petersburg, and in 1886 we made a large number of observations with different needles for him, the resulting error of an observation being in this case ± 1'·3. The most severe test, so far as we are aware, which has been applied to dip observation, is that recently described by M. E. Leyst, of St. Petersburg, in a quarto volume of 133 pages, published in the *Repertorium für Meteorologie*, entitled "Untersuchung über Nadel Inclinatorien."

The author discusses some 6576 observations of dip made with different instruments and needles, and determines their probable errors, which he always find small, so much so that he deduces the corrections to hundredths of a minute of arc. To quote particular cases, he determines from thirty series of comparisons between observations and the simultaneous readings of the magnetographs and the induction inclinometer, that the difference amounts to only 1'·06; and again, by comparing at Pawlowsk the fifteen needles of the three dip instruments of the Pawlowsk, Irkutsk, and Ekaterinburg Observatories (all of English make, obtained through this Observatory), he finds their mean correction to be nil.

Judging from the experience gained at Kew by the examination of probably 150 circles and 500 needles by various makers and different observers, I can thoroughly indorse Prof. Rücker's opinion that Prof. Nipher's instruments are scarcely capable of satisfying modern requirements as to accuracy, and are such that were they submitted to us for examination they would be promptly returned to their makers for adjustment.

G. M. WHIPPLE.

Kew Observatory, November 26.

Gore's Railway.

As I have had several letters concerning my use of Dr. Gore's arrangement, depicted on p. 107 of your last week's issue, perhaps I may as well say that I am aware it is commonly regarded as a Trevelyan rocker, and that I doubt not its function in that connection. This point of view is so familiar to every one, through Tyndall's "Heat," that I thought it unnecessary to mention it. But I have occasionally heard the motion of the ball attributed to the electro-magnetic action of the current on itself—which is impossible—and I thought it useful to point out that it could nevertheless be used as an illustration of electromagnetic force, provided a vertical magnetic field is applied as well as a current. I should imagine the earth not too weak to have an effect under favourable conditions; but of course such an effect would be strictly definite in direction, and reversible.

OLIVER J. LODGE.

The Highclere Bagshots.

THE notice in NATURE for December 1 (p. 104), by my friend Mr. R. S. Herries, of casts of shells in the Bagshot Beds at Highclere tends strongly to confirm the results of my own work in that district. On the strength of physical and stratigraphical evidence, I have shown the development in that neighbourhood of *all the three stages* of the Bagshot formation

as we know it in the London Basin. This will be seen on the publication of a paper which was sent in to the Secretary of the Geological Society on October 10 last, but has not yet been put down by the Council for reading. A. IRVING.

Wellington College, Berks, December 2.

The Ffynnon Beuno and Cae Gwyn Caves.

MR. WORTHINGTON G. SMITH's letter in NATURE of December 1 (p. 105), is so misleading that I hope I may be allowed to reply to it. As is usual with highly prejudiced observers, he has attempted to prove too much for his case, as he might have seen had he taken the trouble to refer to my papers. The scraper which he mentions was submitted to Dr. John Evans for his opinion, and his conclusion as given in my paper in the Proceedings of the Geologists' Association, vol. ix. p. 17, is as follows. The scraper "is not of a river-drift form, so far as at present known, but is precisely like many from the French caves of the reindeer periods, such, for instance, as La Madelaine." Mr. Worthington Smith's contention therefore that it agrees exactly with "the Neolithic scrapers of Icklingham and Mildenhall" can only prove that there is no chronological value in the classification of such implements. I must explain, however, that we have based no argument on the scraper referred to, since it was found, before the explorations were properly commenced, in an open part of the cavern, and, as stated by me in the paper referred to, "it would be improper to dogmatize on this evidence." I may say at once that I entirely demur to any classification based on the form of the implements rather than on the fauna associated with them, and I see no reason whatever to suppose that the worn, roughly-trimmed implements usually found in river gravels are older than the better-preserved flakes and trimmed implements found in caverns, which would be used for a different purpose from the rougher ones. The implements discovered subsequently belong to the so-called oldest types found in caverns, and were associated with Mammalian remains, equally characteristic of the oldest river gravels as of the caverns. Mr. Smith's statements in regard to the drift "in front of the Denbighshire caves" are of so extraordinary a character that I am tempted to ask him, before I criticize those statements, whether he ever visited the Ffynnon Beuno Caves during the course of the explorations, whether he ever saw the section of the drift exposed at the Cae Gwyn Cave, and what evidence he can bring forward to support his statements that the drift "is not in its original position, but distinctly and obviously relaid," and that he doubts "whether before it was relaid it was a true Glacial gravel at all?" I think the members of the British Association Committee, who have carefully conducted the explorations, and have the strongest evidence in support of their conclusion that the caverns, which are now about 400 feet above sea-level, were occupied by man and the animals before the marine drift and boulder-clay covered them over, have a right to ask for the data upon which such statements as those above referred to are based. These relate to facts, and must be dealt with in a different manner from those statements which are made clearly from a bias against the idea of Glacial and pre-Glacial man. Mr. Smith says that he has not been able to read up the literature of the subject, therefore he is probably unaware of the fact that Prof. Prestwich has recently (Quart. Journ. Geol. Soc. for August last) stated that he has arrived at the conclusion that the high-level gravels, with implements, in the valleys of the Somme, Seine, Thames, and Avon date back to Glacial or pre-Glacial times; and that "the great masses of gravel in the neighbourhood of Mildenhall and Lakenheath, also containing flint implements, are certainly not of fluviatile origin"; and that they seem to him "to be part of the phenomena connected with the passage of the great ice-sheet over the eastern counties, and in that sense pre-Glacial." HENRY HICKS.

Hendon, December 2.

Cloud Movements in the Tropics, and Cloud Classification.

A FEW months ago I called attention to the fact that the general movement of the upper clouds in the tropical regions of the Atlantic was from a westerly point; since then I have worked up all my observations (which extend over a period of 331 days spent in these regions in all months of the year except June) with the following results:—

Between latitudes	Upper layer of cloud comes from	Middle layer of cloud comes from
N. 23° and 17°	S. 67° W.	S. 45° W.
N. 16° ,, 11°	S. 56½° W.	S. 83° W.
N. 10° ,, 6°	S. 1° W.	S. 17° W.
N. 5° ,, 0°	N. 41° W.	N. 35° E.
S. 1° ,, 5°	N. 32° W.	N. 78° E.
S. 6° ,, 10°	N. 45° W.	S. 58° W.
S. 11° ,, 15°	N. 53° W.	N. 16° W.
S. 16° ,, 23°	S. 86° W.	N. 55° W.

Taking a general mean for the whole region, this gives for the upper layer of clouds N. 86½° W., and for the middle layer of clouds S. 73° W. These results are from observations taken by myself, and no observation was registered if there was the slightest doubt as to the cloud movement. The ordinary ship register of upper cloud movements is worse than useless, a propagatory movement of the upper clouds being constantly mistaken for their real movement, and the names being hopelessly mixed, the cirro-cumulus being the source of most mistakes.

The cirro-cumulus exceeds all other forms of cloud in extent, ranging from the delicate fine mottles at a great elevation to the large flaky masses quite low down, and until it is considered a middle layer cloud we are certain to have some confusion.

It is quite time that cloud classification was placed on a more satisfactory basis. Now one observer will call a certain form of cirro-cumulus, a cumulo-cirrus; a moderately high (middle layer) stratus of uniform texture, a cirro-stratus; again, one form of low stratus, a pallio-stratus. Another observer will even call a detached fragmentary stratus, cirro-cumulus; and lots of observations will be useless from one observer failing to understand the particular form of cloud A calls pallio-stratus or B calls cirro-cumulus. Far better to keep to Luke Howard's simple nomenculature till some classification is definitely fixed to which all can agree.

To be satisfactory the classification must be founded on the physical and morphological (if I may use the word here) structure of clouds. I find no difficulty in making observers understand the difference between a stratiform and a cumuliform cloud; this is the first step, and once the distinction is thoroughly grasped the rest is comparatively easy. I propose something of this sort. Two orders, the "Stratiforms" and the "Cumuliforms," these to be subdivided into types, and these again into species; *e.g.* taking the ordinary dull-looking stratus commonly seen in anticyclonic areas, it would be described as—

Order	...	Stratus.
Type	...	Low-stratus.
Species	...	Pallio-stratus.

Or take that form of cirrus which appears as lines or threads right across the sky; it would be destribed thus—

Order	...	Stratus.
Type	...	Cirrus.
Species	...	Cirro-filum.

By using this system an observer would be gradually brought to recognize first the broad distinctions and then the minute distinctions in clouds. DAVID WILSON-BARKER.

THE FORMS OF CLOUDS.

SO much attention has been given of late years to the study of clouds, and so many names have been suggested by different writers for the same form of cloud, that the whole question of cloud forms and cloud names must soon be referred to an International Congress. A few remarks on certain broad facts connected with the shapes of clouds, and on the fundamental principles by which weather forecasts are deduced from these forms, may therefore be acceptable to those who have not given special attention to the subject.

The two most important facts which must never be forgotten are: (1) that cloud forms are essentially the same all over the world; and (2) that there are only five or six distinct structures of clouds.

The identity of cloud forms all over the world has recently been demonstrated both before the Royal Society and the Royal Meteorological Society of London by the exhibition of about fifty photographs of clouds taken by the writer in various longitudes, and in latitudes ranging from 72° N. to 56° S. Some of these are reproduced in the illustrations of this article, and the conclusion of identity is irresistible. The cirrifying cloud over an irregular cumulus, in Fig. 3, might be seen over any summer thunderstorm in England, though this example is from tropical Borneo; while the fleecy cirro-cumulus in Fig. 4, which was taken near the Falkland Islands, about 51° S., differs in no respect from the similar cloud we so often see at home. Fig. 6 is a strato-cumulus from near Teneriffe, in the heart of the North-east Trade; but the writer has seen an absolutely identical sky from the summit of North Cape, far within the Arctic Circle.

Fig. 1.—Cirrus wisp over cumulus. Folkestone.

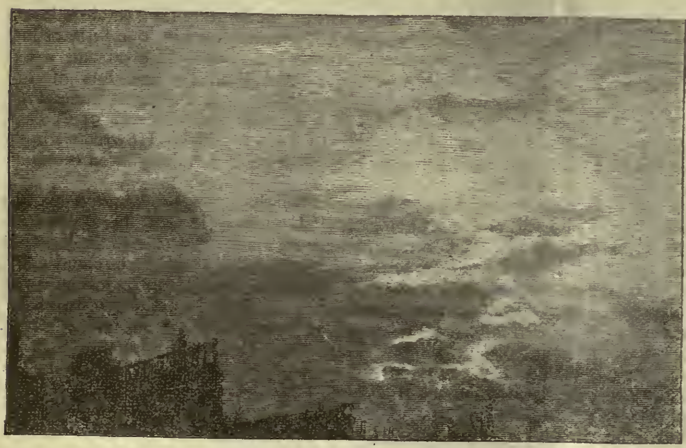


Fig. 2.—Stratus. London

Fig. 3.—Cumulo-nimbus, cirrifying above. Borneo.

The different structures of clouds can certainly be reduced essentially to five or six types. A great deal must of course depend on the definition we adopt of a kind or species of cloud. We believe that one German meteorologist in Rhineland says that he has discovered 30,000 different kinds of cloud, and that he has not yet finished his classification. This is absurd; for though no two clouds are ever exactly the same, any more than any two faces, still certain broad types of cloud structure can readily be recognized.

The first primary type of structure is the cirriform or hairy. The thin fibres of white silvery cloud which constitute a cirrus may assume an almost infinite variety of forms. The commonest is the simple wisp of white threads such as is shown in Fig. 1, floating at a high level over a heavy mass of cumulus cloud. Sometimes the cirrus lies in long straight stripes, which Ley has shown have a great value in forecasting weather; or at other times assumes the "penniform" or plume-like appearance which, according to Vines, precedes a hurricane in the Antilles.

Cirrus as a rule is formed at very high levels—20,000 to 25,000 feet—and the constituent particles are undoubtedly frozen, but we occasionally find a fibrous structure at low levels, where the constituent particles are certainly in a fluid form. Both the cirrus and cumulus in Fig. 1 are composed of icy particles, for the picture was taken on a cold winter day in England when snow showers were flying about. But in Fig. 3 we see a fibrous combed-out structure at quite a low level in Borneo, where the temperature both of the air and the rain makes it certain that the whole cloud mass was made up of liquid particles.

The true cumuloform structure of cloud can never be mistaken. The rising mass of condensed vapour assumes a rocky, lumpy appearance, which is well delineated in the lower portion of Fig. 1. The varieties of form are infinite. Sometimes beautiful little isolated cloudlets, each with its own flat base, float all over the sky, while at other times we only see mountainous masses rising above a gloomy cloud bank on the horizon, as in Fig. 3.

Essentially different from the above is the stratiform structure which is depicted in Fig. 2. Here we have a thin layer of flat cloud, at low level, more or less broken, but showing no trace of either a fibrous, rolled, or lumpy structure. When the sky is broken, this form of cloud is unmistakable, but when overcast it is impossible to dis-

tinguish pure stratus from the flat under surface of some kinds of cumulus or nimbus.

The term nimbus is applied to any cloud which is precipitating rain. In practice we find two rather distinct types—a strato-nimbus or flat cloud, and a cumulo-nimbus or rocky rain-cloud. The former is characteristic of the rainfall in front of an extra-tropical cyclone, the latter of the precipitation from squalls and thunderstorms all over the world. Our illustration (Fig. 3) represents a distant view of the clouds over a thunderstorm in Borneo. Below we see the rocky summits of a mass of cumulo-nimbus, while apparently above, but really at about the same level, we find the characteristic fibrous structure that is called "goat's hair" by some, or "false cirrus" by others.

Another typical structure is that which has been called in all times by all nations fleecy, woolly, or some cognate name. In this, clouds assume the appearance of a fleece of wool. Each little mass of condensed vapour has a peculiar fibrous structure, quite different from true cirrus. The density and level of formation vary a good deal. When the cloud is thin at up to about 25,000 feet, most meteorologists call it cirro-cumulus; but when denser, and down at about 18,000 feet, the name of cumulo-cirrus has been proposed to distinguish this low variety. Fig. 4 is an excellent specimen of cirro-cumulus, from a photograph taken near the Falkland Islands

There is a form of cloud intermediate between pure cirrus and pure stratus which is so common and so characteristic of bad weather that it has universally been classified as cirro-stratus. We apply the term to a sky which is covered with a thin layer of cirrus fibres, more or less mixed up with a formless haze or veil of scattered ice-particles. Sometimes the cirrus threads are thin and white as the finest gossamer, and float 25,000 to 27,000 feet above the earth, but at other times the structure is coarser, and the level of formation not more than 18,000 feet. The first kind is called cirro-stratus, the second strato-cirrus. Fig. 5 is an example of a rather heavy cirro-stratus, taken near Dover. It will be observed that there are two distinct lines of structure about which the cloud masses are grouped, and that the lines intersect one another at a certain angle, so that the whole has a certain reticulated appearance. This is most characteristic of cirro-stratus.

Cirro-stratus with its hairy structure, and cirro-cumulus with its fleecy appearance, might at first sight appear to be radically different from one another; but they are not so really. It is by no means uncommon to see a patch of fibrous cirro-stratus suddenly become fleecy for a few minutes, and then return again to its former state. We cannot give the reason for this, as the origin of both structures is at present unknown.

There is a form of cloud, intermediate between stratus and cumulus, to which the word strato-cumulus is appropriately applied. In this the cloud layer is too lumpy to be called pure stratus, and not rocky enough to be called cumulus. Fig. 6 is an excellent specimen of this type, taken near Teneriffe; and here we see the lumpy masses of cloud getting apparently thinner and thinner as they approach the horizon, till they look at last like a series of stripes or rolls parallel to the horizon. This of course is the result of perspective.

Fig. 4.—Cirro-cumulus, or fleecy structure. Falkland Islands.

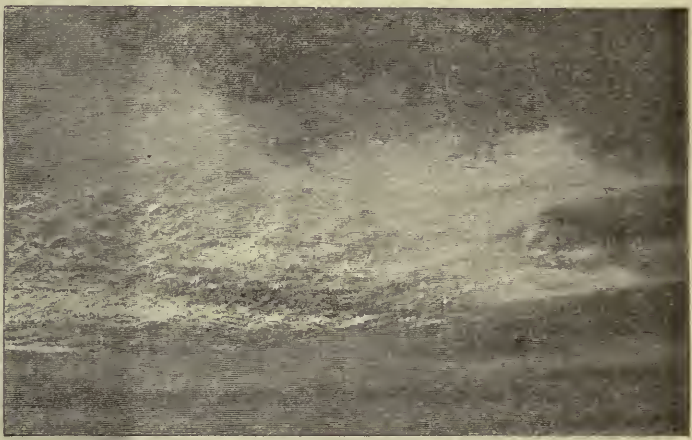


Fig. 5.—Cirro-stratus. Folkestone.

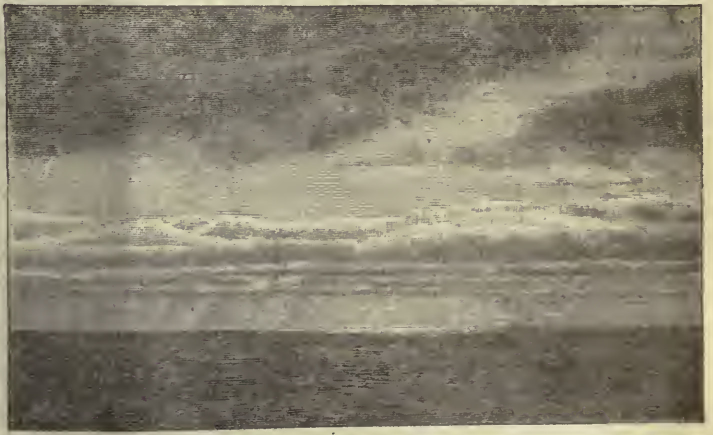


Fig. 6.—Strato-cumulus. Near Teneriffe.

The ten varieties of cloud which we have now described

—cirrus, cumulus, stratus, nimbus and cumulo-nimbus, cirro-cumulus and cumulo-cirrus, cirro-stratus and strato-cirrus, together with strato-cumulus—comprise all the important kinds of clouds; and there are really only five distinct types of structure—cirrus, stratus, cumulus, nimbus, and cirro-cumulus. Prof. Hildebrandsson and myself consider that the ten words above mentioned, compounded out of only four Latin words, are practically sufficient for all ordinary purposes.

Specialists in clouds will of course want more minute varieties, such as different names for some of the kinds of cirrus, and for the low broken clouds, such as scud, wrack, &c. There are also a whole class of pendulous clouds, such as festooned stratus, pocky cloud, or mammato-cumulus; and the long black wreaths of cloud in front of certain types of thunderstorm, but these are all very local, and also very short-lived, so that they need only be mentioned here.

So far for the mere external forms of clouds as they would strike a savage or an artist; but to the meteorologist there is a philosophy behind them. In England some forms presage wind and rain, others indicate the advent of fine weather; while recently it has been shown that different kinds of clouds are developed in different parts of cyclones and anticyclones. For instance, cirro-stratus forms in front, cumulus in rear, of a cyclone; while fleecy cirro-cumulus is very characteristic of the western side of the anticyclones.

But then we are met by the apparent paradox that precisely the same forms of cloud are found on the equator where neither cyclone nor anticyclone was ever developed. Moreover, the same cloud does not prognosticate the same weather all over the world, and even in the same country the same cloud may indicate either good or bad weather according to the circumstances under which it is developed. For instance, cumulus in England is sometimes the associate of a fine day, other times the forerunner of a shower.

The clue to the whole puzzle lies in the fact that the same form of cloud can be produced under totally different circumstances. Vapour-laden air can only condense into cloud, and then be drawn out or rolled about between different currents in a very limited number of ways, and hence the small number of really distinct varieties of cloud structure.

Let us take the case of cumulus in detail as an example of general principles. Cumulus is always the condensed capital of an ascensional column of air, but the source of the uptake need not always be the same. For instance, air may rise either (1) from ordinary evaporation on a fine day; (2) from the uptake of a cyclonic vortex; (3) from the collision between two opposite currents.

The first—evaporation—is the source of fine-weather cumulus in England and all over the world; while the uptake of a cyclone is the cause of rainy cumulus wherever such eddies are formed. The rainy cumulus of the equator is the product of squalls and thunderstorms whose nature at present is unknown in most cases; but one very common cause is the collision between the land and sea breezes of the tropics. The two opposing currents meet, one is forced upwards, and then mountainous cumulus is the result. The cumulo-nimbus in Fig. 3 is over a thunderstorm in Borneo, due to the collision of the land and sea breezes.

All, therefore, that we can say for certain when we see a cumulus cloud is that an ascensional current of air has risen to the level of condensation. What future weather the cloud prognosticates depends on circumstances, and must be judged by our experience and knowledge of the climate in which we may happen to be. Clouds always tell a true story, but one which is hard to read; and the language of England is not the language of Borneo. The form alone only shows that a certain form of condensation is taking place; the true import must be judged by the surroundings, just as the sense of many words can only be judged by the context.

RALPH ABERCROMBY.

FIFTH ANNUAL REPORT OF THE FISHERY BOARD FOR SCOTLAND.

THE Report for 1886 contains so much of general interest that it deserves the attention of many who look upon a Blue-book as the driest of reading, only attractive to those whom it may immediately concern. It is desirable that the scope and practical aims of the Board should be more generally known, and the public should appreciate the excellent work done by it, instead of regarding this as the mere outcome of scientific leanings to certain lines of investigation. The fisheries of Scotland continue to be very productive, and nothing is more striking about them than the great and increasing yield of the herring fishery. Though this increase and the low price at which the herrings have been sold have proved a great boon to the community, especially to the poorer classes, it is deeply to be regretted that the crews sustained very heavy losses from the glutting of the market consequent on the large takes and low prices. A striking feature of the summer herring fishery of 1885 was that many in-shore grounds where herrings had been found in great abundance in previous years but which had been recently all but deserted were restored to their former fertility. This was even more marked in the season of 1886, as all along the east coast from Montrose to the Pentland Firth there seemed to be one immense unbroken shoal of herrings, lying from one to ten miles off land. At no former period in the history of this fishery were the catches so heavy. The winter herring fishery on the east coast was the most productive ever known, yielding a total catch of 128,441 crans. The gross total value of the sea and salmon fisheries of Scotland for 1886 was £2,550,778 8*s*. 3*d*.

During the past year the scientific work consisted chiefly in carrying on the trawling experiments required by the recent Act of Parliament (Sea Fisheries (Scotland) Amendment Act, 1885), but in addition investigations were made as to the development, artificial hatching, structure, and habits of the more important useful fishes. An important part of the inquiry as to the influence of trawling consisted in arranging to obtain statistics showing the quantities of fish landed from the restricted areas, and the conditions under which they were captured—an extremely difficult matter to arrange.

The Board's marine station at St. Andrews has again been under the direction of Prof. McIntosh, whose Report shows that important work on the life-histories and development of the food-fishes has been done at this station by him and Mr. E. E. Prince, by Dr. Scharff on the intra-ovarian eggs of food-fishes, and by Mr. Wilson on the development of the common mussel. The memoir first mentioned, viz. that on the development and life-histories of the food-fishes, is now ready for publication, and is illustrated by thirty-one quarto plates. Its size and the nature of the illustrations of course render it unsuitable for a Parliamentary Blue-book.

The "Report on the Trawling Experiments on the East Coast, Part I. Preliminary," by Prof. Ewart and Sir J. Ramsay Gibson-Maitland, gives the results of an important item in last year's work. The Act already referred to having empowered the Scotch Fishery Board to frame by-laws for the better regulation of sea-fishing, and one such law having been framed, passed, and confirmed, it was necessary to make arrangements to discover, if possible, what influence the prohibition of trawling under the by-law would have in leading to

an increase of fish in the protected waters. At the outset it was evident that it would be necessary to make systematic observations on the various areas by trawling along the same lines, and as nearly as possible under the same conditions, as the ordinary steam trawlers; and further, that it would be equally necessary to obtain as far as possible a record of the fish captured day by day from the various grounds in the Firth of Forth, St. Andrews and Aberdeen Bays.

Representations having been made that a small steam-vessel properly fitted out was indispensable, and a sum for the buying and maintenance of such a vessel having been granted, the *Garland*, an iron fishing yacht, was purchased and duly equipped. She was provided with a steam winch, trawling gear, dredges, &c., and later it was found desirable to add a small bridge to admit of a better "look-out" being kept when at work during the night in the vicinity of small fishing-boats, often imperfectly protected by lights. The beam of the trawl provided is twenty-five feet in length, *i.e.* about half the length of those used by the ordinary steam trawlers. This size was selected partly to suit the weight of the ship, and partly to cause as little disturbance as possible to the fishing-grounds when under periodical inspection. Special forms were prepared to admit of a complete record being kept of the fish taken by the trawl, dredge, and tow-net, and of the temperature, state of the weather, &c. The *Garland* was supplied with charts, showing the extent and direction in which the trawl was to be carried in working over the various trawling stations, and with several books of reference, bottles, tanks, &c., for the preservation of spawn, young fish, crustacea, and other objects which required to be afterwards examined or identified. Recently a complete set of thermometers and other instruments for making physical observations have been provided, and the necessary instructions given for their use.

In the present Report it is pointed out that the Firth of Forth is well adapted either as a feeding-ground or a nursery for the most important of our food-fishes and also for shell-fish. As a matter of fact, there is not, it is stated, on the east coast of England or anywhere else on the coast of Scotland, a stretch of water with so many natural advantages from the fishermen's point of view as the Firth of Forth. The fresh water carries with it food for mussels and other shell-fish. The sea brings in food for herring and other round fish. The water varies considerably in depth and salinity, and the bottom at one part consists of sand or mud, at another of gravel or shingle, and at another of rocks, sometimes bare, sometimes covered with sea-weed, and the temperature throughout the year is fairly constant, there never being great heat in summer or very great cold in winter. The physical conditions of St. Andrews Bay are entirely unlike those of the Forth, and this being the case the fauna may naturally be expected to differ considerably. There has not been time to prepare a complete account of the fauna of St. Andrews Bay during the different months of the year, but it is hoped that with the help of Prof. McIntosh a first list will be ready for the next Report. It is, however, known already that the rocky ground on the south shore is rich in mollusks, crustacea, marine worms, cœlenterates, &c.; and that starfish and other echinoderms, edible, swimming, and hermit crabs and other crustacea are scattered in abundance over the sandy bottom of the bay, and especially that mussels abound near the mouth of the Eden. Further, swimming or pelagic forms (including at certain seasons of the year schools of young fish, crustacea, and mollusks) teem in the surface and deeper waters. As is to be expected from the nature of the bottom, flat fish far out-number round fish all over the bay. The flat fish are chiefly represented by several kinds of dabs, by plaice, flounders, skate, and brill, and at times turbot; and in addition the bay is visited by haddock, whiting, cod, and other round fish. Aberdeen Bay corresponds in some respects with St. Andrews Bay, but the closed area includes not the bay proper so much as a narrow portion of the territorial waters (some eighteen miles in length) which extends from Girdle Ness to the Cruden Scars. This area, very narrow at certain points, never reaches a width of three miles. The Dee, Don, and Ythan flow into the bay, but the fresh water flows over the salt without mingling with it as in the Forth to form a true estuary. The bottom consists chiefly of sand, but towards the north and south sand gives place to rock. The fauna resembles that found at St. Andrews, but although flat fish are relatively plentiful, whiting are far more abundant. It is to be observed that, in comparing the prominent features of the three districts investigated, the Firth of Forth is characterized by an abundance of haddocks, St. Andrews Bay by the predominance of flat fish, and Aberdeen Bay by the large number of gurnards and whitings. As regards the practical working of the by-law, it is only necessary to add that although only a year has elapsed since it was passed, providing for a limited form of protection for the waters referred to, there are already some signs of improvement both in the number and size of the less migratory flat fish, and in the number of young round fish which visit the territorial waters for long or short periods. The fishermen of the Forth and St. Andrews Bay state they are already obtaining better takes of flat fish, and that they believe in a few years the in-shore grounds will have recovered to a considerable extent their former richness.

Prof. Ewart gives an interesting paper on "The Artificial Hatching and Rearing of Sea Fish." The publication last year of "The History of Howietoun" (Sir J. Ramsay Gibson-Maitland) marks an epoch in the history of fish-culture. It affords proof that the Salmonidæ at least can be bred and reared in confinement as successfully as any of the smaller domestic animals, and that fish-culture, notwithstanding all the reverses it has suffered through the misplaced zeal and energy of its many would-be advocates, has a great future before it, not only in re-stocking our own rivers and lakes, but also in peopling the waters of all countries where the conditions are favourable to the development and growth of the Salmonidæ and other valuable food-fishes. Fish-culture at Howietoun has been reduced to a science. Every step in the process, from the impregnation of the eggs to the rearing of the mature fish, has been thoroughly mastered and systematized. So careful have the observations been from first to last, that it is now possible to produce, within certain limits, considerable modifications in the time at which the eggs mature and hatch, and in the rate of growth of both the fry and the older fish; and, further, many hybrids have been bred, the genealogy of which is not a little hard without the aid of an ancestral tree to fully comprehend.

The reasons for putting such knowledge acquired to a practical application are that the demand for salmon is greater than formerly, and the nature of the spawning-grounds has been altered. Nature provides for all natural losses, but she does not, and cannot be expected to cope with those created by the necessities of civilization. It is for science to step in and help to solve the problem of supply and demand.

Unlike the higher animals, fish are not protected in the early stages, and the food-fishes even less than others. A very limited acquaintance with the life-history of sea-fish enables one to readily understand that, though the culture of salmon and trout may be highly advantageous, and often all but imperative, it does not follow that this is the case with the herring and cod and their allies. The most sanguine pisciculturist would scarce dare propose to increase the number of the more migratory fish that live in the open sea. It has been suggested that, by hatching

fish in-shore, local races might be formed; but this is taking for granted that during the process of incubation the fish are brought under some remarkable spell which arrests their strongly inherited instincts, and leads them to settle down for life in the vicinity of their birthplace, instead of roaming about to see the world like their free-born cousins. It seems, therefore, too much to expect cod and haddock and other wanderers to remain always about our doors because they happened to see the light under artificial instead of natural conditions. But though fish-hatching may not be able to influence much, if at all, the number of fish in the open sea, and though it may not be able to establish local races or shoals, it may still be of great service. In the first place, it may be the means of introducing fish, which have the migratory instinct fairly well developed, into waters where they practically did not previously exist. For example, by instituting hatcheries in the upper reaches of some of the long fjords in Norway, a large school of haddocks or other round fish might be readily created which might find all the conditions necessary to their existence without wandering into the open sea; and, in fact, the same results might follow the hatching on a large scale of round fish in some of our own firths and bays. Again, as in America, it might be possible to produce shoals of fish, such as the shad, which, by wandering along the coast or living in the estuaries, would be the means of attracting large and more valuable forms to the in-shore grounds; fish, in fact, which would act the part of the herring, but be a more constant source of attraction—remaining in the firths for several months at a time. Lastly, fish-culture may have a great future before it in hatching flat fish, which have the double advantage of being extremely valuable, while they are often very limited in their migrations. The artificial hatching of sea-fish has not yet had time to obtain a firm footing; for the first trustworthy experiments made were those of the German Commissioners (Meyer, Möbius, and others), who hatched numerous herring in 1874 in the Bay of Kiel. As is well known, Norway has a Society for Promoting the Norwegian Fisheries, with branches at the principal fishing centres. In 1882 an experimental station under Captain G. M. Dannevig was started at Flödevig, near Arendal, where millions of sea-fish have been hatched, and a number of cod and herring reared in a pond near the hatching station. The question of hatching sea-fish is under consideration at the present moment at Grimsby. It is proposed to found a hatchery at Cleethorpes to propagate round and flat fish, with a view of replenishing the exhausted in-shore waters of the North Sea. Even should this experiment prove unsuccessful, it will be of importance in furnishing and spreading the technical education and information so much required among those engaged in the fishing industry.

To successfully hatch sea-fish in large numbers, the first and last requisite is an abundant supply of pure sea-water. This necessitates a small sea-pond and a number of large tanks, from which a constant supply of pure filtered water can be readily obtained. In addition to having at command an abundant supply of sea-water, it is, of course, necessary to have the hatching-station in the vicinity of some rich fishing-ground, where plenty ripe fish may be obtained when wanted.

Given plenty pure sea-water and a number of ripe fish, the next desideratum is a hatching apparatus, the form of which must depend on the nature of the eggs to be manipulated. While herring eggs are heavy, and not only fall to the bottom, but adhere to whatever they touch, the eggs of most of the food-fishes are non-adhesive and lighter than sea-water, and hence they float at or near the surface. Prof. Ewart describes and figures a promisingly practical hatching jar for adhesive eggs lately designed and used by himself, also the apparatus used at Arendal for floating eggs, the most successful hitherto devised. With such apparatus it would be possible, at a very small outlay, to hatch millions of floating food-fish eggs, and thus to restore and maintain the original productiveness of the in-shore fisheries. The conclusion is that we ought to establish hatching stations at one or more centres. One might be for round fish, the other for lobsters and other shell fish. The Firth of Forth and the Cromarty Firth seem admirably adapted for the purpose, one great point being that minute pelagic forms, such as the young fry feed on, are remarkably abundant in both. A hatching station could be provided for about £1000, and it is hoped the Board may soon obtain a vote for the purpose. The hatching operations at Flödevig, of the report of which Prof. Ewart gives an interesting abstract, shows that many important practical questions have been settled, and the conclusions reached at Howietoun and elsewhere as to the influence of extreme temperatures, sudden changes in the surroundings, and also on the eggs and young spawn on full-grown fish, have been well confirmed.

Mr. Duncan Matthews gives (Part I.) a long paper, excellently done, on "The Structure of the Herring and other Clupeoids," with a series of capital plates; also Part II. of the "Report as to variety among the Herrings of the Scotch Coast"; notes on "The Food of the Whiting," and on the "Ova, Fry, and Nest of the Ballan Wrasse." Mr. R. D. Clarkson's paper "On the Nutritive Value and Relative Digestibility of White Fish" is as interesting from the dietetic point of view as Mr. C. E Fryer's suggestions for "The Preparation of Sprats and other Fish as Sardines" is from the economic. Prof. McIntosh reports on the work done last year at the St. Andrews Marine Laboratory. The other scientific investigations include notes on "The Food of Young Gadidæ," and on "The Spawning of the Pike," by Mr. George Brook; on "Entomostraca," by Mr. G. S. Brady; a paper on the "Development of the Common Mussel," by Mr. John Wilson; one on "The Physical Conditions of the Water of the Firth of Forth," by Dr. H. R. Mill; and a "Further Report on the Examinations of River-waters for Micro-organisms," by Prof. Greenfield and Mr. John Gibson. There are a number of tables and plates which add greatly to the interest and usefulness of the work.

PROFESSOR A. WEISMANN'S THEORY OF POLAR BODIES.

ONE of the most noticeable features at the recent meeting of the British Association at Manchester was the manner in which naturalists of all nationalities agreed to do honour to Prof. Weismann, who has contributed to theoretic biology in the last few years with as lavish a hand as that with which he formerly enriched the practical side of the science through detailed observation and far-reaching induction.

Of his later speculations upon the significance of obscure reproductive phenomena, the first[1] was abridged by Prof. H. N. Moseley (NATURE, vol. xxxiii. p. 154); while perhaps the most important contribution to biological science at the Manchester meeting was an abstract of the newer pamphlet[2] recently reprinted in this journal (vol. xxxvi. p. 607). The necessary limits of such an abstract precluded any account of the observations which appeared to support Prof. Weismann's views, as also of the details of the process by which, as he supposes, the plasmata are removed in the polar bodies. As neither the original pamphlet nor the still later account of his observations upon parthenogenetic eggs are generally accessible, it has been suggested that some additional points, in expansion of the abstract, should be given in these columns.

1 "Die Continuität des Keimplasma's," Jena, 1885, 122 pages.
2 "Die Zahl der Richtungskörper," Jena, 1887, 75 pages.

Certain recent observations on the maturation of the ovum are of great interest in this connection, as illustrating the possible mechanism by which ovogenous plasma in the extrusion of the first polar body, and a number of ancestral plasmata in that of the second, are removed from the nucleus of the ovum; the former process being designed to *equalize* in bulk the ovogenous and germinal plasmata contained in the nucleus (*Aequationstheilung*), the latter to *reduce* the total number of ancestral plasmata present by a half (*Reduktions-theilung*).

For this reduction in number of the ancestral plasmata there must be a second and special form of karyokinesis not as yet generally recognized. If any value be attached to the fact, first observed by Flemming,[1] that in normal karyokinesis the nuclear loops are split longitudinally, one of the resultant halves passing to each daughter-nucleus, then the two nuclei produced by such division must be precisely alike, not only quantitatively, but qualitatively. For Prof. Weismann's view, however, there must exist "a type of karyokinesis in which the primary equatorial loops are not split up, but separated into two groups, each of which groups forms one of the two daughter-nuclei." E. Van Beneden[2] has already shown that in the formation of the polar body of *Ascaris megalocephala* the nuclear division differs from the usual type of karyokinesis in that the plane of division is at right angles to the normal; and Carnoy[3] has more recently essentially confirmed the observation, and has further added that, of the eight nuclear loops which are to be found in the equator of the spindle, four are removed in the extrusion of the first polar body, and two of the remaining four with the second. Were it certain that each of the eight loops consisted of ancestral plasma, it would be necessary to regard the first division of the nucleus as a process of reduction, not of equalization; but this is not to be accepted, mainly because the extrusion of the first polar body is to be found also in parthenogenetic ova. With more probability the first polar body of the ovum of *Ascaris* is to be regarded as removing ovogenous plasma, since we know, through the observations of Flemming and Carnoy, that under certain conditions secondary splitting and consequent numerical duplication of the nuclear loops may occur. This shows, in Prof. Weismann's view, that there exist nuclei in which the same ancestral plasmata may be present in different loops. Such "identical loops," however, are not necessarily at the same ontogenetic grade; and this is probably the case here, as the four loops of the first polar body must be regarded as ovogenous plasma, the other four as germinal plasma. This would be practically proved if it could be shown that the eight loops were produced by longitudinal splitting of four primary loops, since such splitting is the means of separating plasmata of different ontogenetic grade from one another, without diminution of the number of ancestral plasmata.

With regard to the male cell, the facts at our disposal are too few to enable us to speak with such confidence as is the case with the ovum, but whether the theory of Pangenesis, or of the Continuity of Germ-plasma, be proved correct, a process of reduction of ancestral plasm similar to that occurring to the ovum must also take place in the maturation of the sperm cell, though probably after a different manner. The ancestral plasmata of the ovum undergo reduction only at the termination of ovarial maturation. Supposing, however, that reduction affected the first ovicell of an organism only, and that the rest were produced from this by normal division, then there would be practically but two kinds of ova in the ripe ovary, coresponding to the two halves of the original ovicell, and but two kinds of individuals produced from them, the members of each kind resembling each other as closely as twins. On the

[1] *Arch. Mikr. Anat.* xvi., and elsewhere.
[2] *Arch. Biol.* iv.
[3] *La Cellule*, 1886.

other hand, the later the period of germ-cell-formation at which the reduction is effected, the more will the ova differ in composition from one another, and the greater scope is afforded for variation among the resultant individuals. Finally, if reduction be deferred till the ova be mature, the variation insured among the progeny is as great as it is possible to achieve. The production of such maximum variation is the probable explanation of the fact that the second polar body is not extruded till the *end* of maturation. With the sperm-cells, however, the conditions of number and size are different from those obtaining in the ova. Though it is quite conceivable that the process of reduction may be deferred till the completion of sperm-cell formation (both of the fission-products probably remaining as sperm-cells), still the other possibility must also be considered—namely, that it may take place at an earlier date in the formation of the sperm, since the opportunity for extreme variation, however necessary in the case of ova of which a considerable proportion are fertilized, is far less requisite among sperm-cells, of which perhaps one in a hundred thousand or a million may be actually effective. The question can only be settled when we know which of the forms of nuclear division it is that effects the reduction of ancestral plasmata; in the meantime there is evidence to show that different types of fission are found at different stages of sperm-cell formation. Van Beneden and Julin[1] have shown that direct and karyokinetic division alternate in the spermatogenesis of *Ascaris megalocephala;* and the observations of Carnoy[2] and Plattner[3] on Arthropoda further point to the occurrence, at certain stages, of that less-known type of karyokinesis which, according to Prof. Weismann, is cha[illegible] the process of reduction. The "*Nebenkern*," described by La Valette St. George as occurring at the penultimate stage of spermatogenesis, is probably comparable to the first polar body extruded by the ovum.

As is now generally known, Prof. Weismann has succeeded in demonstrating that one, and only one, polar body is extruded from the parthenogenetic ovum; but the memoir[4] dealing with the details is but recently published, and is in a periodical inaccessible to most readers. His observations cover the following species:—

CLADOCERA.
Polyphemus oculus.
Leptodora hyalina.
Bythotrephes longimamus.
Moina rectirostris.
Moina paradoxa.
Daphnia longispina.
Daphnella brachyura.
Sida crystallina.

OSTRACODA.
Cypris reptans.
Cypris fusca.

ROTIFERA.
Callidina bidens.
Conochilus volvox.

The process in the Cladocera is as follows. The nucleus of the ovum approaches the periphery, and becomes gradually fainter till it is no longer recognizable except by the help of reagents. A normal nuclear spindle is then formed, and the polar body cut off with the resultant half-nucleus. After extrusion, the polar body may in some instances not only segment, and one of the resultant cells again segment, but, in the case at least of *Moina*, it appears that it secretes that part of the egg-shell which immediately overlies it, so that its true cell-nature is indisputable.

With regard to the Rotifera, a group in which its occurrence has been denied, a true parthenogenesis is proved by the following observation. A female of *Callidina* with two uterine embryos was isolated on a slide; after

[1] *Bull. Acad. Belg.* (3) vii. 312.
[2] *La Cellule*, 1885.
[3] *Intern. Monatschr. f. Anat. Histol.* iii. Heft 10.
[4] Weismann and Ischikawa, "Ueber die Bildung der Richtungskörper bei thierischen Eiern," *Ber. Naturf. Gesell.*, Freiburg i. B., iii. pt. 1, 44 pages, 4 plates.

the lapse of a day was born a young one, which possessed in its uterus an ovum already in segmentation. From this ovum two days later was produced a third female, while a second ovum in the uterus of the mother was also already commencing to segment. Whether *all* the "summer" (parthenogenetic) eggs develop into females or not, has yet to be proved. Here also it was shown that one polar body was extruded.

The second part of the memoir sums up the literature relating to the subject, with the result that the extrusion of two primary polar bodies from fertilized ova has been demonstrated in sixty-six cases, that of one only from parthenogenetic ova in fourteen cases; while none of the few observers who describe the extrusion of one polar body only from a fertilized ovum, have endeavoured to show that a second one may not have been present, at an ontogenetic period other than that which they describe.

G. HERBERT FOWLER.

SIEMENS'S GAS-BURNERS.

OWING to the very high temperature of ignition of gas, the only way in which it can be successfully used with the greatest economy is by the application of regenerators. This was proved practically by the late Sir William Siemens in carrying out his own and his brother's invention of the regenerative gas-furnace. For more than eight years now Mr. Frederick Siemens has devoted a portion of his attention to the domestic applications of gas, and he has quite recently opened a depot in the Horseferry Road for the sale of his gas-lamps. Here, on Tuesday afternoon, he entertained several gentlemen interested in gas illumination, and tested photometrically some of the burners we propose to describe and illustrate.

It is well known that the light intensity of a flame increases with its temperature in a higher ratio than the arithmetical, although the actual ratio has not been absolutely determined. It is, moreover, known that the more the energy of flame is transformed into radiant light and heat the less is the amount carried away in the products of combustion. But the difficulty is to take advantage of these laws in practice, and to combine high temperature with durability, and the use of regenerators with simplicity of arrangement and elegance of appearance in the lamp.

After the Smoke Abatement Exhibition, at which Mr. Frederick Siemens's regenerative gas-lamps were for the first time exhibited in this country, a great outcry was raised on account of their unsightliness. The economy

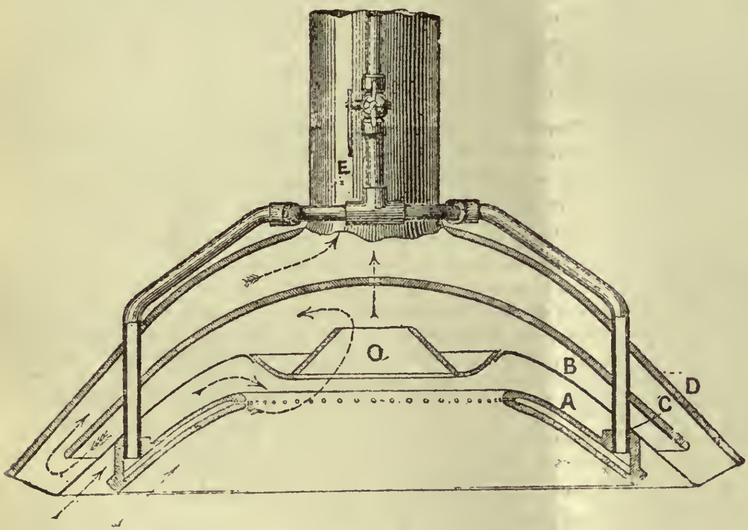


FIG. 1.

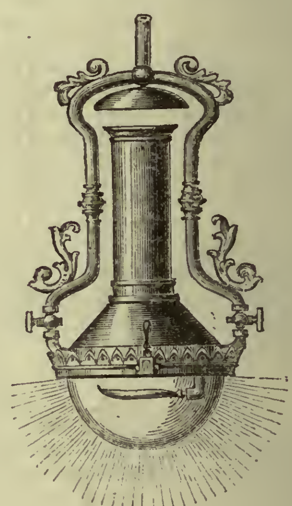


FIG. 2.

of the light and its intensity were in vain dwelt upon: the British public were not inclined to entertain the new lamps, and a comparatively small business was done in them. Besides this, it was discovered by degrees that when impure gas was employed the gas passages became blocked with a sulphurous deposit, so that, in order to maintain their high efficiency when in constant use, these passages had from time to time to be cleared. Mr. Siemens set to work to overcome both these defects, and the lamps he has now produced lend themselves to artistic ornamentation, and have no passages to offer obstructions to gas of ordinary quality.

The lamp we propose to describe in the first instance is the one known as the open-flame sunlight pattern. It is designed for use in positions where the ordinary sunlight lamp is employed, such as public halls, concert, dining, and billiard rooms, banks, and theatres. The flame in this lamp is extremely delicate and elegant in appearance, having the form of an inverted cone of light, apparently unsupported. The annexed drawing illustrates the construction of this lamp. Four hoods made of suitable material are arranged one above the other so as to form passages through which the products of combustion are removed, their waste heat being utilized to heat the air supplied to the flame. The jets of flame issue in a ring from the lowest hood; the products of combustion, passing through the aperture O, are drawn downwards through the annular space B, and then upwards through C to the chimney E. The hood between the passages A and B is intensely heated by the products of combustion descending on its upper surface; and the air which travels through the annular space A, on its way to supply the gas-jets, takes up the heat from the hood, the flame being thus supplied with heated air, as well as burning in an intensely hot atmosphere. The lamp we were shown consumes 24 cubic feet of gas per hour, and gives, with ordinary London gas, a light equal to 180 sperm candles, or 7·5 candles per cubic foot of gas, which is more than twice the light obtainable from the same amount of gas burnt in ordinary burners. This lamp was set up with a ventilator, but was much too brilliant for use in the room in which it was exhibited, the ceiling being only about 12 feet above the floor, whereas it should be placed at an elevation of 30 feet or more, when it would not only serve for illuminating-purposes, but also for those of heating.

The Siemens regenerative flat-flame burner, as will be noticed from the accompanying illustration, is a lamp of a quite different character from the one just described, burning as it does within an inclosing glass, the previous lamp being quite open to the air. The lamps exhibited consumed from 7 to 8 feet of gas each per hour, and are of various ornamental forms. It consists simply of an ordinary bat's-wing burner supplied with hot air through perforated plates, which are heated by the waste heat from the products of combustion, and by radiant heat communicated to the perforated plates. The advantages of this form of lamp are those of construction, application, and economy. The principal parts of the regenerator consist of simple castings, whilst the only wearing part is the tip or burner, which is, as already stated, of the ordinary kind, and may be easily replaced at trifling cost. It can be fitted to the ceiling of a room like any other gas-lamp, or may be connected up to a chimney, so that the products of combustion may be withdrawn from the apartment. There is a provision for lighting this lamp without removing the glass globe, the glass being sufficiently far removed from the flame not to receive any

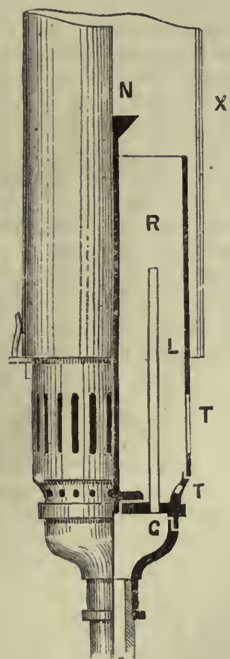


Fig. 3.

deposit upon its surface. With a consumption of 7·2 cubic feet of gas per hour, this lamp has been found to give without reflector a light equal to 72 sperm candles, or 10 candles per cubic foot, being more than three times the light produced by ordinary gas-burners, whilst if three flames are inclosed in the same lamp the efficiency obtained is still higher.

A third form of burner is Siemens's improved argand. This is not a regenerative gas-burner properly so called, and hence the economy is not so great as in either of the burners previously described. Instead of utilizing the waste heat of the products of combustion, in this burner the heat of the lower or non-luminous portion of the flame is applied for the purpose of heating up the air which is supplied to the burner. The sketch shows the arrangement in half-section. It consists of gas-chamber, G, and tubes, R, from which the gas issues and is burnt; a metal stem, N, rises a certain height above the top of the gas-tubes, serving the double purpose of improving the form of the flame and conducting a certain amount of heat down to assist in heating the air supplied to the burner. The air enters through the slots T, in the lower portion of the cylindrical case L, which surrounds it, a hot chamber being thus formed, from which the heated air passes to the flame. A glass chimney, X, incloses the flame as in the ordinary argand burner.

By means of this lamp an intense white light is produced with some economy of gas, the light produced with 6 cubic feet of gas being 26 candles, or 4·33 candles per cubic foot per hour, as compared with 3·2 in the ordinary form of argand burner. When an opal glass shade or reflector is used, throwing down a portion of the light, this burner gives a light of 6·33 candles per cubic foot. Its applications are various, but it is mainly applied for reading and desk purposes.

Mr. Siemens, in reply to a vote of thanks, said that the only economical way of burning gas was with the application of regenerators. This had already been proved by both the late Sir William Siemens and Mr. Frederick Siemens as regards furnaces for industrial purposes, and it is now being exemplified by Mr. Frederick Siemens in the domestic applications of gas.

NOTES.

The University of Cambridge has sustained a severe loss by the death of Mr. Coutts Trotter. He died on Sunday morning last. Next week we shall give some account of his services to his University and to science.

The United States Chief Signal Office has suppressed both its mountain stations, Pike's Peak and Mount Washington. The latter was suppressed at Michaelmas. The grounds alleged are—the reduction of the grant by Congress, which has been very serious, and, further, inability to use the reports in forecasting.

The Chief Signal Officer (Washington) has issued a circular, dated November 10 last, stating that, in view of the large number of letters he has received deprecating the discontinuance on January 1, 1888, of the International Meteorological Observations (see Nature, vol. xxxvi. p. 545), he has decided to continue to receive such observations, made at noon, Greenwich time, after that date. He does not promise to publish them as regularly as heretofore, but he will do what he can to give observers some return for their labours in the interests of the science of meteorology.

The *Annalen der Hydrographie und Maritimen Meteorologie* for November contains the first part of the explanatory text of the daily synoptic charts of the North Atlantic Ocean for the winter quarter of 1883-84, together with charts showing the positions of the principal barometric maxima and maxima (see Nature, vol. xxxvi. p. 159). The depressions of January 22-31 are of especial interest, as they include the lowest barometrical reading ever recorded in Europe, viz. 27·332 inches at Ochtertyre, near Crieff, N.B., on January 26, 1884. The readings nearest to this are 27·33 inches, about 6° further south in the Atlantic, on February 5, 1870, and even 27·245 inches in Iceland on February 4, 1824. A still lower reading has lately been quoted for False Point (Nature, November 17, p. 68). The storm of January 26-27 was also remarkable for the rapid fall before, and the rapid rise after, the minimum pressure.

The Monthly Weather Charts of the Bay of Bengal and adjacent sea north of the equator, recently published by the Meteorological Department of India, very clearly illustrate the distribution of pressure, wind, and currents, as well as the changes of the monsoons, in those parts. The charts have been prepared from data for the years 1855-78, and supplied by the Meteorological Council, at the expense of the Indian Office. Each chart is accompanied by explanatory text.

LAST Saturday there was a severe earthquake in Calabria. Two shocks were felt: one at 5 o'clock in the morning, the other two hours later. Both shocks were felt all over the province of Cosenza, but the second was by far the most violent. All the signalmen's huts on the railway near Sibari were destroyed for a distance of 8 kilometres. The station of Lattarico was also destroyed. At Paola the barracks and the Prefecture and Communal buildings were damaged; at Rogliano and Gravina several houses fell, and all the others were seriously shaken; and at San Marco part of the monastery fell. The results were most disastrous at Bisignano, the greater part of which was destroyed. More than twenty persons were killed, and about seventy injured. The parish priest of Bisignano, after having made his escape from the church, re-entered it, when the building fell in, and he was killed. The results at Bisignano would have been even more terrible, had not most of the inhabitants, alarmed by the first shock, fled from their houses. It is said that 900 houses are in ruins.

A CORRESPONDENT writes to us from Blackburn:—"A shock of earthquake occurred at Chorley, Lancashire, on December 1, at about 10 minutes to 7 o'clock a.m. It was also felt over a wide area. At Blackburn, two distinct vibrations were felt. The direction of the disturbance appeared to be from south-west to north-east."

ON November 5, at 7.16 p.m., a severe shock of earthquake was felt at Bodö, on the north-west coast of Norway. Houses shook, and several objects on walls fell down. There was only one shock, and it lasted about half a second.

ON the evening of November 21, at 5.18 p.m., a magnificent meteor was observed in the neighbourhood of Stavanger, on the west coast of Norway. It first appeared in the western sky, and, having described a semicircle, disappeared below the horizon. Its size was that of a child's head, and its light a brilliant white. The weather was fine and starry at the time.

MR. JOHN AITKEN has contributed to the Proceedings of the Royal Society of Edinburgh an interesting note on the formation of hoar-frost. Experiments were made with a sheet of glass exposed horizontally near the ground. During the deposition of dew the windward edges were generally dry, because the air has to travel over the cold plate before its temperature is reduced to the dew-point; but, when hoar-frost is deposited, the windward edges of the plate have the heaviest deposit. In this case the air seems to act as if it were supersaturated. Although this is impossible in ordinary conditions, the author shows that, if we have a water suface and an ice one at the same temperature, the vapour will tend to pass from the water to the ice, because the vapour-pressure of water is the higher; and he concludes that something like this takes place when hoar-frost is forming, the air which is saturated to a water surface being supersaturated to an ice one.

DR. FRIDTJOF NANSEN, of the Bergen Museum, has announced his intention of attempting to cross the interior of Greenland next summer on *Ski*, viz. the snow-runners found so advantageous during the last Nordenskiöld expedition across that continent. It may be remembered what extraordinary progress the Lapps made at that time on these Scandinavian means of locomotion across snow-fields. Dr. Nansen, who has on a former occasion visited the inland ice in Greenland, has placed his plan before Baron Nordenskiöld, who fully believes in its realization, and is giving Dr. Nansen every assistance. The explorer purposes crossing from the east to the west coast, the reverse of Baron Nordenskiöld's attempt.

AN important paper by Prof. Lothar Meyer, upon the subject of "oxygen carriers," will be found in the current number of the *Berichte*. It embodies the results of a systematic series of experiments in which currents of oxygen and sulphur dioxide gases were simultaneously passed for some hours through solutions of certain salts of known strength contained in flasks heated upon the water-bath. At the end of each experiment the sulphur dioxide remaining in solution was expelled by a current of carbon dioxide, and finally a determination was made of the amount of sulphuric acid formed by oxidation of the sulphur dioxide. The results show that the salts of certain metals exert a most remarkable action in causing the union of oxygen with sulphur dioxide. The most active of all is manganous sulphate, $MnSO_4 . 5H_2O$, 2·4 grammes of which, dissolved in 200 c.c. of water, caused the formation of no less than six times as much sulphuric acid as that originally contained in the salt; that is, for every molecule of the sulphate employed, five molecules of free acid were synthesized. Manganese chloride under like circumstances was also found to act as an energetic oxygen carrier, one molecule of $MnCl_2 . 4H_2O$ causing the formation of 4·3 molecules of free sulphuric acid. Copper salts were next experimented upon, and a 3 per cent. solution of the sulphate, $CuSO_4 . 5H_2O$, was found to be most effective, one molecule causing the production of about a molecule of the acid. Both cuprous and cupric chlorides, the former in spite of its insolubility, act even more energetically than the sulphate, while the oxide hydrate, and, metal itself also work in a lesser degree. In a similar manner salts of iron, cobalt, nickel, zinc, cadmium, and magnesium were found capable of causing the oxidation of sulphurous acid, while salts of thallium and potassium merely acted like pure water, being absolutely powerless in this respect. These remarkable results are due, in the opinion of Prof. Meyer, to alternate oxidations and reductions, and this is certainly very strongly supported by the fact that those metals act most powerfully which readily pass from one stage of oxidation to another. As zinc, cadmium, and magnesium are also found to act in this manner, it is presumed that these metals have also an inclination to form sub-salts which have never yet been prepared.

SOME days ago a peasant ploughing at Tjöring, in Denmark, unearthed a handsome armlet of pure gold weighing 12 ounces, which, according to the Director of the Museum of Antiquities in Copenhagen, dates from the second or third century A.D. There was formerly a barrow in the field where the armlet was found, and flint implements, broken pottery containing decayed bones, &c., have frequently been brought to light; but all traces of the barrow have now disappeared through ploughing.

IT is reported from India that Mr. Rea, of the Madras Archæological Survey, has recently excavated some ancient burial-places at Dadampatti, Paravai, and other places in the Presidency, and investigated the cromlechs near Kodaikanaul. He has obtained a considerable collection of ancient pottery, and in some of the tombs found a large number of bones and a complete human skull. The latter had been filled up and inclosed in soft clay, so that its contour and characteristics are perfectly preserved. Mr. Rea also brought away a small specimen of a pyriform tomb.

LAST Thursday, Sir John Lubbock read a paper before the Linnean Society, in continuation of his previous memoirs, on "The Habits of Ants, Bees, and Wasps." He said it was generally stated that our English slave-making ant (*Formica sanguinea*), far from being entirely dependent on slaves, as was the case with *Polyergus rufescens*, the slave-making ant *par excellence*, was really able to live alone, and that the slaves were only, so to say, a luxury. Some of his observations appeared to throw doubt on this. In one of his nests the ants were prevented from making any fresh capture of slaves. Under these circumstances, the number of slaves gradually diminished, and at length the last died. At that time there were some fifty of the mistresses still remaining. These, however, rapidly died

off, until at the end of June 1886 there were only six remaining. He then placed near the door of the nest some pupæ of *Formica fusca*, the slave ant. These were at once carried in and soon came to maturity. The mortality among the mistresses at once ceased, and from that day to this only two more have died. This seems to show that the slaves perform some indispensable function in the nest, though what that is still remains to be discovered. As regards the longevity of ants, he said that the old queen ant, which had more than once been mentioned to the Society, was still alive. She must now be fourteen years old, and still laid fertile eggs, to the important physiological bearing of which fact he called special attention. He discussed the observations and remarks of Graber as regards the senses of ants, with special reference to their sensibility towards the ultra-violet rays, and referred to the observations of Forel, which confirmed those he had previously laid before the Society. Prof. Graber had also questioned some experiments with reference to smell. He, however, maintained the accuracy of his observations, and pointed out that Graber had overlooked some of the precautions which he had taken; his experiments seemed to leave no doubt as to the existence of a delicate sense of smell among ants. As regards the recognition of friends, he repeated some previous experiments with the same results. He took some pupæ from one of his nests (A) and placed these under charge of some ants from another nest (B) of the same speciés. After they had come to maturity, he placed some in nest A and some in nest B. Those placed in their own nest were received amicably, those in the nests of their nurses were attacked and driven out. This showed that the recognition is not by the means of a sign or password, for in that case they would have been recognized in nest B and not in nest A. Dr. Warsmann had confirmed his observations in opposition to the statement of Lespis, that white ants are enemies to those of another nest, even belonging to the same species; the domestic animals, on the other hand, can be transferred from one nest to another, and will be amicably received. In conclusion, he discussed the respective functions of the eyes and ocelli, and referred to several other observations on various interesting points in the economy of the Social Hymenoptera.

In an interesting paper read the other day before the National Academy of Sciences, New York, Prof. W. P. Trowbridge gave an account of a discovery which had lately been made by his son. This discovery is that birds of prey and some others have the power to lock securely together those parts of the wing holding the extended feathers, and corresponding to the fingers of the human hand. The action of the air on the wing in this condition extends the elbow, which is prevented from opening too far by a cartilage, and the wings may keep this position for an indefinite length of time, with no muscular action whatever on the part of the bird. While resting in this way, the bird cannot rise in a still atmosphere; but, if there be a horizontal current, it may allow itself to be carried along by it, with a slight tendency downward, and so gain a momentum by which, with a slight change of direction, it may rise to some extent, still without muscular action of the wings. Prof. Trowbridge also believed it quite possible for a bird to sleep on the wing. In discussing this paper, Prof. J. S. Newberry said that he had once shot a bird which came slowly to the ground as if still flying, but reached it dead. He believed that it had died high in the air; but he had never been able to account for the manner of its descent till now, when he found an explanation in the statement of Prof. Trowbridge.

The cultivation of oysters in France appears to have greatly increased of late. Thus, while in 1885 the number exported was 30,000,000, 35,000,000 have been exported in the first eight months of 1887 (twice as much as in the corresponding part of 1886), and the total for the year will probably be about 52,000,000. At the same time the importation into France from Portugal has been declining. Thus, from 154,647 kilogrammes in 1883, it had fallen to 1500 kilogrammes in 1885, and no figures are forthcoming for the first eight months of 1887.

In his Report for 1886–87, presented to the Parliament of Tasmania, Mr. Saville-Kent speaks of the oyster-fisheries on the Tasmanian coast-line. The results obtained during the past year, from the series of Government oyster-reserves established in accordance with Mr. Saville-Kent's recommendations, seem to him to justify the opinion that, with an extension of the same system, conducted on scientific principles, the produce of these reserves, combined with that raised on the private beds, will be sufficient within the course of a few years to establish once more a lucrative oyster trade in the colony. At all of the several reserves there has been an abundant fall of spat, but more especially in those of the Spring Bay district. This locality, Mr. Saville-Kent anticipates, will, as in former years, become the chief station of the Tasmanian oyster-fishery. The number of breeding-oysters at present laid down upon the various Government reserves may be reckoned at about 150,000; to these may be added, as the produce of the past year's spatting season, at least an equal number of young brood. A further supply of 100,000 adult stock, for placing on the additional reserves projected or in course of construction, will be obtained from the natural beds during the current year.

Mr. Harry Page Woodward (eldest son of Dr. Henry Woodward, F.R.S.), who had served for more than three years, under Mr. H. Y. Lyell Brown, as Assistant Government Geologist in South Australia, has, by the advice and upon the recommendation of Dr. A. Geikie, F.R.S., Director-General of the Geological Survey of Great Britain, been appointed by the Secretary of State for the Colonies to the post of Government Geologist for Western Australia. Mr. Woodward sailed for King George's Sound in the P. and O. steam-ship *Shannon* on the 2nd inst.

A peculiar phenomenon is being noticed in the large lakes near the village of Mazuren (near Gumbinnen, Prussia). The level of the water is continually decreasing; during the last ten years it has fallen 1 metre annually, so that many of the islands in the lakes have now become peninsulas.

The People's Lectures, begun under the auspices of the London Society for the Extension of University Teaching, have attracted large audiences, and there is no reason to doubt that the success hitherto achieved will be maintained. Yesterday evening, Prof. H. G. Seeley, F.R.S., delivered, at the Great Assembly Hall, Mile End Road, the first of a course of three lectures on "Glimpses into Nature's Workshop." The special subject of this lecture was "Water, the Earth Leveller." The next two lectures of the course—"Ice, the Earth Engraver," and "Underground Heat, the Earth Moulder and Modeller"—will be delivered on December 14 and 21, at the Memorial Hall, London Street, Bethnal Green.

Another series of lectures at the Memorial Hall, London Street, Bethnal Green, is likely to be of good service. It is intended especially for working lads, and the lectures are called "Science Talks." Last Thursday, Dr. Gerard Smith delivered a lecture on "The Structure of Trees and Plants"; and this evening he will lecture again, taking as his subject "Microscopic Life in the Sea." On December 15, Mr. C. A. Newton will lecture on "The Wonders of the Heavens."

A Conference on Technical Education will be held at the Royal Victoria Hall, Waterloo Bridge Road, on Wednesday, the 14th inst. Sir Henry Doulton will take the chair. Two short papers—one by Dr. Fleming, of University College, the other

by Mr. Bochett, a working man—will be read; and it has been arranged that the reading of these papers shall be followed by a discussion. It is hoped that employers and employed will both be largely represented at the meeting. The Hon. Secretary will be glad to send tickets for the platform or for reserved seats to anyone who may apply for them.

IN the Report of the Newcastle Public Libraries Committee for 1886–87, it is stated that, at the annual stock-taking in June-July 1886, only three volumes were found to be unaccounted for. Only sixteen volumes have been lost since the opening of the Library in 1880. During the same period the issue of v olumes has reached a total of 1,538,445.

A NEW edition of the catalogue of books in the juvenile lending department connected with Newcastle Public Library has just been issued. A glance at the contents, as the compiler truly says, will show that in this juvenile department "a wonderful wealth of entertainment is placed at the command of the young people of Newcastle." No fewer than two thousand carefully selected volumes are at their disposal. During the seven years the Library has been open, the Committee has more than doubled the stock of books in this collection, and 215,092 volumes have been lent to children.

A VISITOR to the beaver colony at Amlid, some distance from Christiansand, in Norway, to which we referred some months ago, states that the colony has flourished considerably during the summer, and is now probably the largest in Norway. Sometimes as many as a dozen animals may be seen at a time in the water. The huts are built close to the shore, and have two stories, one above and one below the surface of the water. The walls are made of timber, laid as in a human dwelling, whilst the roof is covered with twigs and mud. All the aspen-trees in the vicinity have now been felled, and the animals have begun to attack the birches. Trees upwards of 18 inches in diameter at the root have been cut down. The animals appear to have most use for the branches, many stems stripped of the same lying about in the woods. The material required is dragged to the waterside along regular "log runs," su ch as wood-cutters leave in forests, and in some places roots crossing the same have been gnawed off, so as to make the run smooth. Shortly after an increase in the colony the new-comers begin to build a new house. Not one of the animals has as yet been killed, and visitors come from all parts for the purpose of watching their peculiar mode of living. It has been found that sentinels are posted, giving the alarm to the rest of the colony in case of danger. When such an alarm is given, all the animals leave their dwellings for the water.

READERS of Icelandic Sagas will remember that in the celebrated Njal's Saga there is a record of an attack on Njal's dwelling, Bergthorshval (named after his wife, Bergthora), and of its being burned, with the whole of Njal's kin. In order to demonstrate the historical accuracy of the Saga, a member of the Iceland Archæological Society some two years ago proposed to excavate the spot where Njal's dwelling was said to have stood. This was done last year, and resulted in the discovery, at a depth of some 6 feet, of a layer of ashes, remains of charred beams, &c. But this was not all. Below the ashes three lumps of some substance of a spongy nature, dirty-white in colour, were found; and Dr. Storch, Director of the Royal Agricultural Laboratory in Copenhagen, by whom these lumps have just been analyzed, pronounces them to be ancient curdled milk and cheese. Such milk, called *Skyr*, was much liked in Iceland in remote times, and was often solidified to a kind of cheese by the fluid matter being pressed out. Strangely enough, the Saga mentions the fact of women bringing *Skyr* to extinguish the fire. Dr. Storch, by slowly treating fresh *Skyr* to a temperature of a little more than 100° C., has thereby obtained a substance in every respect similar to that found in the supposed ruins of Njal's dwelling.

THE additions to the Zoological Society's Gardens during the past week include a Striped Hyæna (*Hyæna striata*) from North Africa, presented by Mr. Ernest Heydon Marquis; a Crested Porcupine (*Hystrix cristata*) from Suakim, presented by His Grace the Duke of Hamilton, K.T., F.Z.S.; two Common Squirrels (*Sciurus vulgaris*), British, presented by Mrs. Henry Alex. Hankey; a Horned Tragopan (*Ceriornis satyra* ♂) from the South-eastern Himalayas, presented by Mr. R. J. Lloyd Price; a Vinaceous Dove (*Turtur vinaceus*) from West Africa, presented by Mr. R. H. Mitford; three South African Scorpions (*Scorpio* ——) from South Africa, presented by Mr. W. K. Sibley; a Zebu (*Bos indicus*) from Africa, two Sandwich Island Geese (*Bernicla sandvicensis*) from the Sandwich Islands, deposited.

OUR ASTRONOMICAL COLUMN.

THE NEW ALGOL VARIABLES, Y CYGNI AND R CANIS MAJORIS.—Mr. Chandler has just published in *Gould's Astronomical Journal*, No. 163, his elements for these two interesting variables. In the case of Y Cygni, it will be recollected (see NATURE, vol. xxxv. pp. 307, 329) that before its period had been fully determined by observation, Mr. Chandler concluded, from the analogy of all the then known stars of the type, that it would prove to be about thirty-six hours. This is now found to be correct, the actual period being 1d. 11h. 56m. 48s. The ground upon which the inference was based was the circumstance that with the other stars of the type the shorter the period of the star the higher is the ratio which the time of oscillation bears to the entire period. The first exception to this rule is R Canis Majoris, the variable star discovered by Mr. Sawyer last March (see NATURE, vol. xxxvi. p. 376), the duration of the oscillation for this star being 5h. instead of 6h., as it should be on the same principle.

The following are the elements of the two stars:—

	Y Cygni.	R Canis Majoris.
Epoch	1886, Dec. 9, 11h. 14m. 30s.	1887, Mar. 26, 14h. 58m. 30s.
Period ...	1d. 11h. 56m. 48s.	1d. 3h. 15m. 55s.
Brightness at maximum	7·1m. ...	5·9m.
Brightness at minimum	7·9m. ...	6·7m.
Duration of decrease ...	4h. ...	2·5h.
Duration of increase ...	4h. ...	2·5h.
Stationary maximum brilliancy	28h. ...	22h.

MINOR PLANET NO. 271.—This object has received the name of Penthesilea.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *DECEMBER* 11–17.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on December 11

Sun rises, 7h. 58m.; souths, 11h. 53m. 22·9s.; sets, 15h. 49m.: right asc. on meridian, 17h. 12·9m.; decl. 23° 1′ S. Sidereal Time at Sunset, 21h. 9m.

Moon (New on December 14, 19h.) rises, 3h. 11m.; souths, 8h. 47m.; sets, 14h. 14m.: right asc. on meridian, 14h. 6·5m.; decl. 7° 27′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	6 7 ...	10 3[illegible] ...	14 57 ...	15 51·2 ...	18 26 S.
Venus.....	3 33 ...	8 46 ...	13 59 ...	14 4·9 ...	9 56 S.
Mars......	0 47 ...	6 54 ...	13 1 ...	12 12·5 ...	0 44 N.
Jupiter....	5 37 ...	10 6 ...	14 35 ...	15 24·9 ...	17 47 S.
Saturn....	19 28* ...	3 16 ...	11 4 ...	8 34·1 ...	19 10 N.
Uranus...	2 9 ...	7 43 ...	13 17 ...	13 1·6 ...	5 52 S.
Neptune..	14 44 ...	22 24 ..	6 4* ...	3 45·0 ...	18 3 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Dec.	h.	
11 ...	8 ...	Venus in conjunction with and 2° 37′ south of the Moon.
12 ...	11 ...	Venus at least distance from the Sun.
12 ...	18 ...	Jupiter in conjunction with and 4° 16′ south of the Moon.
13 ...	9 ...	Mercury in conjunction with and 3° 24′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·3 ...	81 16 N. ...	Dec. 12,	0 25 *m*
			,, 17,	0 5 *m*
Algol	3 0·8 ...	40 31 N. ...	,, 11,	20 23 *m*
λ Tauri...	3 54·4 ...	12 10 N. ...	,, 11,	1 9 *m*
			,, 15,	0 2 *m*
ζ Geminorum ...	6 57·4 ...	20 44 N. ...	,, 14,	19 0 *M*
R Canis Majoris...	7 14·3 ...	16 11 S. ...	,, 11,	3 28 *m*
			,, 17,	23 3 *m*
U Coronæ	15 13·6 ...	32 4 N. ...	,, 14,	20 47 *m*
R Scuti	18 41·5 ...	5 50 S. ...	,, 12,	*M*
β Lyræ...	18 45·9 ...	33 14 N. ...	,, 14,	20 0 *M*
Y Cygni	20 46·6 ...	34 10 N. ...	,, 14,	22 4 *m*
			,, 17,	21 58 *m*
δ Cephei	22 25·0 ...	57 50 N. ...	,, 16,	2 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near Pollux	117° ...	31° N. ...	Rather swift.
From Leo Minor ...	143 ...	39 N. ...	Swift; streaks.
Near λ Draconis ...	158 ...	72 N.	

M. POTANIN'S JOURNEYS IN EAST TIBET AND EAST GOBI.

A CONDENSED report of the results obtained by the three years' journey of MM. Potanin, Skassy, and Berezovsky, in China, Amdo plateau of Tibet at the sources of the Hoang-ho, and East Gobi, has just appeared in the Russian *Izvestia* of the Geographical Society (iii. 1887.) Without repeating what has already been mentioned in his letters, M. Potanin gives in his paper a masterly sketch of the physical characteristics of the various regions explored by his expedition.

The route followed was from Peking, across the Utai-shan mountains which border the Peking depression in the west, and where the well-known Utai Buddhist monasteries are situated, to the city of Kuku-khoto. Thence south, across the Ordos region, to Lan-tcheu, capital of the Han-su province, and to San-tchuan on the middle Hoang-ho, where M. Potanin spent the winter of 1884–85, while M. Skassy wintered at the above city, and M. Berezovsky at Hoi-siang, on the Sy-tchuan frontier of the Han-su province. Thence the expedition proceeded south-east tp Min-tcheu on the Tao-he, and to Sun-pan. Lun-an-fu was the utmost point reached towards the south, and the expedition returned to Lan-tcheu to spend the second winter at the Humbum monastery, close by Si-nin. The third summer was spent for the return journey, which was made *viâ* Kuku-nor, across the mountains which separate the Tsaidam from the Mongolian plateau, and the cities of Han-tcheu and Su-tcheu. Then, taking a course due north, the expedition crossed the Gobi, as also several ridges continuing the Ek-tag Altai in the east, and the Hanghai ridge, and reached the Orkhon River, whence it proceeded to Kiakhta and across Siberia to Russia.

The Peking plain, covered with fertile loess, is separated by a series of three ridges built up of gneisses and limestones, from the plateau of the Ordos, watered by the middle Hoang-ho. Of Europeans, only M. Przewalski, the missionary Huc, and M. Potanin's expedition have visited the Ordos—a plateau about 3300 feet high, covered with shifting sands, the best part of which is on their eastern border. Owing to the moistness brought by the numerous streams which flow towards the Hoang-ho, the sands on the eastern border are not so bad as those described further west by M. Przewalski, and the *barkhans* are covered with bushes of *Shyavyk*, *Artemisiæ*, *Hedysarum lævi*, and thickets of the *Pugionium cornutum*—a new shrub discovered by Przewalski; sometimes dark growths of Thuja cover the *barkhans*. The hollows between the sandy hills are either covered with some bushes or occupied by the fields of the Mongols, who chiefly grow *setaria*, buckwheat, and hemp. The wet depressions, covered by meadow-grasses and partly with Halophytes, and called *tchaidams*, are enlivened by the herds and the mud huts of the half-nomadic Mongols. The sands are steadily moved by the winds from the south-west towards the north-east, and this constant motion explains why the Chinese gave to the sand-desert the name of Sha-he, or "River of Sand."

In the highlands which connect the Tibet mountains with those of Shan-si the expedition spent fifty days. Thick layers of loess cover there the horizontal layers of salt-bearing sandstones and conglomerates. The region is a bigh plateau deeply burrowed by the *cañons* of the rivers, which sometimes are 2000 feet deep, and are cut both through the loess and the sandstones. The narrow *cañons* are mostly waterless, while the broader ravines are watered by rivers and therefore are the seat of many villages. There is little wind or rain, and the atmosphere is charged with dust.

In Tibet the expedition crossed only the Amdo plateau, separated from the Mongolian plateau by the Nan-shan ridge. For 400 miles the expedition crossed there a region the lowest parts of which rise above 7000 and 8000 feet. Even the Hoang-ho at Gui-dui has an altitude of 7600 feet, and the valley of the E-tsin at the Pabor-ta-sy monastery is 8000 feet high; the valleys of the Urunvu and the Tumun-guan are at altitudes of from more than 9000 to 10,000 feet. The highest parts of the plateau rise, however, to 12,000 feet, and Lake Kuku-nor is spreading its waters at the height of Alpine peaks, *i.e.* 10,700 feet. Still higher grassy plateaus, where it never rains but often snows, and marshes spread over large areas, rise to the south of the lake. Only a few of the mountain-ridges which inclose this plateau are snow-clad. It has a quite original flora, discovered by General Przewalski. Forests are few; as to the high meadows, they are inhabited by nomad Tangutes, and, on lower levels, by a mixed population of Chinese and settled Mongols described under the name of Daldas.

The Alpine highlands watered by the northern tributaries of the Blue River, which separate the Amdo high plateau from the Chinese lowlands, are the most picturesque part of China. The routes which cannot follow the bottoms of the narrow and rocky valleys pass over the mountains, flights of steps being cut in the rocks, or wooden balconies being built along the steep slopes of the rocky hills. Suspended bridges, swinging under the weight of a mule, cross streams which flow in a succession of rapids and waterfalls. The Chinese monsoons deposit all their moistness on the south-eastern slopes of the mountains; thick forests, of Conifers on higher levels and of deciduous trees lower down, clothe the mountain slopes. Maples, lime-trees, oaks, *Helwingiæ*, and a number of shrubs and climbing plants are growing in impracticable thickets, while all crags are thickly covered with ferns, mosses, and orchids. Mollusks (*Bulymus* and *Helix*) cover the crags by thousands. And finally at the foot of the mountains the sub-tropical flora—palms, bamboos, banana-trees, and tea-trees—makes its appearance.

The villages and the towns—clean and well-watered—are strikingly picturesque, as the houses (with windows, like our European dwellings) are built in the shape of amphitheatres on the slopes of the steep forest-clothed hills. In some towns the roofs of the houses are the workshops and sitting-places of the inhabitants. The valley of the "Golden Lakes"—Kser-ntso—with its background of snowy peaks is especially picturesque.

As to the region crossed between the Amdo plateau and Kiakhta, it is sharply divided into two parts. The southern is a true desert, which stretches towards the north as far as the Khangai Mountains. The Nan-shan rises as an immense snow-clad wall on its southern border; then comes a narrow strip of inhabited and cultivated land, which is followed by a gravelly desert, where only a few trees of *Haloxylon Ammodendron*, and bushes of *Calligonum* and *Ephedra* grow here and there, while the course of the E-tsin is marked by narrow strips of meadows covered with *Elymus*. The depression of the E-tsin, which flows into the Gashiun-nor, has an altitude of only about 3000 feet, and it is bordered in the north by the Tostu ridge, and three other parallel ridges, of which the northern is snow-clad. The valleys which separate these four ridges are waterless; old river-beds, now dry, are seen on their bottoms, but even the *Haloxylon* forests which formerly grew in their valleys are now disappearing, only decayed trees having been seen by the expedition.

As to the plateau in the north of the Khangai Mountains, it is covered with rich meadows, while the slopes of the hills are clothed with forests of larch; the Siberian cedar-tree also makes its appearance. In the lower valleys the Mongols carry on some agriculture.

The above account is followed by an ethnographical sketch of the Ordos-Mongols and the Daldas.

The results obtained by the expedition are very important. A survey has been made of a stretch of no less than 4400 miles. Latitudes and longitudes have been determined at sixty-nine places. Two hundred photographs, 700 specimens of mammals and birds, a bulky herbarium, and rich collections of lizards, insects, mollusks, and rocks have been brought in. M. Berezovsky still remains in the region he has become so fond of, and he wrote last February, from Hoi-siang, that his journeys about Si-ning and Tai-tchan have enriched his collection with 500 more specimens of birds, some of which are very interesting.

P. A. K.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 17.—"Specific Inductive Capacity." By J. Hopkinson, M.A., D.Sc., F.R.S.

Colza Oil.—This oil has been found not to insulate sufficiently well for a test by the method of my former paper. Most samples, however, were sufficiently insulating for the present method. Seven samples were tested with the following mean results:—

No. 1. This oil was kindly procured direct from Italy for these experiments by Mr. J. C. Field, and was tested as supplied to me—

$$K = 3 \cdot 10.$$

No. 2 was purchased from Mr. Sugg, and tested as supplied—

$$K = 3 \cdot 14.$$

No. 3 was purchased from Messrs. Griffin, and was dried over anhydrous copper sulphate—

$$K = 3 \cdot 23.$$

No. 4 was refined rape oil purchased from Messrs. Pinchin and Johnson, and tested as supplied—

$$K = 3 \cdot 08.$$

No. 5 was the same oil as No. 4, but dried over anhydrous copper sulphate—

$$K = 3 \cdot 07.$$

No. 6 was unrefined rape purchased from Messrs. Pinchin and Johnson, and tested as supplied, the insulation being bad, but still not so bad as to prevent testing—

$$K = 3 \cdot 12.$$

No. 7. The same oil dried over sulphate of copper—

$$K = 3 \cdot 09.$$

Omitting No. 3, which I cannot indeed say of my own knowledge was pure colza oil at all, we may, I think, conclude that the specific inductive capacity of colza oil lies between 3·07 and 3·14.

Prof. Quincke gives 2·385 for the method of attraction between the plates of a condenser, 3·296 for the method of lateral compression of a bubble of gas. Palaz (*La Lumière Électrique*, vol. xxi. 1886, p. 97) gives 3·027.

Olive Oil.—The sample was supplied me by Mr. J. C. Field—

$$K = 3 \cdot 15.$$

The result I obtained by another method in 1880 was 3·16.

Two other oils were supplied to me by Mr. J. C. Field.

Arachide.—K = 3·17.

Sesame.—K = 3·17.

A commercial sample of *raw linseed oil* gave K = 3·37.

Two samples of *castor oil* were tried: one newly purchased gave K = 4·82; the other had been in the laboratory a long time, and was dried over copper sulphate—

$$K = 4 \cdot 84.$$

The result of my earlier experiments for castor oil was 4·78; the result obtained subsequently by Cohen and Arons (*Wiedemann's Annalen*, vol. xxviii. p. 474) is 4·43. Palaz gives 4·610.

Ether.—This substance as purchased, reputed chemically pure, does not insulate sufficiently well for experiment. I placed a sample, purchased from Hopkin and Williams as pure, over quicklime, and then tested it. At first it insulated fairly well, and gave K = 4·75. In the course of a very few minutes K = 4·93, the insulation having declined so that observation was doubtful. After the lapse of a few minutes more observations became impossible. Prof. Quincke in his first paper gives 4·623 and 4·660, and 4·394 in his second paper.

Bisulphide of Carbon.—The sample was purchased from Hopkin and Williams, and tested as it was received—

$$K = 2 \cdot 67.$$

Prof. Quincke finds 2·669 and 2·743 in his first paper, and 2·623 in his second. Palaz gives 2·609.

Amylene.—Purchased from Burgoyne and Company—

$$K = 2 \cdot 05.$$

The refractive (μ) index for line D is 1·3800,

$$\mu^2 = 1 \cdot 9044.$$

Of the benzol series four were tested: *benzol*, *toluol*, *xylol*, obtained from Hopkin and Williams, *cymol* from Burgoyne and Company.

In the following table the first column gives my own results, the second those of Palaz, the third my own determinations of the refractive index for line D at a temperature of 17°·5 C., and the fourth the square of the refractive index:—

			μ	μ^2
Benzol	2·38	2·338	1·5038	2·2614
Toluol	2·42	2·365	1·4990	2·2470
Xylol	2·39	—	1·4913	2·2238
Cymol	2·25	—	1·4918	2·2254

For benzol Silow found 2·25, and Quincke finds 2·374.

Linnean Society, November 17.—Prof. St. George Mivart, F.R.S., Vice-President, in the chair.—Mr. A. Bennett drew attention to new British plants, viz. (1) *Arabis alpina*, gathered on the Cuchillin Mountains, Isle of Skye; (2) *Juncus alpina*, obtained in Perthshire; and (3) *Juncus tenuis*, got near Galloway, Kirkcudbrightshire.—Mr. W. H. Beeby made remarks on *Carex cæspitosa* from Shetland.—Photographs of a branched palm (*Borassus flabelliformis*) was shown for Surgeon-General Bidie, of Madras, and a letter thereon read. The tree is growing near Tanjore, at a village named Paducottah, and is remarkable in being divided into eight branches.—Mr. W. Wilson sent for exhibition branches with ripe berries of *Taxus baccata*, and its variety *hybernica*, produced by natural cross-fertilization: these were grown in Central Aberdeenshire.—Mr. T. Christy showed a new species of *Strophanthus* from the Niger; it is distinguished by its brown velvety seed and intensely bitter taste.—Mr. D. Morris exhibited the following specimens: (1) a fibre from Vera Cruz, named Broom Root, which examination showed to be the root fibres of *Epicampus macroura*, known as "Raviz de Zacaton" by the Mexicans, its yearly value in export is £60,000; (2) another Mexican fibre, "Ixtli," much used for nail-brushes, &c., in Britain, by reason of its short tough fibre, is found by the Kew authorities to be derived from *Agave heteracantha.*—Mr. J. G. Baker showed *Lycopodium albidum*, a new species from the Andes of Ecuador; it is allied to *L. clavatum*, but without chlorophyll except at the base. He also showed *Neobaronia xiphoclades*, a new Papilionaceous plant from Madagascar, obtained by the Rev. R. Baron.—A paper was read by Mr. P. Geddes, on certain factors of variation in plants and animals.—Then followed a paper on the Copepoda of Madeira and the Canary Islands, by Mr. I. C. Thompson. In all, sixty-five species were obtained. Of these, six are new to science, and three probably of generic significance. Twenty-three are known in British waters, and of these fourteen belong to the family Harpacticidæ. There is a similarity in species in the different islands, but the numbers of each vary greatly.

Geological Society, November 9.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—Note on the so-called "Soapstone" of Fiji, by Henry B. Brady, F.R.S. The Suva deposit, which has a composition very similar to that of the volcanic muds at present forming around oceanic islands in the Pacific, is friable and easily disintegrated. The colour ranges from nearly white to dark gray, the mass being usually speckled with minerals of a darker hue. Under the microscope the rock presents the character of a fine siliceous mud with crystals of augite, &c.,

together with the sparsely scattered tests of Foraminifera. The approximate chemical composition of typical specimens is:—Silica, 50 per cent.; alumina, 18 per cent.; lime and magnesia, from 5 to 6 per cent.; ferric oxide, from 3 to 8 per cent.; water, 16 per cent., with a small proportion of alkalies, chiefly potash, and but small trace of carbonates. The author's attention was chiefly directed to the common gray friable rocks which may be softened in water and washed on a sieve, the residue consisting mainly of Foraminifera with a few Ostracoda. Of three specimens examined, (1) is a light-gray rock from close to the sea-level; (2) of a lighter colour, from about 100 feet elevation; (3) is nearly white and somewhat harder, and was derived from an intermediate point. So far as the Microzoa are concerned, the first two present no differences which might not be observed in dredgings from the recent sea-bottom, taken at similar depths a little distance apart. The third appears to have been deposited in somewhat deeper water. There is a marked scarcity of arenaceous Foraminifera. Then followed notes on the rarer and more interesting species, together with a list of the ninety-two species of Foraminifera found. Of these, eighty-seven are forms still living in the neighbourhood of the Pacific islands. Two of the remaining five are new to science, and the rest extremely rare. The author concluded that these deposits are of Post-Tertiary age, formed at depths of from 150 to 200 fathoms in the neighbourhood of a volcanic region. The following new or little-known species were selected for illustration:—*Ellipsoidina ellipsoides*, var. *oblonga*, Seguenza; *Haplophragmium rugosum*, D'Orb.; *Ehrenbergina bicornis*, nov.; *Sphæroidina ornata*, nov. The President hoped that this paper might be regarded as one of the first-fruits of travels undertaken by the author for the purpose of investigating the interesting deposits of this nature. Prof. Rupert Jones agreed that this was a valuable instalment of work to be expected. The peculiar Foraminifer specially mentioned by Mr. Brady (*Ellipsoidina ellipsoides*, var. *oblonga*, Seguenza) must have connections, so that, as the author has intimated, the interest attached to it was not yet wholly worked out.—On some results of pressure and of intrusive granite in stratified Palæozoic rocks near Morlaix, in Brittany, by Prof. T. G. Bonney, F.R.S.—On the position of the Obermittweida conglomerate, by Prof. T. McK. Hughes.—On the Obermittweida conglomerate: its composition and alteration, by Prof. T. G. Bonney, F.R.S.—Notes on a part of the Huronian series in the neighbourhood of Sudbury (Canada), by Prof. T. G. Bonney, F.R.S. The specimens noticed by the author were in part collected by him in the summer of 1884, when the Canada Pacific Railway was in process of construction, and in part subsequently supplied to him by the kindness of Dr. Selwyn, Director-General of the Geological Survey of Canada. The eastern edge of the district assigned to the Huronian consists of rocks, which may possibly be part of the Laurentian series modified by pressure. But after crossing a belt of these, barely a mile wide, there is no further room for doubt. All the rocks for many miles are distinctly fragmental, except certain intrusive diabases or diorites. These fragmental rocks are grits, conglomerates, and breccias, which are described as far as about two miles west of Sudbury. The included fragments in these rocks appear to have undergone some alterations subsequent to consolidation: these are described. In some cases the changes appear to be anterior to the formation of the fragments. The matrix also has undergone some change, chiefly the enlargement of quartz grains, and the development or completion of mica-flakes, as in the Obermittweida rock. The author gave some notes on other specimens collected by him along the railway, further west, and on those supplied to him from near Lake Huron by Dr. Selwyn. As a rule these are but little altered. Some contain fragments of igneous rocks, apparently lavas. The author discusses the significance of the changes in these rocks, as bearing on general questions of metamorphism, and states that, in his opinion, the name Huronian, at present, includes either a series of such great thickness that the lower beds are more highly altered than the higher, or else two distinct series; and he inclines to the latter view. Both, however, must be separated from the Laurentian by a great interval of time, and neither exhibits metamorphism comparable with that of a series of schists and gneisses, like the so-called Montalban. The newer reminds him often of the English Pebidians. After the reading of this paper there was a discussion, in which the President, Dr. Geikie, Mr. Rutley, and others took part.

Royal Meteorological Society, November 16.—Mr. W. Ellis, President, in the chair.—The following papers were read:—The use of the spectroscope as a hygrometer simplified and explained, by Mr. F. W. Cory. The object of this paper is to suggest as simple a way as possible of using the spectroscope as a hygrometer in order to facilitate its introduction amongst observers as a standard meteorological instrument. The best form of hygro-spectroscope as a recognized standard for the purpose of investigating and scrutinizing the changes of the three parts of the spectrum mentioned is that originally termed by Mr. Rand Capron "The Rainband Spectroscope." It ought to have a fixed slit, and in addition a milled wheel at the side for the easier adjustment of the focus. The author concludes by giving a set of hints to observers for taking weather observations with a pocket spectroscope.—Rainfall on and around Table Mountain, Cape Town, Cape Colony, by Mr. J. G. Gamble. The author calls attention to the great and in some respects peculiar differences that exist between the quantity of rain that is registered on and around Table Mountain. The most striking feature is the small fall on the signal hill. The signal hill, otherwise called "the Lion's Rump," lies to the west of Cape Town, between it and the Atlantic. The average annual fall there is only 15 inches, while the fall at the western foot is 21 inches, and in Cape Town 27 inches. The signal hill is 1143 feet above the sea. The fall at Platteklip, on the northern slope of Table Mountain, overlooking Cape Town and 550 feet above the sea, is considerable, namely 45 inches. The greatest fall is at Waai Kopje, about half a mile to the southward of the highest point of the mountain, at an elevation of 3100 feet, or 450 feet below the top. Another station on Table Mountain further south—that is, to the leeward in the rainy season—and 2500 feet above sea-level, has only 39 inches. The eastern suburbs, Rondebosch, Newlands, and Wynberg, all have a comparatively abundant rainfall, 40 to 50 inches and upwards, the greater part of which falls in winter time.—On the cause of the diurnal oscillation of the barometer, by Dr. R. Lawson. The object of this paper is to show that the durnal oscillation of the barometer is mainly due to the combination of the earth's rotation with its orbital motion.

PARIS.

Academy of Sciences, November 28.—M. Janssen in the chair.—On the most general equations of double refraction compatible with Fresnel's wave surface, by M. Maurice Lévy. Whatever view be taken of polarized light in a plane, whether it be regarded as the effect of an elastic or electro-magnetic disturbance, whether it result from rectilinear vibrations or from mean rotations (vortices), or from any other cause, it is certain, as remarked by Maxwell, that this cause is measurable by a quantity which is in the nature of a vector. This vector, whether it be a vibration or a force, the axis of a vortex or of a magnetic momentum, or aught else, is here called a luminous vector, and an attempt is made to determine its most general expression compatible with Fresnel's wave surface.—On the movement of cirri and their relations to cyclones, by M. H. Faye. These phenomena are compared to the action of a river on which floating ice is borne along. Whenever an eddy is formed, the nearest fragments of ice are seen to be drawn within its influence, following its spiral movements and disappearing with it on reaching the centre, while the masses lying beyond its influence continue to drift with the stream. Precisely analogous phenomena are presented by the cirri carried along by atmospheric currents in the higher regions. They are in the same way sucked down by the gyratory action of the whirlwind, giving rise in the lower regions to heavy rains, hail, and thunderstorms, while the more distant clouds continue to follow the general course of the wind.—Researches on the importance of consumptive patients breathing a pure air uncontaminated by pulmonary exhalations, by MM. Brown-Séquard and d'Arsonval. These remarks are made in connection with an apparatus submitted to the Academy, which has been constructed for the purpose of removing from bedrooms all the air exhaled by one or more persons. The importance is shown of thus purifying sick-rooms, hospital wards, &c., especially when occupied by patients suffering from affections of the lungs.—On a class of differential equations, by M. R. Liouville. Here are studied more especially the differential equations, amongst which are comprised all those of the geodetic lines.—Action of vanadic acid on the fluoride of potassium, by M. A. Ditte. It is shown that vanadic acid in combination with the fluoride of potassium yields compound substances more or less rich in fluoride. But in the presence of oxygen a certain quantity of potassa is de-

veloped, which forms vanadates with a part of the vanadic acid employed in the process.—Ammonical cyanides of zinc, by M. Raoul Varet. The chloride, bromide, and iodide of zinc combined with ammonia yield a relatively large number of compound substances. But with the cyanide of zinc, whatever be the conditions, the only substances obtained are $ZnCy,NH_3HO$ when the reaction takes place in the presence of water, and $ZnCy, NH_2$ in all other cases.—Application of a method of de Senarmont to the reproduction of celestine and anglesite by the wet process, by M. L. Bourgeois. The process by means of which de Senarmont obtained artificial crystals of barytine is here applied to the production of the allied minerals, celestine and anglesite.—On the importance of the nutritive function in determining the distinction between plants and animals amongst the lower organisms, by M. P. A. Dangeard. The Chytridineæ and the Chlamidomonadineæ, the two primary groups of the vegetable kingdom, are both connected below with the Flagellæ, branching off upwards one to the Algæ the other to the Fungus group. It is here shown that by the process of nutrition alone is it possible to determine the point where plant and animal become differentiated.—On the suckers of the Rhinantheæ and Santalaceæ, by M. Leclerc du Sablon. These hold an intermediate position between non-parasitic and true parasitic plants, drawing their nutriment both through their roots and through suckers from other plants. The present observations deal exclusively with the suckers and their various functions.—On the discovery of carboniferous formations with marine and vegetable fossils in the neighbourhood of Raon-sur-Plaine, by M. Bleicher. The recent discovery of coal in this district supplies the connecting link between the carboniferous measures of the Bruche and Rabodeau valleys (Alsace and Lorraine).

BERLIN.

Physiological Society, November 4.—Prof. du Bois Reymond, President, in the chair.—Dr. Goldschneider spoke on the fact, which has been known for a long time, that when carbonic acid gas is allowed to come in contact with the skin it produces a greater sensation of warmth than air of the same temperature. He has carried out a prolonged series of experiments to determine the cause of this increased sensation of heat. He examined first the purely physical factors which might have some influence on the observed facts—namely, the moistness, specific heat, and heat-absorption by the gases. When he compared the sensation of heat produced by moist air with that produced by dry air, he found that the former always seemed the greater; the difference between the two might be as much as 5° C. to 6° C. when the air was at a higher temperature than that of the skin. Thus, air at 35° C. whose saturation with moisture was 80 produced the same sensation of heat as air at 41° C. whose saturation was only 30. When experimenting with carbonic acid gas he found that a difference of 40 in the saturation produced a difference in the resulting sensation of heat corresponding to 2° to 3° of temperature. But even when equally moist or dry air and carbonic acid gas were allowed to act on the skin the sensation of heat produced by the latter was always the greater. It does not seem possible to explain the greater sensation of heat with carbonic acid gas by reference to the extremely small differences of specific heat of air and this gas, still less by reference to their somewhat greater coefficients of heat absorption. He also investigated the effect of the more ready absorption of carbonic acid gas by fluids, by removing the epidermis with a blister on a circumscribed portion of the skin and allowing the gas to act upon this place. The carbonic acid gas was speedily absorbed by the lymph, but it still produced a sensation of greater heat even when all moisture was removed from the surface exposed by the blister. He hence considers that the purely physical properties of the gas will not suffice to explain its remarkable influence on the sensory nerves for heat. Dr. Goldschneider next investigated the physiological factors which might suffice to explain the observed phenomenon. He proved that there is no recognizable objective rise of temperature under the influence of the carbonic acid gas. It is true that he observed now and again a distinct dilatation of the blood-vessels, but this was by no means constant, and not sufficient to account for the increased sensation of heat. He proved however as has been observed by many physiologists, that the carbonic acid gas has a direct effect upon the sensory nerves; but in contrast to the results of others, who attribute an anæsthetic action to this gas, he observed that at first it produces a hyperæsthesia of those nerves specially connected with the production of heat sensations, and then this makes way for an anæsthesia. The nerves connected with heat sensations were more strongly stimulated than those connected with sensations of cold. The speaker summed up the results of his extremely numerous experiments by urging that in addition to the greater absorption of heat by the carbonic acid gas and its power of producing hyperæmia of the skin, its action is to be explained chiefly by its direct chemical action on the endings of the nerves concerned in the production of sensations of heat. This therefore is to be regarded as the cause of the observed phenomenon that when carbonic acid gas is brought into contact with the skin it produces a greater sensation of heat than does the contact of equally warm and equally dry air.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Thomas A. Edison and Samuel F. B. Morse: D. B. Denslow and J. M. Parker (Cassell).—Our Earth and its Story: Edited by Dr. R. Brown (Cassell).—Stigmaria Ficoides: W. E. Williamson (Palæontographical Society).—Index Catalogue of the Library of the Surgeon General's Office, United States Army, vol. viii. (Washington).—The Ethical Import of Darwinism: J. G. Schurman (Williams and Norgate).—A Manual of Orchidaceous Plants, Part 1, Odontoglossum; Part 2, Cattleya and Lœlia (Veitch).—Osservazioni e Studii dei Crepuscoli Rosei 1883-86: A. Riccò (Roma).—Journal of the College of Science, Imperial University, Japan, vol. i. Part 4 (Tokio).

CONTENTS.

THURSDAY, DECEMBER 15, 1887.

THE HORTICULTURAL SOCIETY.

THE Horticultural Society of London was founded in 1804, among the first members being Sir Joseph Banks. Its objects were "to collect every information respecting the culture and treatment of all plants and trees, as well culinary as ornamental," and "to foster and encourage every branch of horticulture, and all the arts connected with it." The Earl of Dartmouth was the first President. The Society was incorporated by Royal Charter in 1809. In 1820 the Society purchased 21 Regent Street, which was its London home for forty years. In 1822 it obtained a lease of the present Gardens at Chiswick, which have been cultivated and embellished under the Society's auspices for upwards of sixty-five years.

At the conclusion of the war in 1815 the Society began to import plants from abroad, and this country owes to its early exertions many of the beautiful camellias, azaleas, peonies, roses, and chrysanthemums which are natives of the East, and among other plants the *Wistaria* (*Glycine*) *sinensis*, a lovely creeper now quite at home in England. Indeed, one cannot take a day's ride anywhere through the country without meeting some of the beautiful introductions of the Society. Among the collectors sent out by the Society was Douglas, to whose energy the country owes *Pinus Lambertiana*, *P. insignis*, *P. ponderosa*, *P. nobilis*, *P. Douglasii*, &c.

Dr. Lindley, one of the most eminent botanists this country has ever produced, was appointed Assistant Secretary in 1822, and continued connected with the Society until his death in 1865. No account of the early days of the Society would be complete without a record of the fruits of Fortune's journeys, under its auspices, in China. Not only did he send innumerable valuable plants home, but his travels in the Chinese tea-country were the direct cause of the introduction of tea-cultivation into India.

In 1839 the Duke of Devonshire was elected President, on the death of Mr. Andrew Knight, who had been President for twenty-seven years, and to whom the Society owed much. In January 1858 the Duke of Devonshire died, and H.R.H. the Prince Consort graciously consented to succeed him. The establishment of the Society at South Kensington, under H.R.H.'s guidance and direction, is so comparatively recent an event that it is not necessary to refer to it at length. At first the prospect was promising, and had not the Prince Consort's life been cut short, the result might have been very different from what it has proved. But the money expended on the buildings and the gardens at South Kensington, from the funds of the Society, was little short of £100,000—a sum which, with the experience we now have, no one would dream of devoting to such purposes. This enormous expenditure hung like a millstone round the neck of the Society, which soon found itself unable to pay the interest on the money borrowed to meet it. The result was that, under a clause of the Society's agreement with the Commissioners of the 1851 Exhibition, the latter body resumed possession of the Gardens six years ago, and the money spent upon them by the Society was swept away at a blow.

Nevertheless the horticultural work of the Society has been carried on with undiminished energy. This surely is the proper and legitimate work of such a Society. Every departure it takes from its true functions alienates the sympathy and support of those to whom it properly looks, and to promote whose objects it exists. Since the Society has been established at South Kensington, its activity in horticultural work has been as marked as even in its most prosperous times. Many thousands of new plants, fruits, and vegetables have been submitted to the examination and the verdict of the Society's Fruit and Floral Committees, which consist of practical men, of the greatest knowledge and experience in their several departments. The value attaching to "First-Class Certificates" is shown by the care nurserymen take to record them in their catalogues. At Chiswick a long series of elaborate trials and experiments have been carried on with fruit, vegetables, and plants, whereby useful and profitable varieties have been selected and their qualities established, and inferior varieties ear-marked.

Although the Society has been unable to hold great shows owing to the loss entailed by them, it has held fortnightly shows in summer and monthly shows in winter, at which a vast number of new plants and new introductions have been seen for the first time. Such shows, though small, are often far more interesting to horticulturists than the big shows which were the fashion formerly.

It may well be asked why, if the Society can give so good an account of itself, it should be in any difficulty? The answer is that its troubles are due to its connection with South Kensington. It cannot be said that the Commissioners of the 1851 Exhibition have behaved with any conspicuous liberality to the Society. Perhaps they could not do so, as they have said that it was necessary for them to make an income out of the Royal Horticultural Gardens. But the connection with South Kensington has made it necessary for the Society to meet the views of local subscribers, who were not horticulturists; and, moreover, it has led to the Society being saddled with a charter, which prevents its expansion and adaptation to altered times and circumstances.

The views of the Council are set forth in general terms in the statement and appeal which we print elsewhere The interest in horticulture in the United Kingdom grows and spreads without check. Surely the horticulturists of the wealthiest country in the world will gladly provide the very moderate sum required for the maintenance of a Society which has done much for them, is still doing much, and has before it untold possibilities of usefulness.

BALBIN'S QUATERNIONS.

Elementos de Calculo de los Cuaterniones, &c. Por Valentin Balbin, Doctor en Ciencias, &c. (Buenos Ayres: imprenta de M. Biedma, 1887.)

ALL praise is due to the Argentine Republic for its institution of a University in which the Faculty of Sciences is endowed with a chair of the higher mathematics.

The book before us is the outcome of one of the courses of lectures which the holder of that chair, Dr.

Valentin Balbin, delivered to an audience comprising several of his colleagues. The volume, written in Spanish, has been printed at Buenos Ayres, and in size (xix. and 359 pages), and in quality of paper and print, presents a very handsome appearance.

In his preface the author informs us that in his opinion the calculus of quaternions is the best vehicle for the teaching of applied mathematics, and that therefore he has had recourse to Sir William Rowan Hamilton's beautiful invention. The author is aware of the fact that he is the first to introduce quaternions into the Spanish scientific literature, and for this reason he aims at presenting the theory from its very elements up to its higher branches of application.

In the matter of notations we are also informed that those of Hamilton and of Prof. P. G. Tait have been scrupulously adhered to, and that, in one word, the author has not found it advisable to follow those of M. Hoüel and of M. Laisant. It may not be known to everybody that these two French mathematicians have in their publications (1874, 1877, 1881) adopted a thorough reversal of Hamilton's lettering. In the place of the inventor's Greek letters, they use Roman characters (X for ρ, Y for σ, A for α, &c.); and in the place of the handy S, V, T, U, they put black-letter symbols, which are at once difficult to write, and tiring to the eyesight. These are what they call "slight alterations" or "improvements."

Again, they upset the rule about the relative appellation of the factors of a product. Our author (p. 40) states the Hamiltonian rule, and justifies it by the simple example drawn from $a + a = 2a$, where (according to everybody's ideas) the coefficient 2 is the multiplier, and a the multiplicand. According to the "innovated" rule one ought to write $a + a = a \times 2$.

The rule just named makes its influence felt more particularly in the establishment of the operator of conical rotation, and here we are sorry to find that our author falls a victim to a delusion. Instead of Hamilton's well-established $q(\ \ )q^{-1}$, he arrives at the inverse $q^{-1}(\ \ )q$ (at p. 296), and uses it under this form through several pages (up to p. 303). This comes from following M. Laisant's text, and forgetting his own rule. In M. Hoüel's opinion, "nothing is easier than to pass from one system (his own system) to the other"; nevertheless, such passage requires to be nicely managed, because by it the expression for the instantaneous axis is affected, and we might ask whether it be fair to introduce a source of confusion into a theory which in itself is difficult enough. Our author does not introduce us to the searching treatment which Prof. Tait has devoted to the question of the movement of a solid about its centre of mass ("Elementary Treatise," &c., second edition, §§ 383-400). M. Balbin's treatment of that question is very curtailed, and we might be inclined to attribute this shortness to a feeling of distrust, otherwise how could we understand his utterance, at p. 87, where he says forcibly, "Some simplifications, particularly in the physico-mathematical applications, must be made in the future as to the matter of symbols" (*se hagan*, imperative of *hacer*).

The more we consider the innovations, the more are we convinced that their proposer and his follower, publishing in 1874 and 1881, had not fully realized the extent and importance of the researches which, during many years, had been expressed in what we may term the Hamiltonian notations. In 1862 no less a Frenchman than M. Allégret set the example of following these last-named, and that precedent ought to have been adhered to. As it is, students of MM. Hoüel and Laisant will be hampered by the French notations when they approach those rich mines of information contained in such unique classics of the quaternion method as Hamilton's "Lectures" and "Elements," and the "Elementary Treatise on Quaternions," by Prof. Tait.

Let us now try to give some idea of the contents of the volume. For the English student these are all contained in the sources known to him. First "The Introduction to Quaternions," by Kelland and Tait. This work has been reproduced in its whole extent, with the exception of Chapter X., due to Prof. Tait alone. The author acknowledges in several instances (pp. 114, 120, 252) his special indebtedness to the English authors; and his translations are adequate. Perhaps, however, he knows them best through the medium of M. Laisant's reproduction of a great part of Kelland's work (with acknowledgments in the preface, tempered by the praise of the new notations).

In the second place, in the treatment of linear vector-functions and the resolution of equations involving them, which were originally given by Hamilton, there are clear indications that our author has taken his text from works where the innovated notations reign supreme; some traces of x (at pp. 183, 184, 193), for instance, are left standing in the place of ρ, and are contained concurrently with ρ in one and the same equation, in several cases; no explanation about the signification of x being given. A similar fate befell the vector ρ, at p. 138, where x is put into its place by being defined: $x = ix_1 + jx_2 + kx_3$. Under this form x is introduced into the operator ∇, which in its turn undergoes a little adaptation. But all this is not the promised adhering to Hamiltonian notations.

The solution of the vector-equation $\Sigma\alpha S\beta\rho = \gamma$ is gone partially into; but the calculation of the coefficient m of the cubic (at p. 192) contains an inexact intermediate step, and the coefficient m_1 is given with the wrong sign. Finally the solution of the proposed equation (p. 193) is incorrect, owing to the absence of the factor γ in the first term of the second member. These three inaccuracies can be traced to one of the French texts.

In the third place, curves in space, and centres of curvature of those curves, and of plane sections of surfaces, subjects exhausted by Hamilton and by Prof. Tait, have been treated by our author with the help of Dr. Graefe's little volume on Quaternions (Leipzig, 1883). We might take exception to Dr. Graefe's deduction (p. 236 of Balbin's) of Meusnier's theorem, as well as of that of the curvature of a normal section of a surface. To replace a scalar, say $S\alpha\beta$, by $\frac{1}{2}(\alpha\beta + \beta\alpha)$, in order to procure an expression of the product $\alpha\beta$ separately, seems to us to be forsaking the spirit of the method of quaternions; the expression for $S\alpha\beta$ being given, and that of $V\alpha\beta$ being deducible from other considerations, it would have been far simpler to deduce $\alpha\beta$ by forming the sum $S\alpha\beta + V\alpha\beta$ straight forward. Some reticences (p. 236), and even some inaccuracies, in the text of Dr. Graefe, have been

reproduced also by M. Balbin at pp. 136, 138, 247. Dr. Graefe, like other German authors on quaternions, reproduces a great part of the "Introduction to Quaternions" by Kelland and Tait, and also some parts of the "Elementary Treatise" by Prof. Tait; but after having once pronounced the name of Hamilton, he has done all in the matter of acknowledgment, and the name of Tait is not to be found in the little volume.

We now come to the fourth class of subjects treated by our Argentine author. This comprises kinematical, statical, and dynamical questions. Here we meet with the treatment, in good form, of questions included in Hamilton's "Elements," and in the second edition (1873) of Prof. Tait's "Elementary Treatise." Of this last source of information our author seems to have only a second-hand knowledge: he reproduces verbatim the contents of § 405 of the "Elementary Treatise" (second edition), but he attributes the authorship of it to M. Laisant. Evidently, M. Laisant reproduced this § 405, which treats of Foucault's pendulum, but the origin of the treatment is to be found in the Proceedings of the Royal Society of Edinburgh of 1869, *auctore* P. G. Tait. Again, by the small-print note at p. 303 we have another indication that our author was unacquainted with the contents of the two or three last chapters in the second edition of the "Treatise." Had he known them, he could not have withheld a more special acknowledgment of results worked out by the immediate follower of Hamilton.

Prof. Tait certainly can claim to have been the first to make quaternions intelligible, not alone to ordinary students, but to advanced mathematicians—"such as have the [rare] gift of putting an entirely new physical question into symbols." But the Edinburgh Professor has particular claims to the thankfulness of students of the first-named category (the writer amongst them), for, under the plea of teaching the quaternion method, he has given them an insight into those physico-mathematical questions which are so unapproachable when obscured by the apparatus of Cartesian co-ordinates. When these questions are expressed and solved in quaternion language, they acquire a clearness and a conciseness which might well astonish their original proposers—Green, let us say, Ampère, Poinsot, even Newton, not to name living workers. We cannot be expected to enumerate the list of the questions treated; we will allude only to those in which the operator ∇ is pressed into services of such marvellous fecundity, to those in which the linear vector-functions play an eminent *rôle*, and to those in which the operator of conical rotation is such a powerful auxiliary.

The last chapter of the volume contains a painstaking record of the history of quaternions. The English reader will find much of this, and even more, in the article on "quaternions" in the "Encyclopædia Britannica." We may say that the imaginaries of algebra having done good service during the process of discovery, can be safely now banished from the principles and practice of the quaternion method—unless bi-quaternions are under treatment. In the ordinary applications of the method the extraction of the square root of the members of an equation such as $\epsilon^2 = -1$ (ϵ being a unit-vector) is looked upon as impracticable, and the reason is clearly this: the combination $\epsilon\epsilon$, represented by ϵ^2, is a symbol *sui generis* just as much as ϵ itself, and cannot be decomposed or attacked—to speak the language of chemistry—by the algebraical operation of extracting the square root of it. To assimilate a unit-vector with $\sqrt{-1}$, the square root of negative unity, is as if, in the differential calculus, one were to assimilate a derivate, $\frac{dy}{dx}$, with the symbol $\frac{0}{0}$ of indetermination. We cannot resist the temptation of helping our author to preserve a little curiosity in the history of the subject. The author records the verdict of an unnamed French mathematician, who says: "Quaternions have no sense in them, and to try to find for them a geometrical interpretation is as if one were to turn out a well-rounded phrase, and were afterwards to bethink oneself about the meaning to be put into the words. . . ." This, after all, is rivalled by the verdict of a German mathematician, who simply declared the quaternion method to be "eine Verirrung des menschlichen Geistes" (an aberration of the human intellect).

GUSTAVE PLARR.

CABLE-LAYING.

On a Surf-bound Coast; or, Cable-laying in the African Tropics. By Archer P. Crouch, B.A. Oxon. (London: Sampson Low, 1887.)

IT is somewhat remarkable that the business of making and laying submarine telegraph cables—which hitherto has been a monopoly of Great Britain, and employs large numbers of skilled workmen of all kinds, of scientific men, and of sailors—should be so little understood by people not directly connected with it. Yet the daily history of any cable-laying expedition, if faithfully written, would contain matter of engrossing interest for all readers. To secure a contract on advantageous terms requires diplomatic talent of a high order. For, although the business is a British monopoly and there is no competition with the foreigner, there is all the keener competition between the rival British companies. Further, the negotiations are almost always with Government departments, either home, colonial, or foreign, and are necessarily of a delicate character. In the history of any particular cable the preliminary diplomatic details would no doubt have by far the greatest interest for most readers, but it would be obviously indiscreet and unadvisable to publish them. In tendering for a cable against powerful competitors it is important to have as accurate a knowledge as possible of the depth of water and the nature of the bottom where the cable is to lie, in order to know exactly the lengths of the different types of cable which will have to be employed, and so to estimate the cost. In obtaining this knowledge the cable-laying companies have been the chief contributors to the science of deep-sea research, or oceanography. The contract obtained, the cable made, and the route determined on, the operation of laying has to be undertaken. When it is merely a question of laying a length of cable between two points over smooth ground, this is in most cases a very simple affair; although if the shore-ends of the cable have to be landed on exposed beaches, as is only too often the case, there is plenty of opportunity for thrilling incident and hair-breadth escape. The expedition in which Mr.

Crouch was engaged had for its object to connect a number of settlements on the West Coast of Africa from Bathurst to St. Paul de Loanda, and belonging to the British, French, Spanish, and Portuguese. Although it is fixed beforehand what places are to be connected, it is only when the ship arrives on the ground that the exact place of landing, the amount of assistance to be got from the shore, and a host of matters of minute local detail, but of great importance, can be settled, and to do so satisfactorily, expeditiously, and without friction, requires qualities of a very high order in the chief of the expedition. How difficulties were overcome, dangers met, and accidents repaired, in the course of the laying of one portion of the West African Company's cables is told in a pleasant and readable way in "On a Surf-bound Coast." Mr. Crouch carries his descriptions only as far as Cutanu, a French settlement lying between Accra and Lagos. From this place the cable was taken to the Portuguese islands St. Thomê and Principe, the French settlement on the Gaboon, and the Portuguese town St. Paul de Loanda; but this part of the expedition is reserved for description in a possible future volume.

The narrative begins with the start of the s.s. *Thracia* and her consort the *Pioneer* from the Thames, under the chief command of Mr. White. The names of persons and ships are purposely altered. The work really begins with their arrival at Bathurst, the chief British settlement in the Gambia district. In the previous year the cable had been laid as far south as the French settlement Conakry, about 70 miles north of Sierra Leone; and outside of Bathurst the cable, coming from the Cape Verde Island of St. Jago, had been joined to it, forming a T piece. Their first job was to cut out this piece, and run the three ends, leading respectively northwards to Dakar, westwards to St. Jago, and southwards to Conakry, into Bathurst. This affords the author an opportunity of describing the operations of "picking up" in shallow water, also of laying shore ends in protected water, and of splicing cables. In this Mr. Crouch acquits himself fairly well; indeed, it is very difficult to make intricate mechanical operations of the kind quite intelligible to the uninformed without the use of drawings. In the course of these operations the two ships each passed a portion of their time on the sand-banks, which are here plentiful and almost completely unsurveyed.

The next piece of work was connecting the French settlement Conakry with Sierra Leone. Here, again, there was no dearth of incident, as the *Pioneer*, in landing the shore end, went on a rocky patch, and was with difficulty got off. During the laying the cable got round the propeller, and might have caused great delay but for the promptitude and courage of Mr. White, who, without hesitation, went overboard and dived straight down to the propeller, and on coming up ordered "three half turns more in the same direction," when the cable came free. There are many other instances scattered through the book of the resource, energy, and perseverance required for success in this kind of work.

The *Pioneer* was obliged to return home, and the *Thracia* left Sierra Leone alone and proceeded round the coast, landing shore ends at Grand Bassam, Accra, and Cutanu or Appi. The company's larger steamer, the *Copperfield*, meanwhile came out with the bulk of the cable, and connected these shore ends. Mr. Crouch was transferred to the *Copperfield*, and assisted at the laying of a portion of these cables, and he was landed at Accra, along with two other members of the staff, to attend that end of the cable, which, for the time being, had a blind end buoyed out at sea. It was the duty of these gentlemen to watch night and day the spot of light on the scale of the galvanometer, so as to be ready to answer whenever the ship picked up the end and "called" them. The fatigue and monotony of this weary work of waiting and watching is well described. There are also interesting descriptions of Accra and its inhabitants. Indeed, the latter half of the book is by far the more interesting; there is more business in it and less of the jokes and chaff of the quarter-deck, which, though useful and even amnsing at the time, seldom possess sufficient permanent value to justify their being printed at any length.

The book, taking it all round, is a useful and entertaining one, and as a first attempt is altogether creditable and full of promise. In any future work the author should not be afraid of tiring the reader by careful and detailed description of any operation of interest on which he may be engaged. Outside of those directly connected with the business or the profession, the general reader knows nothing of the methods of laying and working submarine cables. J. Y. B.

TEXT-BOOK OF GUNNERY.

Text-book of Gunnery, 1887. By Major G. Mackinlay, R.A. (London: Printed for Her Majesty's Stationery Office by Harrison and Sons, 1887.)

TO realize the great alterations which have taken place in artillery in the last twenty years it will be necessary to compare the present work with the corresponding "Treatise on Artillery" of 1866, prepared for the use of the Practical Class, Royal Military Academy, in which there is no mention of rifled artillery, iron armour, or electro-ballistic apparatus, and the Practical Class were expected to go forth fully equipped to compete with any foreign enemy with the smooth-bore gun, the spherical projectile, the formulæ for penetration into earth, and such information on velocity and resistance as the ballistic pendulum could afford. If twenty years can make such alterations in the science of artillery, imagination attempts to penetrate the future and to gather some information as to the view in which the present treatise of 1887 will then be held.

Official treatises, however, must not be criticized according to the irreverent sceptical rules of modern scientific inquiry. The authors are prevented by military discipline from expressing any opinion on the merits of the weapons they describe, even when of an experimental nature. Thus the Armstrong rifled gun had been in serviceable use for seven or eight years, and Mr. Bashforth had been experimenting with his chronograph on elongated projectiles and the resistance of the air to their flight for nearly two years, before the appearance of the 1866 edition of the "Treatise on Artillery"; and, coming to the present edition, we find little or no mention of such important matters as steel shields for the protection of the gunners in the field against bullets (*vide* reports on the Boer War),

the importance of range-finders in lessening the amount of ammunition to be carried in the limbers, the dynamite gun for use in sieges, and other modern developments.

After the Battle of Waterloo we went comfortably to sleep on our laurels, and awoke to find ourselves engaged in the siege of Sebastopol with exactly the same weapons we had employed in the Peninsular War. Sebastopol with modern weapons could have been taken with one-tenth of the hundreds of millions that were lavished; so it is important for the future that the taxpayer should take an intelligent interest in military preparations and see that we are provided with the very best weapons that money can procure. Such an intelligent public has been educated in the Volunteer force, and these "men with muskets" are not prevented by military discipline from criticizing their muskets, or equipment in general; and it is to them that we owe the healthy criticism that has lately been exercised on our armaments and state of military preparation.

Hotspur's description of the regular military officer saying: "It was great pity, so it was, that villainous saltpetre should be digged out of the bowels of the harmless earth, which many a tall fellow had destroyed so cowardly; but for these vile guns," &c., is true to this day; for the modern artillery officer's pride in his gun varies inversely as the weight, for certain tangible reasons; and generally a soldier looked upon his weapons as something to keep clean and to drill with until some recent warfare taught him the importance of the despised musketry instruction. The officer's attention is fully occupied in attending to the drill and discipline of his men according to the regulations; and we find that the scientific development of methods of destruction is generally due to amateur civilians like Benjamin Robins, of Quaker extraction, the father of modern gunnery, and the Rev. Mr. Bashforth; while the Gatling gun is a product of Philadelphia, the City of Brotherly Love. Clerk's "Naval Tactics," written by John Clerk of Eldin, a relative of Prof. Clerk Maxwell, and an Edinburgh barrister, was the treatise which put a stop to the ineffective naval engagements of the last century—ineffective because culminating in the failure of the fleet to relieve, and the consequent surrender of, Yorktown.

Major Mackinlay's treatise appears to be very carefully compiled, and taking into account the restrictions under which the author works, it is fully up to date with the development of our own artillery; whether with the artillery of foreign countries is another question. We notice, however, with some regret that the guns illustrated in the text are all muzzle-loaders, as if breech-loading was the temporary fad which the rifled gun was considered in the time of the treatise of 1866.

A valuable chapter on steel, new in this edition of the treatise, reminds us that our authorities are now after thirty years' delay taking up the Whitworth method of construction of ordnance, omitting, however, the Whitworth hexagonal bore. An official Solomon gave decision in favour of Armstrong against Whitworth in their celebrated competition, with the effect of alienating the greatest steel manufacturer of the world from Government purposes. His great prototype would have encouraged now one and now the other, without committing himself to an absolute decision, and would thus have reaped for his country the benefit of the invaluable services of both competitors.

Major Mackinlay has done good service by collecting all the ballistic tables based upon the important experiments of Mr. Bashforth, and by showing how they are applied to the questions of artillery. We must be on our guard, however, against using ink instead of gunpowder, from economy, and against imagining that there is no further need of careful experiment and practice. It is of the greatest importance, too, that cadets should learn from this treatise that the science of artillery is not entirely comprised in guns of the smallest dimensions, manœuvred over rough country, and the doing of some snap shooting The history of recent wars teaches us that the field artillery of both sides is used up in the first two or three engagements, and that the conflict finally resolves itself into a vast siege, in which the whole army and navy are converted into garrison artillery.

The article by the author of "Greater Britain" in the *Fortnightly Review*, tells us of the immense pains now taken on the Continent in military preparations. Let us avoid in time the necessity of the dreary up-hill labours which the French have been compelled to undertake, now at length beginning to culminate in an organization which, it is important to keep in mind, might at any moment be tested by being brought to bear against this country.

ROMANTIC LOVE AND PERSONAL BEAUTY.

Romantic Love and Personal Beauty. By H. T. Finck. Two Vols. (London: Macmillan and Co., 1887.)

IN dealing with the subject, or, rather, the group of subjects, here indicated, Mr. Finck seems to have had before him a twofold object, scientific and practical. On the scientific side he deals with romantic love, showing (*a*) that it is a recent growth, (*b*) what are its conditions, and (*c*) what are the conditions of beauty as essential to romantic love. From a practical point of view he (*a*) gives rules for health, which is essential to beauty and therefore to romantic love, and (*b*) insists upon the necessity of free choice in love being left to young people. Let us see briefly what he has to tell us upon these points.

Goldsmith, in the "Citizen of the World," was wrong, Mr. Finck holds, in teaching that love proper existed in ancient Rome. "Romantic love is a modern sentiment, less than a thousand years old. . . . Of all personal affections the maternal was developed first, and the sentiment of romantic love last." Here Mr. Finck has certainly got hold of a truth, but he puts it much too strongly. There is nothing improbable in the growth of a new emotion, or (as we would rather say) in an old emotion receiving a new direction and a great expansion. Vol. I. (pp. 34–37) shows that parental and filial love have little or no existence among animals and among some savages; and if civilization can develop these feelings to their present pitch of intensity, it might well do the same for the mental, as distinguished from the bodily, attraction between man and woman. But the modern form of love is not a new feature; it is essentially a development. It was stunted and kept down at Rome and in most of Greece but still it was in existence; and, if Mr. Finck will extend

his classical reading, he will find more traces of it than those which he enumerates. Let him begin with the Greek novelists, and see whether Heliodorus's account of the loves of Theagenes and Chariclea will not come up to his standard.

But what are the conditions favourable to the growth of romantic love? Greece—by which Mr. Finck chiefly means Athens—was cut off from such love by three causes: the degraded position of women, the absence of direct courtship, and the impossibility of exercising individual preference (i. 126). The second and the third seem to us to run together, but still we see here some of the points on which romantic love depends; and to these may be added intellect (ii. 14), monogamy (i. 58), and a *long* courtship (i. 59). The old-fashioned plan which Goethe describes—

> "In der heroischen Zeit, da Götter und Göttinnen liebten,
> Folgte Begierde dem Blick, folgte Genuss der Begier,"—

has left us many a charming picture, and none more charming than the Homeric hymn to Aphrodite; but such prompt satisfaction of love no doubt did not give to a romantic passion sufficient time to grow. The feeling was there, but rudimentary. Now, that rudimentary feeling has so grown as to have largely pushed out of sight its physical basis, and men and women act (or think they act) upon other and higher impulses. To this change the agencies enumerated by Mr. Finck have doubtless contributed, and he would apparently acknowledge, too, that the general alteration in the position of women has affected the way in which their lovers regard them. But we should lay more stress than he does on the influence of poets and novelists; they have gone on painting unreal feelings until they have made them real; what a few characters felt at first has been worked by this agreeable sermonizing into the nature of all the readers.

But, after all, the starting-point of romantic love is beauty. Where the women are secluded, beauty cannot be seen. Where matches are made by the parents, beauty does not count. But, where free selection is left to young people, beauty takes its proper place. It is a sign of health, and "love in its primitive form urges animals to prefer those that are most healthy." Mr. Finck therefore goes on next to describe the causes which bring out beauty: "a climate tempting to outdoor life; a considerable amount of intellectual culture and æsthetic refinement; a mixture of nationalities, fusing *ethnic* peculiarities into an harmonious whole; and love, which fuses *individual* complementary qualities into an harmonious *ensemble* of beautiful features, graceful figure, amiable disposition, and refined manners" (ii. 25); or, more shortly, health, crossing, love, and mental refinement (ii. 73). Thus love and beauty act and react on each other; in connection with which point Mr. Finck makes a suggestion of some importance when he says (ii. 94-95):—

"The artificial preservation of disease and deformity, in and out of hospitals, due to Christian charity, might in the long run prove injurious to the welfare of the human race, were it not for the stepping-in of modern love as a preserver of health and beauty. What formerly was left to the agency of natural selection, is now done by love, through sexual selection, on a vast scale."

It is even more difficult to persuade women than it is to persuade men to do what is good for them, and if the close connection thus pointed out between health and beauty will not induce women to take a little trouble to preserve or improve the former, we must give them up as hopeless. By insisting on this cardinal truth, Mr. Finck will do a useful work, though some day perhaps our descendants will wonder that it should have needed insisting. There is room enough for improvement in both health and beauty. Mr. Galton tells us that "our human civilized stock is far more weakly through congenital imperfection than that of any other species of animals"; while, as for beauty, it is likely that the world is but at the beginning of what sexual selection, unhampered and unthwarted by other agencies, can do for us. It is, Mr. Finck affirms, a moral duty for girls to defy parental tyranny "where money or rank are pitted against love. For the health and happiness of the next generation are at stake."

This is strong speaking; but still, if our author would always speak as seriously and soberly as this we should have but little quarrel with him. Unfortunately he has spoiled an interesting book, not only by a gossiping and confused arrangement of its matter, but also by an intolerably jaunty style, flavoured with Americanisms. A book which claims scientific value should not be disfigured by stupid jokes (as on Prior and priority), or by such phrases as "the female persuasion," "Schopenhauer's Will is an æsthetic sort of a chap," "a young animal that would risk its own life in defence of its mother or father is yet to be heard from."

F. T. RICHARDS.

OUR BOOK SHELF.

Earth-Knowledge: A Text-book of Elementary Physiography. By W. J. Harrison, F.G.S., and H. R. Wakefield. (London: Blackie and Son, 1887.)

THIS is a small text-book adapted to the somewhat remodelled syllabus of the Science and Art Department's elementary stage of physiography. There can be no doubt about the usefulness of the book, but it is to be regretted that more originality is not displayed in the treatment of the subject of matter and energy. Of the rest of the book no complaint can be made: it is excellent. That which deals with matter and energy, however, is meagre and unequal, and the arrangement is at times unnatural. Gravitation, for instance, is discussed without any direct reference to *weight*, although two pages are devoted to the methods of determining specific gravities. Then, again, one would scarcely expect nowadays to read a chapter on *energy* without finding some mention of the doctrine of the conservation of energy.

We are afraid, also, that the chapter on voltaic electricity will be rather misleading to beginners, as no mention whatever is made of the existence of any kind of battery beyond that consisting of a single copper-zinc cell, whilst effects are described which could only be produced by the current from many such cells. The definition of a stress as the "mutual action at the surface of contact between two bodies, whereby each exerts a force upon the other," is also rather misleading, since it does not include the stresses of gravitation, electricity, and magnetism.

Of course too much cannot be expected of an elementary text-book, but it is quite time that the modern ideas regarding force, energy, and matter should be introduced into such books.

A. F.

A Dictionary of Place-Names. By C. Blackie. Third Edition, revised. (London: John Murray, 1887.)

EVERY teacher of geography knows that the derivation of place-names never fails to excite the interest of intelligent scholars. It is satisfactory, therefore, that there should have been a demand for a third edition of Mr. Blackie's excellent book, in which he presents in plain and simple language many of the most suggestive results established by students of topographical etymology. The work has been carefully revised, and in its present form ought to be of service to many a "general reader" and tourist, as well as to schoolmasters and their pupils. Prof. J. S. Blackie contributes to the volume an introductory essay, in which he offers, in his lively way, many useful hints as to the spirit in which the study of topographical etymology ought to be pursued.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

The Supposed Earthquake in England.

FROM the inquiries which have been made it is now ascertained that the loud noise—as of an explosion—heard over so wide an area on the morning of November 20, and referred to by Mr. Worthington G. Smith in your issue of last week (p. 127), was due to the breaking up of a large meteor, which crossed the north of Herts from east to west, upon a line of which the extreme points are approximately Saffron Walden, in Essex, and Swindon, in Oxfordshire. The meteor was seen by one observer from Hertford, and probably it would have been generally noticed but for the foggy state of the atmosphere. I have undertaken to investigate this matter as far as Herts is concerned, and shall be very much obliged to any of your readers who can give me assistance if they will send a note of their observations, especially as to the direction from which the shock they experienced appeared to reach them. H. GEORGE FORDHAM.

Odsey Grange, Royston, Cambridgeshire, December 12.

The "Umbria's" Wave.

IN NATURE, vol. xxxvi. (p. 508) you published some details from Mr. W. Watson about the wave which struck the *Umbria* in mid-Atlantic. Having heard of two similar cases, and being in possession of the details of one, I have made the following comparison.

Comparison of the "Umbria's" and the "Faraday's" wave.

		Umbria.	*Faraday.*
Date		26.7.87	14.2.84
Hour		4.40 a.m.	6.45 a.m.
Position of ship	Long. W.	27° 8′	27° 53′
	Lat. N.	50° 5′	46° 11′
Ship's speed—knots		about 16	about 6
Ship's course		West (probably partly South)	E. 18° N.
The wave struck		the bow	the port beam.
Probable course of wave		East partly North.	South partly East.

These two courses if prolonged backwards would intersect at about 30° W. lat. and 50° N. long. This is the very point where the *Faraday*, while laying a cable in 1882, discovered a reef rising about 6000 feet above the bed of the ocean. The *Umbria* when struck was about 120 miles to the east of this position, and the *Faraday* about twice as far to the south-east.

The *Faraday's* wave was seen fully five minutes before it struck, and then like the *Umbria's*, it did considerable damage. Three life-boats, chart-house, deck-house, and part of the bulwarks were smashed and one of the large buoys carried away.

In his letter Mr. Watson doubts whether this wave was caused by an earthquake, but a few more similar occurrences in the neighbourhood of Faraday's Reef will possibly demonstrate it to be of recent and volcanic growth. C. E. STROMEYER.

Strawberry Hill, November 22.

The Planet Mercury.

THE atmosphere in this country is generally so unfavourable that it is a very rare occurrence to see the planet Mercury even at its greatest elongation from the sun, unless carefully looked for. My experience of the last few days may therefore be worth mentioning. The day before yesterday, at 10 minutes past 7 in the morning, I was in bed at some distance from a window, through which, without directing my attention to it, I saw a star shining with sufficient brightness in the twilight to attract my notice. I raised the window and made use of a large opera-glass, when any doubt I had would have been dispelled even if I had had no previous experience of Mercury, for there was to be seen a small planet with distinct disk some 15° above the horizon—Venus, a magnificent object, being of course visible also. The same thing happened this morning, when I again noticed Mercury, without having him in my mind, before leaving my bed; but this time I was better prepared, and in the course of the next quarter of an hour had shown the planet, in a 3⅞-inch telescope, to several persons who saw it for the first time. G. F. P.

Hanworth, Middlesex, December 9.

Meteor.

ON Friday night, about 9.15, a fine meteor, as bright as a star of the first magnitude, was seen in the western sky. It made its appearance at an elevation of 35° west-south-west, and disappeared in the west, at an elevation of 20°, leaving no streak. Perhaps some other of your readers might be able to identify the meteor, and thus a clue to its course might be arrived at. If you think this worth inserting, it may interest some one.

Barrow-on-Humber, Hull, December 9. M. H. MAW.

"Fairy Rings."

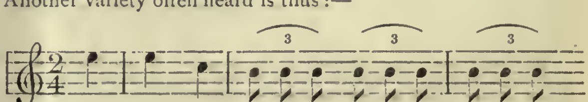
YOUR article on "Fairy Rings" (November 17, p. 61) speaks of rings of 100 feet in diameter as wholly exceptional. In the parish of Stebbing, in Essex, there is a field containing numerous rings of *Paxillus giganteus.* The largest of these is incomplete, being broken in places by gorse bushes and stopped on one side by a hedge and ditch. Measurement is consequently difficult, but the diameter of the ring cannot be less than 120 feet. At Bunchrew, in Inverness-shire, I once saw the same fungus covering about a rood of ground. The grass all over this was very coarse and dark green in colour, being chiefly *Dactylis glomerata*, but there was no trace of a ring. J. SARGEAUNT.

Felsted, December 6.

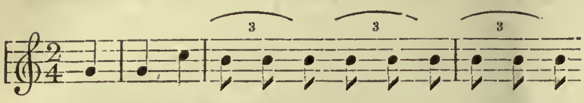
Music in Nature.

IN NATURE (vol. xxxvi. pp. 343 and 605) reference was made to melodies of birds, &c. I have often heard in the provinces of New Brunswick and Nova Scotia a bird which sings as clear and accurate a melody as can be given forth by any human songster. It is a small gray bird with double lines of velvety white on the sides of its head. I do not know its name. The song varies somewhat in different individuals, but always has the same characteristics. The commonest form is as follows:—

Another variety often heard is thus:—



A rarer form is as follows :—



I have noticed that this latter form seems more difficult for the little musicians, one of whom in particular used to provoke me by singing the B most outrageously flat. I have been accustomed to imitate these birds by whistling, and they very readily answer my whistle. In this way the different forms of their theme have become fixed in my memory. W. L. GOODWIN.

Queen's University, Kingston, Canada, November 11.

Who was Mr. Charles King?

AMONG the ingenious in many considerable parts of the world, of whose undertakings, studies, and labours the Philosophical Transactions of the years 1700 *sqq.* gave some account, an able microscopist suddenly appears, of whose life and work one would like to have more accurate information than seems to be current. Perhaps a member of the Royal, or the Royal Microscopical, Society may be able to supply some particulars about this "Anglois anonyme," as Trembley calls him, and willing to assist in rescuing his name from an undeserved oblivion. His first contribution to the Philosophical Transactions—of very little importance indeed—is to be found in No. 266, for September and October 1700, pp. 672–673, under the title, "A Letter from Mr. Charles King to Mr. Sam. Doudy, F.R.S., concerning Crabs Eyes;" it is dated, "Little Wirley, Decemb. 14," and subscribed, "Ch. King." In the copy of the Transactions I have before me, a contemporary, who seems to have been tolerably well informed, has inserted divers MS. notes, remarks, and corrections; he added here the words, "Staffordsh^{e}." to the locality, and "Student of Ch. Ch. Oxon." to the subscription, which, as far as I know, does not recur in any of the subsequent Transactions. But under the title, "Two Letters from a Gentleman in the Country, relating to Mr. Leuwenhoeck's Letter in Transaction, No. 283, Communicated by Mr. C." (in No. 288, for November and December 1703, pp. 1494–1501, with eight figures, text and illustrations being both equally remarkable for the period), the same hand has again inscribed the name of "Mr. Charles King," and filled up the blanks left on pages 1494 and 1495 by the initials "W." and "W. Ch. Esq." with the additions of "irley par. Cōm. Stafford." and "Walter Chetw... of Ingestry Staffords^{e}." (the rest has been cut off by the binder of the volume), so that there remains no reasonable doubt as to the truth of the identification. Now we read in the second of these letters from the country, dated "July 5, 1703," p. 1501, "But of those" (viz. animalcula) "(among other things) I last year gave an account to Sir Ch. Holt, which I hear will shortly be publish'd in the Transactions." I don't think it is bold to conjecture that the account here alluded to had already been published, and is, in fact, the article printed in No. 284, for March and April 1703, pp. 1357(*bis*)–1372 (with excellent figures on the plate accompanying that number), under the title of "An Extract of some Letters sent to Sir C. H. relating to some Microspocal" (*sic*) "Observations. Communicated by Sir C. H. to the Publisher" (H. Sloane); and no doubt these epistles may also be ascribed to the same anonymous gentleman.

In all the above-mentioned letters we have some early and first-rate contributions to microscopical science, the importance of which had been shortly before so evidently demonstrated by the wonderful discoveries made by the improved magnifying-glasses.

Quæritur: Who was Mr. Charles King? S.

The Hague, November 27.

NOTE ON A PROPOSED ADDITION TO THE VOCABULARY OF ORDINARY ARITHMETIC.[1]

THE total number of distinct primes which divide a given number I call its Manifoldness or Multiplicity.

[1] Perhaps I may without immodesty lay claim to the appellation of the Mathematical Adam, as I believe that I have given more names (passed into general circulation) to the creatures of the mathematical reason than all the other mathematicians of the age combined.

A number whose Manifoldness is n I call an n-fold number. It may also be called an n-ary number, and for $n = 1, 2, 3, 4, \ldots$ a unitary (or primary), a binary, a ternary, a quaternary, ... number. Its prime divisors I call the *elements* of a number; the highest powers of these elements which divide a number its *components;* the degrees of these powers its *indices;* so that the indices of a number are the totality of the indices of its several components. Thus, we may say, a prime is a one-fold number whose index is unity.

So, too, we may say that all the components but one of an odd perfect number must have even indices, and that the excepted one must have its base and index each of them congruous to 1 to modulus 4.

Again, a remarkable theorem of Euler, contained in a memoir relating to the Divisors of Numbers ("Opuscula Minora," vol. ii. p. 514), may be expressed by saying that *every even perfect number is a two-fold number, one of whose* components *is a prime, and such that when augmented by unity it becomes a power of* 2, *and double the other component.*[1]

Euler's function $\phi(n)$, which means the number of numbers not exceeding n and prime to it, I call the *totient* of n; and in the new nomenclature we may enunciate that *the totient of a number is equal to the product of that number multiplied by the several excesses of unity above the reciprocals of its elements.* The numbers prime to a number and less than it, I call its *totitives.*

Thus we may express Wilson's generalized theorem by saying that any number is contained as a factor in the product of its totitives increased by unity if it is the number 4, or a prime, or the double of a prime, and diminished by unity in every other case.

I am in the habit of representing the totient of n by the symbol $\tau n, \tau$ (taken from the initial of the word it denotes)

[1] It may be well to recall that a perfect number is one which is the half of the sum of its divisors. The converse of the theorem in the text, viz. that $2^n(2^{n+1} - 1)$, when $2^{n+1} - 1$ is a prime, is a perfect number, is enunciated and proved by Euclid in the 36th (the last proposition) of the 9th Book of the "Elements," the second factor being expressed by him in the sum of a geometric series whose first term is unity and the common ratio 2. In Isaac Barrow's English translation, published in 1660, the enunciation is as follows:—"If from a unite be taken how many numbers soever 1, A, B, C, D, in double proportion continually, untill the whole added together E be a prime number; and if this whole E multiplying the last produce a number F, that which is produced F shall be a perfect number."

The direct theorem that every even perfect number is of the above form could probably only have been proved with extreme difficulty, if at all, by the resources of Greek Arithmetic. Euler's proof is not very easy to follow in his own words, but is substantially as follows:

Suppose P (an even perfect number) $= 2^n A$. Then, using in general $\int X$ to denote the sum of the divisors of X,

$$2 = \frac{\int P}{P} = \frac{\int 2^n \cdot \int A}{2^n A} = \frac{2^{n+1} - 1}{2^n} \cdot \frac{\int A}{A}.$$

Hence $$\frac{\int A}{A} = \frac{2^{n+1}}{2^{n+1} - 1}, \text{ say } = \frac{Q+1}{Q}.$$

Hence $A = \mu Q$, and $\int A = 1 + \mu + Q + \mu Q + \ldots$ (if μ be supposed > 1). Hence unless $\mu = 1$ and at the same time Q is a prime

$$\int A > \mu(Q + 1),$$

i.e. $\dfrac{\int A}{A}$ is greater than itself.

Hence an even number P cannot be a perfect number if it is not of the form $2^n(2^{n+1} - 1)$, where $2^{n+1} - 1$ is a prime, which of course implies that $n + 1$ must itself be a prime.

It is remarkable that Euler makes no reference to Euclid in proving his own theorem. It must always stand to the credit of the Greek geometers that they succeeded in discovering a class of perfect numbers which in all probability are the only numbers which are perfect. Reference is made to *so-called* perfect numbers in Plato's "Republic." H, 546 B, and also by Aristotle, Probl. I E 3 and "Metaph." A 5, which he attributes to Pythagoras, but which are purely fanciful and entitled to no more serious consideration than the late Dr. Cummings's ingenious speculations on the number of the Beast. Mr. Margoliouth has pointed out to me that Muhamad Al-Sharastani, in his "Book of Religious and Philosophical Sects," Careton, 1856, p. 267 of the Arabic text, assigns reasons for regarding all the numbers up to 10 inclusive as perfect numbers. My particular attention was called to perfect numbers by a letter from Mr. Christie, dated from "Carlton, Selby," containing some inquiries relative to the subject.

being a less hackneyed letter than Euler's ϕ, which has no claim to preference over any other letter of the Greek alphabet, but rather the reverse.

It is easy to prove that the half of any perfect number must exceed in magnitude its totient.

Hence, since $\frac{3}{2} \cdot \frac{5}{4}$ is less than 2, it follows that no odd two-fold perfect number exists.

Similarly, the fact of $\frac{3}{2} \cdot \frac{7}{6} \cdot \frac{11}{10}$ being less than 2 is sufficient to show that 3, 5 must be the two least elements of any three-fold perfect number; furthermore, $\frac{3}{2} \cdot \frac{5}{4} \cdot \frac{17}{16}$ being less than 2, shows that 11 or 13 must be the third element of any such number if it exists[1]—each of which hypotheses admits of an easy disproof. But to disprove the existence of a four-fold perfect number by my actual method makes a somewhat long and intricate, but still highly interesting, investigation of a multitude of special cases. I hope, *numine favente*, sooner or later to discover a general principle which may serve as a key to a universal proof of the non-existence of any other than the Euclidean perfect numbers, for a prolonged meditation on the subject has satisfied me that the existence of any one such—its escape, so to say, from the complex web of conditions which hem it in on all sides—would be little short of a miracle. Thus then there seems every reason to believe that Euclid's perfect numbers are the only perfect numbers which exist!

In the higher theory of congruences (see Serret's "Cours d'Algèbre Supérieure") there is frequent occasion to speak of "a number n which does not contain any prime factor other than those which are contained in another number M."

In the new nomenclature n would be defined as *a number whose elements are all of them elements of* M.

As τN is used to denote the totient of N, so we may use μN to denote its multiplicity, and then a well-known theorem in congruences may be expressed as follows.

The number of solutions of the congruence

$$x^2 - 1 \equiv 0 \pmod{P}$$

is $2^{\mu P}$ if P is odd,

$2^{\mu P - 1}$ if P is the double of an odd number,

$2^{\mu P}$ if P is the quadruple of an odd number,

and $2^{\mu P + 1}$ in every other case.

In the memoir above referred to, Euler says that no one has demonstrated whether or not any odd perfect numbers exist. I have found a method for determining what (if any) odd perfect numbers exist of any specified order of manifoldness. Thus, *e.g.*, I have proved that there exist no perfect odd numbers of the 1st, 2nd, 3rd, or 4th orders of manifoldness, or in other words, no odd primary, binary, ternary, or quaternary number can be a perfect number. Had any such existed, my method must infallibly have dragged each of them to light.[2]

In connection with the theory of perfect numbers I have found it useful to denote $p^i - 1$ when p and i are left general as the Fermatian function, and when p and i have specific values as the ith Fermatian of p. In such case p may be called the base, and i the index of the Fermatian.

[1] 3, 5, 7 can never co-exist as elements in *any* perfect number as shown by the fact that $\frac{1+3+3^2}{9} \cdot \frac{1+5}{5} \cdot \frac{1+7+49}{49}$; *i.e.* $\frac{26}{15}\left(1 + \frac{1}{7} + \frac{1}{49}\right)$ is *greater* than 2. Thus we see that no perfect number can be a multiple of 105. So again the fact that $\frac{5}{4} \cdot \frac{7}{6} \cdot \frac{11}{10} \cdot \frac{13}{12} \cdot \frac{17}{16} \cdot \frac{19}{18}$ is *less* than 2 is sufficient to prove that any odd perfect number of multiplicity less than 7 must be divisible by 3.

[2] I have, since the above was in print, extended the proof to quinary numbers, and anticipate no difficulty in doing so for numbers of higher degrees of multiplicity, so that it is to be hoped that the way is now paved towards obtaining a general proof of this *palmary* theorem.

Then we may express Fermat's theorem by saying that *either the Fermatian itself whose index is one unit below a given prime or else its base must be divisible by that prime.*[1]

It is also convenient to speak of a Fermatian divided by the excess of its base above unity as a Reduced Fermatian and of that excess itself as the Reducing Factor.

The spirit of my actual method of disproving the existence of odd perfect numbers consists in showing that an n-fold perfect number must have more than n elements, which is absurd. The chief instruments of the investigation are the two inequalities to which the elements of any perfect number must be subject and the properties of the prime divisors of a Reduced Fermatian with an odd prime index.

New College, November 28. J. J. SYLVESTER.

[1] So too we may state the important theorem that if *an element of a Fermatian is its index the component which has that index for its base must be its square.*

COUTTS TROTTER.

A GREAT calamity has fallen on the University of Cambridge and on Trinity College, and many men differing widely in their interests and callings are bearing together the burden of a common sorrow in the knowledge that the Rev. Coutts Trotter, the Vice-Master of Trinity College, was no more. Mr. Trotter suffered from a severe and prolonged illness during last winter and early spring, and though in the summer he seemed to have almost regained his health, he began as the year advanced once more to lose ground. When he returned from abroad in October his condition gave rise to great anxiety among his friends; as the term went on he grew worse rather than better; and an attack of inflammation of the lungs rapidly brought about the end, which took place in his rooms in College, in the early morning of Sunday, December 4.

During the last twenty of the fifty years of Mr. Trotter's life both the University of Cambridge and Trinity College have undergone great and important changes. In bringing about these changes Mr. Trotter had a great share, perhaps a greater share than any other individual member of the University; and while those changes are probably neither wholly good nor wholly evil, but good mixed with evil, no one hand, as the changes were being wrought, did so much good and so little evil as his. A wide and yet accurate knowledge of many different branches of learning, a genuine sympathy with both science and scholarship, a judicial habit of mind which enabled him to keep in view at the same time broad issues and intricate details, a clear insight into the strength and weakness of academic organization, and a singular skill in drafting formal regulations,—these qualities, aided by a kindly courtesy which disarmed opponents, and a patience which nothing except perhaps coarse rudeness could ruffle, enabled him in his all too short life to do for his College and for his University more than it seemed possible for one man to do.

The academic labours which thus year by year increased upon him, though they in many ways, both directly and indirectly, tended to the advancement of science, became, increasingly, hindrances to his pursuing actively any special path of scientific inquiry, as he had once hoped to do. His love of science began with his boyhood, while he listened to the Royal Institution lectures of Faraday. Having taken a degree, with honours in both classics and mathematics, and having obtained a Fellowship at Trinity, he gave up to scientific study much of the leisure thus afforded to him; and, in order more thoroughly to train himself, spent the best part of two years at Heidelberg, during a portion of which time he was engaged in physiological research under Helmholtz. He acquired a very

considerable knowledge of chemistry and botany, but afterwards confined his attention more especially to physics, and lectured experimentally on this subject for several years in Trinity College. In his earlier days he was an enthusiastic Alpine climber, and this led him to direct his knowledge of physics towards the solution of glacial problems. He commenced a few years ago, in the ice-caves of Grindelwald, a series of observations on the physical properties of ice, some of the initial results of which were communicated to the Royal Society. He was never able, however, to continue, much less to complete, these observations, and perhaps the cruellest feature to him of his illness last winter was that it prevented him from spending the Christmas vacation at Grindelwald, as he had hoped to do, in carrying on measurements of ice, under the most natural conditions, in the depths of an ice-cave.

But the gain to science from Trotter's life is not to be measured by his formal contributions to scientific literature. He had a great unwillingness to write "papers." Though he served for several years as one of the secretaries, and at the time of his death was President, in the second year of office, of the Cambridge Philosophical Society, whose very life consists in scientific research, and though in the discussions at the meetings he frequently made his critical power felt, his name does not often appear in the Society's publications. He was especially interested in physiological optics, but, though he made many observations, was always disinclined to commit his results to paper. His real scientific usefulness is to be seen in his University and College work. The recent development of natural science (other than mathematical) at Cambridge is coincident in time with, and in great measure due to, Trotter's academic activity.

In the encouragement given at Trinity to natural science, in all the changes of University ordinances tending to encourage scientific research, and to place the teaching of science on a broader and firmer basis, it is easy to trace his hand. He did not always have his own way, and often thought it prudent to accept an arrangement the shortcomings of which he clearly saw; but his influence, becoming more and more powerful year by year, was always exerted to promote the growth of science in the University, for he at least had no doubt that he was thus working for the welfare both of the University and of his College. He had such a firm grasp of the dominant ideas, and was so wholly in touch with the spirit, of almost every one of the various branches of science, that each teacher and worker sought his help and trusted in his counsel. On the other hand, his conspicuous sympathy with literature and art enabled him to win from those who were strangers to science an assent which would have been denied to claims advocated by others. Happily, too, his singularly catholic mind and temper were made still more potent by a remarkable skill in handling details and conducting business. Were Maxwell now alive, he would be able to tell, as Rayleigh and Thomson can tell, how great a help Trotter was to the Cavendish Laboratory and to physics. The Regius Professor of Physic knows how often Trotter's great knowledge of the needs of medicine on the one hand, and of the capabilities of academic organization on the other, as well as his legislative ability, were of signal service in the difficult deliberations of the Board of Medical Studies. Liveing can say how much not only the very existence, but the details of construction, of the new Chemical Laboratory are due to Trotter's co-operation with himself, and Stuart will tell a like story about the Engineering School. Each science in turn brought its wants to Trotter, and seldom brought them in vain. He recognized Frank Balfour's powers as early as I did, and did more for him in his College and in the University than I could do. All my younger friends whom I am proud to think of as once my pupils, who are making their names known in physiology, in morphology, and in botany, have always looked up to him as a friend who never failed. And, as for myself, whatever I may have done at Cambridge has been done from first to last through him, and could not have been done without him: in him I have lost my oldest, truest, best helpmate.

I first came to know him a year or so before I received my appointment at Trinity College. Happening to pay a visit to Prof. Humphry, I was taken by him to call on "a young Fellow of Trinity interested in science, and especially in physiology, a capital fellow!" That "young Fellow" was Trotter. I saw, even in our brief interview, much in him to draw me to him, and he seemed to see something of the same kind in me, so that when, a year after, a sudden change in all my plans placed me within the walls of Trinity, he and I began a friendship which has ceased only with his death. All through the thirteen years during which, while working within the University, I was really outside the University, my every movement was made by and through Trotter; and since I have been Professor my every movement has been made with him. For seventeen years I have been able to make him a partner in my plans; he has shared in my hopes and soothed me in my failures; where I have been successful he has helped, and when I have refused or neglected his counsel I have generally gone wrong. When Balfour was taken I could feel that Trotter was left, and now he is gone too.

But I ought not to thrust these personal matters on the readers of NATURE, and indeed, great as my own loss is, that of Trinity College and of the University is far greater. Those who know the University and knew Trotter will feel at once how great a blow is his death at the present juncture. The University, both in its scientific and in its other work, is straitened for lack of funds: laboratories cannot be built, teachers cannot be adequately paid, research cannot be properly encouraged, because the necessary money is not at hand. At the same time the revenues of the several Colleges are suffering acutely from the depreciation in the value of land, and a movement has been set on foot with the view of diminishing the contributions of the Colleges to the University. If this movement is successful—and its success seems assured by the fact that the new Member for the University has, in his address to the electors, given a conspicuous pledge that he, with his commanding scientific authority, will support it in Parliament—it will need the wisest and most skilful handling of details to prevent the result proving disastrous to the cause of learning, and especially of scientific learning, in the University. So long as Trotter was alive we felt that we had one in whom devotion to his College was no less strong than his love for the University and for learning, and we looked to him as the man who, trusted alike by the Colleges and by the University, would be found to have skill to steer us in the difficult way before us. Now, in the darkness of his death, we seem to be driving, without a pilot, straight upon the rocks.

M. FOSTER.

H. C. F. C. SCHJELLERUP.

THE Danish astronomer Prof. Hans Carl Frederick Christian Schjellerup died at the Copenhagen Observatory on November 13 after a prolonged illness. He was born on February 8, 1827, at Odense, where his father was a jeweller, and was apprenticed to a watchmaker, but by private study he succeeded in supplementing the education he had received in his native town so well that he was able to pass the entrance examination at the Polytechnic School of Copenhagen in 1848. Here he distinguished himself by his mathematical ability, and was able to finish his studies in the course of two years, when he passed the final examination in applied mathematics and mechanics. In 1851 he

was appointed observer in the old Observatory at Copenhagen, which had been built at the time of Longomontanus, on the top of a high tower, and was therefore, after the lapse of two centuries, greatly behind the times, both as to locality and instruments. A few years afterwards he was appointed Professor of Mathematics at the Naval Academy, and Instructor in Geometrical Drawing at the Polytechnic School. These appointments he retained till the time of his death, as well as his position at the Observatory, and though he was in 1875, after the death of Prof. D'Arrest, strongly urged by the Minister of Public Instruction to allow himself to be appointed Professor of Astronomy, he preferred remaining as he was, partly owing to the pecuniary loss the change would have entailed, partly because his scientific activity was untrammelled by routine duties, and left him leisure to pursue his studies in whatever direction he chose.

As long as Schjellerup had only at his disposal the instruments of the old Observatory, he chiefly occupied himself with the computation of orbits of planets and comets, among which his determination of the orbit of the comet of 1580 deserves to be specially mentioned. This was founded on a complete reduction of Tycho Brahe's original observations of the distance of the comet from stars, and of his time determinations by altitudes and azimuths of standard stars. In 1861 the new Observatory was finished, and furnished with an 11-inch refractor by Merz and a transit-circle by Pistor and Martins. With the latter Schjellerup commenced in September 1861 to observe zones of stars, chiefly of the eighth and ninth magnitudes, between $+15^\circ$ and -15° declination, and already in the beginning of December 1863 he had finished the observation of ten thousand stars, while the reductions had been pushed on with so much energy that the complete catalogue of mean places for 1865 was laid before the Royal Danish Society of Science a month after. When it is remembered that the author of this work during the greater part of the year had to spend three or four hours a day in teaching, it will be conceded that he made good use of his time. The star catalogue is so well known for its fulness and accuracy that it is unneccessary to dwell further on it in this place. After its completion, Schjellerup intended, and had already commenced, to continue the observations north of the limit of $+15^\circ$, as Bessel had done forty years before, but about this time his interests took a new direction, which made him discontinue systematic observations, while he may also have been influenced by the circumstance that the great undertaking of the Astronomische Gesellschaft, viz. the observing of all stars in the northern hemisphere down to the ninth magnitude, had just then been planned, whereby zone work on Lalande's plan became of less importance.

Schjellerup now with his usual energy threw himself into the study of Oriental languages, especially Arabic and Chinese. In the Royal Library of Copenhagen he found a manuscript of the description of the heavens by the Persian astronomer Abd-al-Rahman al-Sûfi, a work which up to that time had been very little known among astronomers. Finding that it contained a complete and careful uranometry from the tenth century, or in other words from an epoch nearly equidistant between Ptolemy and Argelander, he resolved to translate it and was fortunate enough to obtain the use of another manuscript from the Imperial Library of St. Petersburg. The work was published in 1874 by the Academy of St. Petersburg under the title, "Description des étoiles fixes composée au milieu du dixième siècle de notre ère par l'astronome Persan Abd-al-Rahman al-Sûfi." It has been found most valuable by the astronomers who of late years have studied the relative brilliancy of the fixed stars, and Sûfi's results have been systematically collated with their own by Messrs. Peirce, Pritchard, and Pickering.

The great value which this old work was found to possess for modern research induced Schjellerup to endeavour to make other observations of the ancient astronomers fruitful for the investigations of the present day. To the journal *Copernicus* he contributed three papers under the common title, "Recherches sur l'Astronomie des Anciens." The first shows that the time stars of Hipparchus had been so well selected that their culminations gave the correct time every hour of the night within a minute, the second discusses the Chinese observations of the total eclipses of the years −708, −600, and −548, while the third compares seven conjunctions of the moon with fixed stars recorded by Ptolemy, with Hansen's lunar tables. He further examined the occultations and conjunctions of planets observed by the Greek astronomers, and the principal eclipses of the Middle Ages, but these investigations appear to have been left unfinished at his death.

Among Schjellerup's minor publications should be mentioned his catalogue of red stars (first published in 1866, and in a revised edition in 1874), which appeared most opportunely at a time when the spectroscope had just commenced to be applied to the study of the physical constitution of the stars.

In addition to being a man of very extensive knowledge, both scientific and general, Schjellerup was a kind teacher and friend, always willing to assist with his vast store of learning anybody who consulted him. His memory will be gratefully cherished by those who had the good fortune to know him. J. L. E. DREYER.

NOTES.

DR. ASA GRAY, we are sorry to learn, has been stricken with apoplexy at his house in Cambridge, Massachusetts.

SIR GEORGE BURROWS, F.R.S., died on Monday. He was in his eighty-seventh year.

WE regret to have to announce the death, at the early age of thirty-four, of Prof. Humpidge, of the University College of Wales. Dr. Humpidge was educated at the Grammar School, Gloucester, was for some years in trade, and in spare time student in science classes, where he obtained a silver medal in geology from the Department. He afterwards studied at the School of Mines, and obtained one of the three Jodrell Scholarships. In the examination for B.Sc. at the London University he obtained the second place in the honours list, and the two years' £40 scholarship. After studying with Bunsen at Heidelberg, and teaching at Hofwyl in Berne, he was appointed in 1879 to the chemistry class at Aberystwith. At Kensington Dr. Humpidge carried on some researches on the coal-gas of the metropolis, under Prof. Frankland, and in Heidelberg he took up the study of the rare metals yttrium, erbium, and beryllium, results of which were published in the Journal of the Chemical Society, Philosophical Transactions, and Proceedings of the Royal Society. His later work was the preparation of several rare metals in a state of purity for the determination of their specific heats in his calorimeter. The fire which unfortunately destroyed the College in the summer of 1885 caused irreparable loss to Dr. Humpidge, all his papers and results and chemicals being burnt, and he had also a very narrow escape from the flames in endeavouring to rescue people and property. The shock of this accident undermined his health, and although he continued to teach in temporary premises for some time he was finally obliged to visit the South of Europe for a winter, but the relief was only temporary, and he succumbed, after three weeks of great suffering, on November 30. Dr. Humpidge translated Kolbe's "Inorganic Chemistry," which has reached its second edition. Unfortunately his long illness ran away with any provision

that may have been made for his wife and family (two children of three and five years), and their sad condition calls for the consideration of his scientific colleagues.

IN opening the exhibition, at the People's Palace, of the work of London apprentices, on Saturday, the Prince of Wales delivered an excellent speech on technical education. He was able to announce that, thanks to the generosity of the Drapers' Company and the Charity Commissioners, the People's Palace will soon be on a permanent footing. He also stated that the Ironmongers' Company and the Charity Commissioners are to co-operate for the establishment, in some other part of London, of an institution corresponding to the People's Palace—an institution for providing technical, scientific, commercial, and artistic education united with physical and social recreation.

A COMMITTEE, consisting for the most part of members of the two Commissions which presided over the Prehistoric and Ethnographic Sections in the Paris Exhibition of 1878, has been appointed by the French Minister of Commerce and Industry to preside over Section I. of Technological History at the Exhibition of 1889. This department, which will be located in the so-called Palais des Arts libéraux, will represent physical, or technical, anthropology, prehistoric archæology, and ethnography. The four other Sections connected with anthropological and ethnographic sciences will respectively illustrate the liberal arts, arts and trades, means of transport, and military arts. The President of the Committee is M. de Rozière, and the Acting Secretary M. P. Topinard, editor of the *Revue d'Anthropologie*, through whose pages an appeal is made to foreign as well as French anthropologists for contributions to this Section of the coming Exhibition, such, more especially, as casts of skulls and other parts of the body by which racial types can be best illustrated.

THE Chief Signal Officer of the United States has issued his Report for the fiscal year ending June 30, 1887. The Report shows that there has been a growing demand for weather forecasts: as a rule, predictions are made for forty different districts. The system of cold-wave warnings continues in successful operation: these warnings imply that the temperature will fall below 45°, and that in twenty-four hours an abnormal fall of 15° or more will occur. Such predictions are valuable both as regards agricultural interests and personal comfort. A bulletin showing the effect of the weather for the previous seven days on important growing crops is now issued once a week. The State services play an important part in the meteorological organizations of the United States. These now number nineteen, in addition to the New England Meteorological Society. It is recommended that the attention of Congress be called to the propriety of making an appropriation for the service of telegrams now sent from the United States to Europe, in view of their importance to ship-masters of all countries.

AT the meeting of the French Meteorological Society, on November 9, attention was drawn to the establishment of a meteorological station at Bagnères-de-Bigorre. This station is of importance from its position at the foot of the Pic-du-Midi, being about 7540 feet below the mountain observatory. M. Teisserenc de Bort submitted an atlas of maritime meteorology, which has just been published with the co-operation of the Central Meteorological Office of France.

ON November 16, Dr. Buys Ballot, Director of the Royal Meteorological Institute of the Netherlands, was presented with a gold medal, at a banquet held in his honour, as a mark of respect on his completion of forty years of eminent services (1847–87). The meeting was attended by men of science from various countries.

Ciel et Terre of November 1 discusses an investigation of the surface temperature of the ocean, by Prof. O. Krümmel, in the *Zeitschrift für Wissenschaftliche Geographie*, containing charts for February and August, with coloured isotherms for each 2° C., over all oceans. The space occupied in latitude by water of 75° F. is calculated for the Atlantic and Pacific Oceans. Temperatures above 86° F. are found only at isolated points, as on the west coast of Central America, in August. Nearly 40 per cent. of the whole superficies of the ocean, both in February and August, has a temperature above 75°. The low temperature on the west coasts of Africa and South America is attributed by the author to the action of the winds instead of to the action of Polar surface currents, by which it has hitherto been explained.

DURING last autumn the German Fishery Association despatched the steamer *Holsatia* into the Baltic for scientific research, some of the results of which have just been made public. There were on board Prof. Hensen, Dr. Brandt, Dr. Oldenburg, and several officials connected with the German fisheries. The *Holsatia* left Memel on September 14, and, steering in a north-westerly direction, trawled over her course in order to ascertain what fish were most plentiful at that season. This proved to be herring. In the deep channel running to the north-west of Memel, between that city and the Hoberg bank, off the island of Gottland, it was found that the temperature of the sea, at a depth of 142 metres only, was 3° C., whereas at the surface it was 14° C. Several measurements were taken, but with the same result. This spot being one of the deepest in the Baltic, it has been suggested that this abnormal temperature may be caused by some cold under-current coming from the Gulf of Bothnia or the Bay of Finland. From this point the course was shaped for the island of Öland and the fishing-bank called "Mittelbank," soundings being taken throughout. Net-fishing was also carried on, particularly with a so-called "vertical" net, employed for the purpose of ascertaining the nature of the food of fish in certain waters. Some trawling resulted in the bringing up of boulders of a very curious shape, as well as mussels and other marine animals. All the objects brought up were photographed.

IN the December Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, there is an interesting account of cubebs, the value of which has risen rapidly during the last few years. There are also excellent papers on Sabicu wood, Mexican fibre or istle, the food-grains of India, broom root or Mexican whisk, Contrayerva, the introduction of the Brazil nut to the East Indies and Australia, and the Castilloa rubber of Central America.

ANOTHER comprehensive application of the well-known reaction of Messrs. Friedel and Crafts, which has played so remarkable a *rôle* in organic chemistry, has recently been made by M. Léon Roux. In a long but highly interesting communication to the *Annales de Chimie et Physique*, M. Roux describes how he has been enabled, with the aid of that wonderful substance, chloride of aluminium, to extend the bounds of chemistry still further, by preparing a large number of higher homologues of naphthalene. In fact, he has been successful in building up from the heavier molecule of naphthalene an entirely new series of hydrocarbons, analogous in many respects to the series derived, by the earlier use of this reaction, from the lighter molecule of benzene. The insertion of the CH_3 groups, however, is a much more difficult operation in the naphthalene than in the benzene series, and requires a much higher temperature; the homologues themselves, moreover, are much more interesting, inasmuch as two isomeric kinds, α and β, of each are possible. Thus the methyl naphthalene $C_{10}H_7 . CH_3$ formed by the new method was found to consist of a mixture of the α and β isomers, which could be partially separated by taking advantage of

their different points of solidification. Ethyl naphthalene, $C_{10}H_7 . C_2H_5$, was most readily obtained by warming, in a flask connected with an inverted condenser, a mixture of 200 grammes naphthalene, 200 grammes ethyl iodide, and 20 grammes of aluminium chloride, added gradually as the reaction proceeded. The fraction of the product boiling between 249° and 254° was isolated as a colourless highly refractive liquid, exhibiting violet fluorescence, and was shown by analysis, and by the nature of its oxidation products, to consist of almost pure β ethyl naphthalene mixed with a minute quantity of the α compound. In a similar manner, propyl, butyl, amyl, and benzyl naphthalene have been prepared; indeed, there appears to be no limit to the number of naphthalene derivatives possible to be obtained in this manner, and there can be no doubt that M. Roux is perfectly warranted in applying the somewhat exclusive term "classical" to the work of Messrs. Friedel and Crafts, which has led to the synthesis of so large a number of carbon compounds.

AT the meeting of the Helvetic Society of Sciences this year Prof. Weber described a very sensitive micro-radiometer made in the following way:—One arm of a Wheatstone's bridge is formed by a thin tube, which is filled in its middle part with mercury, and at its ends, for about 5 mm., with a solution of zinc sulphate. To each end of the tube is fitted a metallic case, one side of which consists of a plate of rock salt. This case is filled with air, which dilates under the influence of radiation forces back the zinc-sulphate solution in the tube, and thus greatly increases the electric resistance on that side. The apparatus is made symmetrical, to eliminate variations of temperature and pressure. This radiometer will indicate 100-millionths of a degree. The moon's radiation gives a galvanometric oscillation of about five divisions.

IT is estimated that the air in a room becomes distinctly bad for health when its carbonic acid exceeds 1 part in 1000. An apparatus has been recently patented by Prof. Wolpert, of Nürnberg, which affords a measure of the carbonic acid present. From a vessel containing a red liquid (soda-solution with phenolphthalein) there comes every 100 seconds, through a siphon-arrangement, a red drop on a prepared white thread about a foot and a half long, and trickles down this. Behind the thread is a scale beginning with "pure air" (up to 0·7 per 1000) at the bottom, and ending above with "extremely bad" (4 to 7 per 1000 and more). In pure air the drop continues red down to the bottom, but it loses its colour by the action of carbonic acid, and the sooner the more there is of that gas present.

SOME interesting experiments on the reciprocal influence of organs of sense have been recently made by Herr Urbanschitsch, of Vienna. His general conclusion is that any sense-excitation has for result an increase of the acuteness of other senses. Thus, sensations of hearing sharpen the visual perceptions. If coloured plates are placed at such a distance that one can hardly distinguish the colours, and various sounds are then produced, the colours become generally more distinct the higher the sounds. Similarly, one can, while a sound affects the ear, read words which one could not read before. Again, the ticking of a watch is better heard when the eyes are open than when they are closed. Red and green increase auditive perceptions; but blue and yellow weaken them. Several musicians, however, were agreed that red, green, yellow, and blue caused an intensification of sound about one-eighth; while violet had a weakening effect. Taste, smell, and touch are under like laws. Light, and red and green colour, increase their delicacy; while darkness, blue, and yellow diminish it. Under the influence of red and green, taste extends from the anterior border of the tongue to the whole surface. On the other hand, a strengthening of smell, taste, or touch, exalts the other sensitive perceptions. Specially interesting is the reciprocal influence of touch and the sense of temperature. If one tickle the skin with a hair, and plunge the hand in hot water, the tickling sensation ceases; on the contrary, if the hand be placed in cold water, and a part of the body tickled, the temperature is felt more vividly. Herr Urbanschitsch finds in this reciprocal action an explanation of supposed double consecutive sensations on excitation of one sense.

PROF. LINDEMAN contributes to the last two issues of the *Bulletin de la Société des Naturalistes de Moscou* (1887, Nos. 2 and 3) two very elaborate papers on the Hessian fly. He points out that there can be no universal remedies for this pest, because the manner of life of the Hessian fly, and the conditions of its multiplication, vary to some extent in different climates. His study of the Hessian fly in the neighbourhood of Moscow has enabled him to describe at length the conditions which are, and those which are not, favourable for its development in that district. About Moscow it never propagates on any of those plants —Gramineæ or others—which grow amidst the crops of the Russian corn-fields. Of the three generations which develop there—the spring generation, from the beginning of May to the beginning of June (old style), the summer one, from June 19 to the beginning of August, and the autumn one, to the end of August—each must find for its propagation green stems of rye, wheat, or barley; and these stems must remain green and succulent throughout the twenty-eight days that the larva is living. Of insects which hunt the larvæ of the Hessian fly, *Geophilus*, the larva of a *Cantharid*, and one mite are noticed. The parasitic Pteromalines of the fly have been described by the same author in the first number of this year's *Bulletin*.

WE have received the last number of the Transactions of the Asiatic Society of Japan (vol. xv. Part 1), in which the well-known Chinese scholar Mr. E. H. Parker discusses in two papers the relation between the Japanese language and the languages of the neighbouring continent. He comes to the conclusion, after an elaborate examination of a list of a thousand Japanese words, that a great part of the modern Japanese language may be traced back to a language common with that language from which the modern dialects of China have all been derived. Mr. Walter Dening gives an abstract of the rules, an account of the general work, and a list of the papers published in the Proceedings of a Japanese Society established for the discussion and elucidation of various educational questions; or, in the words of its rules, "to raise the standard of scholarship and supply the wants of the teacher and reformer." Amongst the papers which have been published by the Society we select the titles of a few in order to show its scope:—"Female Education"; "An Account of the Origin and Development of Natural History in Japan"; "The Compilation of a Japanese Grammar"; "On Sending Students of Natural History to China and Corea"; "The Connection of Clothing and Health"; "Iron Ore"; "The Origin of Certain Customs"; "The Five Races of China." So far, eight parts of the Society's magazine appear to have been published, and these contain about a hundred papers by Japanese scholars of eminence, many of them, like Ito Keisuke, the veteran botanist, bearing names known in Europe.

WE have received Nos. 31–45 of "Länderkunde des Erdteils Europa," a valuable and most interesting work, edited by Prof. Alfred Kirchhoff, which is being issued in "Lieferungen." Prof. Kirchhoff is aided by many eminent writers. The publishers are F. Tempsky, of Vienna and Prague, and G. Freytag, of Leipzig.

TWO papers just printed in the Philosophical Transactions of the Royal Society have been sent to us—"Some Anomalies in the Winds of Northern India, and their Relation to the Distribution of Barometric Pressure," by S. A. Hill; and "Studies on some New Micro-organisms obtained from Air," by Grace C. Frankland and Percy F. Frankland.

MR. F. MOORE, having completed the "Lepidoptera of Ceylon," has now in preparation a much more extensive work comprising the Lepidopterous insects of the entire Indian region. It will be issued in monthly parts, to subscribers only, by the publishers of his previous work, Messrs. L. Reeve and Co.

MR. H. T. OMMANEY, C.S., of Karwar, has sent to the Bombay Natural History Society a full-grown live specimen of the Hamadryad, or King Cobra (*Ophiophagus elaps*). The reptile, which measures about 12 feet in length, is jet black, with faint cream-coloured bars across its back. The throat is of a golden-yellow colour.

A NEW "Catalogue of Mathematical Books," including many of the works of the old mathematicians, has been issued by Messrs. Macmillan and Bowes, Cambridge.

DR. OVERBECK, who owns part of the collections that originally belonged to Alexander von Humboldt, has sent a report about them to the Saxe-Thuringian Naturalists' Society at Halle. He enumerates 290 objects. Dr. Overbeck intends to present Humboldt's collection of minerals to the Mineralogical Museum of Halle University.

THE additions to the Zoological Society's Gardens during the past week include three American Flying Squirrels (*Sciuropterus volucella*) from Florida, presented by Mr. Henry D. Harrison; two Great Eagle Owls (*Bubo maximus*), European, deposited; two Common Wolves (*Canis lupus* ♂ ♀), European, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE NATAL OBSERVATORY.—Mr. Neison, Superintendent of this Observatory, has issued his Report for 1886, and it appears from it that the astronomical work during that year was almost wholly confined to routine observations with the transit instrument, though the meteorological observations were carried on as usual. This partial suspension of activity was due to the fact that only one assistant is now on the staff, and that, through the severe illness of the Superintendent during the first part of the year and his enforced absence in England during the latter part, the assistant, Mr. Grant, was left practically single-handed. The present year will probably show better results, as Mr. Neison returned to his post before the close of 1886, and several needed instrumental improvements and repairs had been successfully carried out. Mr. Neison had commenced an important work connecting the fundamental declinations of the star catalogues of the northern and southern observatories, by means of observations of the differences in zenith distance between 32 selected stars which cross the meridians of the great northern observatories near their zeniths on the one hand, and a set of corresponding southern circumpolar stars on the other.

OLBERS' COMET, 1887.—The following ephemeris for Berlin midnight for this object is in continuation of that given in NATURE of December 1, p. 37:—

1887.	R.A. h. m. s.	Decl. ° ′	Log r.	Log Δ.	Brightness.
Dec. 17	16 7 41	2 47·2 N.	0·1990	0·3593	0·63
19	16 12 21	2 18·4			
21	16 16 56	1 50·5	0·2090	0·3645	0·59
23	16 21 25	1 23·5			
25	16 25 48	0 57·3	0·2190	0·3695	0·55
27	16 30 6	0 31·9			
29	16 34 19	0 7·4 N.	0·2290	0·3741	0·51
31	16 38 27	0 16·2 S.			
1888.					
Jan. 2	16 42 30	0 39·0	0·2389	0·3783	0·48
4	16 46 27	1 1·2			
6	16 50 20	1 22·7 S.	0·2486	0·3821	0·45

PROBABLE NEW CLASS OF VARIABLE STARS.—The Rev. T. E. Espin considers that a number of our variable stars possess characteristics which justify their being formed into a separate class. They are irregular both in period and variation, the latter being usually about 1½ mag., and they show spectra of Secchi's fourth type, *i.e.* like No. 152 Schjellerup. Their changes in brightness are rapid and uncertain. Mr. Espin names 19 Piscium, Birmingham 277, 521, 535, 541, and Espin 116, 154, as belonging to this new class, which perhaps embraces also Birmingham 85, 120, 121, 240, 290, 418, 464, 483, and 502.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 DECEMBER 18-24.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on December 18

Sun rises, 8h. 4m.; souths, 11h. 56m. 45·5s.; sets, 15h. 50m.: right asc. on meridian, 17h. 43·9m.; decl. 23° 24′ S. Sidereal Time at Sunset, 21h. 38m.

Moon (at First Quarter on December 22, 7h.) rises, 10h. 48m.; souths, 15h. 23m.; sets, 20h. 4m.: right asc. on meridian, 21h. 10·7m.; decl. 16° 43′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	6 35	10 44	14 53	16 31·1	21 4 S.
Venus ...	3 48	8 48	13 48	16 34·5	12 16 S.
Mars ...	0 38	6 39	12 40	12 25·6	0 37 S.
Jupiter ...	5 17	9 44	14 11	15 30·8	18 9 S.
Saturn ...	18 59*	2 47	10 35	8 32·8	19 16 N.
Uranus...	1 43	7 16	12 49	13 2·5	5 58 S.
Neptune.	14 16	21 56	5 36*	3 44·3	18 1 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultation of Star by the Moon (visible at Greenwich).

Dec.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
18	ι Capricorni	4½	17 58	18 59	103 350

December 22.—Sun at greatest declination south; shortest day in northern latitudes.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m
U Cephei	0 52·3	81 16 N.	Dec. 21,	23 44 *m*
λ Tauri	3 54·4	12 10 N.	,, 18,	22 54 *m*
			,, 22,	21 46 *m*
ζ Geminorum	6 57·4	20 44 N.	,, 19,	22 0 *m*
			,, 24,	22 0 *M*
R Canis Majoris	7 14·3	16 11 S.	,, 19,	2 19 *m*
			,, 20,	5 35 *m*
S Cancri	8 37·5	19 26 N.	,, 21,	23 57 *m*
δ Libræ	14 54·9	8 4 S.	,, 18,	22 2 *m*
U Coronæ	15 13·6	32 4 N.	,, 21,	18 29 *m*
R Serpentis	15 45·5	15 29 N.	,, 21,	*M*
β Lyræ	18 45·9	33 14 N.	,, 18,	2 0 *m*
Y Cygni	20 45·6	34 10 N.	,, 20,	21 51 *m*
			,, 23,	21 45 *m*
δ Cephei	22 25·0	57 50 N.	,, 23,	2 0 *M*

M signifies maximum; *m* minimum.

Meteor-Shower.

	R.A.	Decl.
Near λ Ursæ Majoris	130°	49° N.

GEOGRAPHICAL NOTES.

THE new number of *Petermann's Mitteilungen* contains a letter from Dr. Hans Meyer, written from Taveta, at the foot of Kilimanjaro, giving some details of his ascent of that mountain, and the results of his observations; it is accompanied by a sketch-map. Dr. Meyer, with one white companion and twenty-two natives, started from Mareale's village, at the south foot of the mountain, in the beginning of July, and proceeded to mount the southern slopes. At 1800 metres the last bananas were passed, and at 2000 metres the saturated forest belt was entered, which on the second day was left behind. Immediately above this stretches a broad belt of grass,

and here a north-west line was struck, and for two days the upper edge of the forest was skirted. On the second day Johnston's old camp was reached, where in the water-courses an abundance of large Ericaceous plants was found growing. Here the two beautiful peaks were seen for the first time, and thenceforth only partial glimpses were obtained through the prevailing clouds. Only eight men would go further than this, and when the snow-line was reached five of them refused to go further. On the third day a northerly route was taken over grass-covered lava-fields, with snow-streams sometimes cutting their channels 50 metres deep into the lava. Dr. Meyer made for the saddle which joins the two peaks of Kibo on the west and Kimawenzi on the east. After 6000 paces a level spur of the saddle was reached, where between the great blocks of lava the green meadows marked the upper course of the snow-streams. Here the last traces were seen of *Senecio Johnstoni* in the bed of a brook about 4000 metres high. About 2000 paces further up great cliffs of lava were met with, and here at the snow-line the tent was pitched. Thence, with his companion and three natives, photographic apparatus, and provisions for three days, Dr. Meyer proceeded to ascend to the Kibo crater. After 3000 paces a wild and shattered hill of lava, whence the lava-stream proceeded, was met with; this was the first of a series of such hills, between which the snow lay thick. Turning to the north-west the party made direct for Kibo over the old lava-streams, and at about 5000 metres reached the last cone of ashes before the ascent to the summit itself. Here the two white men encamped (the natives going back), with a night temperature of -11° C. Early next morning they made directly for the east side of the mountain over debris-covered lava, and came on great snow-fields in the spaces between the lava-hills. After a time sleet came on, and, as the sun got higher, clouds covered the mountain, and the temperature fell from $+8^\circ$ C. to -3°. Dr. Meyer's companion became so exhausted he had to drop behind, and he himself suffered greatly. Proceeding onwards, he met with more extensive snow-fields, and higher still with great ice-blocks, and a less steep stretch covered with ice-debris. Some 20 metres beyond this point he saw a great blue wall of ice rise before him to about 34 or 40 metres high, and evidently stretching all round the crater. In Dr. Meyer's exhausted condition, and without ice-axes, to ascend this wall, which evidently surrounded the crater, was impossible. So, after taking some hasty observations and notes, he began his descent, which was accomplished safely. As the wall seems to extend round the east, south, and west sides of the crater, Dr. Meyer concludes that probably the crater itself is filled with ice. It is remarkable that no snow seems to exist at all on the north side. Dr. Meyer promises to give full details on his return home to Leipzig, and these may render his account more intelligible to Alpinists.

OTHER articles in the new number are on "Temperature Abnormalities on the Earth's Surface," by Herr Rudolf Spitaler, accompanied by a map illustrative of the paper; and "Production of Tin in the Riouw-Tongga Archipelago," by Dr. Posewitz.

LIEUT. WISSMANN, whose health is not good, has given a preliminary account of his journey across Africa to the Berlin Geographical Society. He began with a very brief sketch of the first part of his journeyings, which consisted of his first voyage up the Kassai. By his last journey up the Kassai he has determined that its largest tributary is the Kwango. The Sankuru has only half the volume of water possessed by the Kassai above the confluence of the two rivers. From Lulnaburg, Wissmann began his great forward march to the north of the Sankuru and Lomami. A lengthened stay was made on the Lubi, and after crossing the Sankuru the party entered the region of virgin forests. These were found partially peopled by the savage Batetela and the Batua, the latter being the pygmies described in a previous number. Turning south, Wissmann passed through the territory of the marauding Ben Mona, and where on a former journey he found gigantic villages he now found the place depopulated by war and small-pox. From Nyangwe, Wissmann reached the East Coast by Lakes Tanganyika and Nyassa, and the Zambesi. The latter part of the route was through hitherto unexplored territory. Lieut. Wissmann has been compelled to go to Madeira on account of his health, but we believe there is some likelihood of his appearing at the Royal Geographical Society some time next spring.

FROM the full report of recent explorations in Tierra del Fuego, to which we have recently referred in these notes, we have some further information as to the real character of the region. The reports refer chiefly to the main island, which, instead of being a mountainous region of eternal snow, presents great diversity of surface—high mountains, deep valleys, rolling table-lands, fertile plains, numerous lakes, and frequent water-courses. Occupying a large portion of the extreme north, and extending from one extremity to the other of the straits, are continuous chains of mountains, running into peaks several thousand feet high. Adjacent to these mountains on the south is a wide belt of high and rather barren plain, running the entire width of the island. Then succeed lofty table-lands quite covered with forests. South of this is another chain of sierras, and still further south the country opens into an extensive plain, which occupies all the central portion of the island, and is quite desolate of trees except small patches here and there of hardwood and shrubs. The plain is covered in some parts with an abundance of rich grasses. The extreme south is also mountainous, some of the peaks being volcanic, with numerous glaciers and dense forests. The geological formation of Tierra del Fuego exactly corresponds to that of Patagonia. The broken and disjointed mountains, with wide seas running where they have been depressed, are but the continuation of the Andes; while the plains and uplands partake of the same geological characteristics as the Patagonian steppes.

AT Monday's meeting of the Royal Geographical Society the paper read was by Mr. D. D. Daly on his explorations in British North Borneo, in 1883–87. Mr. Daly's paper consisted mainly of an itinerary with minute details of the economic character of the country through which he travelled, and of the people. He gives some interesting information about the numerous bird-nest caves which he met with, and on the methods of collecting the nests. Most of the people are eager head-hunters, but Mr. Daly made treaties with several of the tribes in which they undertook to give over the practice. Mr. Daly went in both from the east and the west side. In the former journey he went up the River Kinabatangan to the centre of North Borneo; in the latter he went a long distance up the Padas River.

ON THE METEORIC IRON WHICH FELL NEAR CABIN CREEK, JOHNSON COUNTY, ARKANSAS, MARCH 27, 1886.[1]

THE Johnson County meteoric iron, the tenth whose fall has been observed, is of more than ordinary interest, because its fall is so well substantiated, because it is the second largest mass ever seen to fall, and, again, because it fell within five months of the date of the ninth recorded fall, that of the Mazapil. It is almost an exact counterpart of the Hraschina (Agram, Croatia) iron, the first of the recorded falls. The Agram iron fell in two fragments, one weighing about 40 kgm., and the other about 9 kgm., the combined weight being about equal to that of the Johnson County iron.

This mass fell about 6 miles east of Cabin Creek, Johnson County, Arkansas, in longitude 93° 17′ W. of Greenwich, latitude 35° 24′ N., within 75 yards of the house of Christopher C. Shandy. Mrs. Shandy states that about 3 o'clock on the afternoon of March 27, 1886, while in her house, she heard a very loud report, which caused the dishes in the closet to rattle, and which she described as louder than any thunder she had ever heard. At first she thought it was caused by a bombshell, and ran out of the house in time to see the limbs fall from the top of a tall pine-tree, which, she says, stands about 75 yards from her dwelling. She did not investigate the matter until her husband came home, about 6 o'clock in the evening, when, in company with John R. Norton, their hired man, they went out to find the cause of the noise that had so startled Mrs. Shandy. They discovered that a large hole had been made in the ground by some falling object. The iron had buried itself in the ground to the depth of 3 feet, and the earth around it to the thickness of 1 inch seemed to be burned. The ground was still warm when the iron was taken out, and the iron itself was as hot as the men could well handle.

The noise was heard 75 miles away, and was likened to a loud report, followed by a hissing sound, as if hot metal had come in contact with water. It caused a general alarm among the people, and teams of horses 25 miles distant, becoming frightened, broke loose and ran away; and in Webb City, Franklin County, on the south side of the Arkansas River, a number of bells kept on sale in a store are said to have been

[1] From the *American Journal of Science*, vol. xxxiii., June 1887.

caused to tinkle. Cabin Creek is on the north side of the Arkansas River.

Mr. B. Caraway says it was heard by fully 1000 people, and that he heard two loud reports at Alma, Crawford County, 75 miles away, at 3 o'clock on March 27, 1886. The report was also heard at Russellville, and in the adjoining county of Pope.

Prof. H. A. Newton, who has kindly interested himself in this matter, says that the data furnished indicate that the mass must have fallen nearly from the zenith. This was the direction

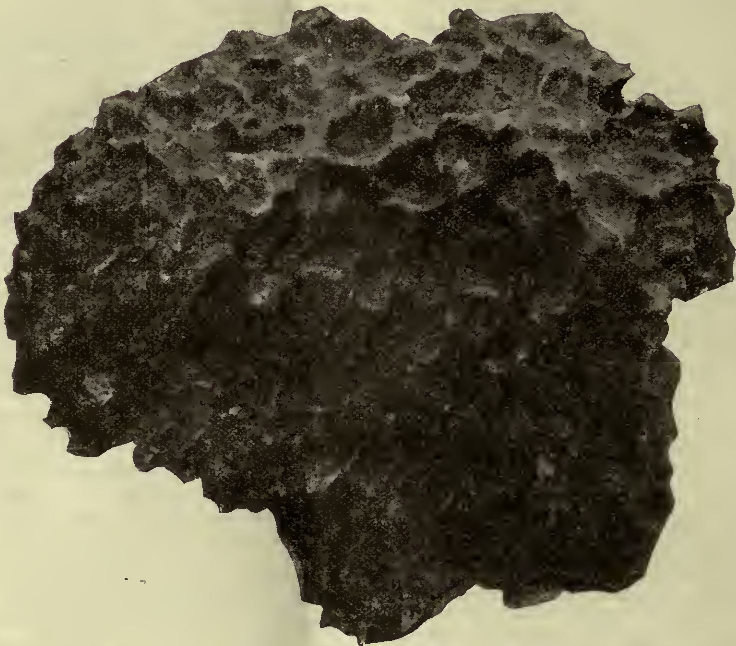


FIG. 1.—Upper Side.

FIG. 2.—Lower Side.

Johnson County, Arkansas. (Scale two-ninths.)

of the end of its path, the earlier portion being more inclined to the vertical, as the path must be affected by gravity and the resistance of the air. The earlier direction must have been from the north-east, and more nearly from the east than the north.

The mass is in general quite flat and very irregular, resembling strongly a mass of molten metal thrown on the ground and then pitted. The illustration of the Agram[2] mass figured

[2] "Beiträge zur Geschichte und Kenntniss Meteorischer Stein- und Metall-massen," by Dr. Carl von Schreibers. Wien, 1820, folio, plate viii.

by Von Schreibers could be mistaken for the upper side of this, were it not that this is larger. It measures 17½ inches (44 cm.) by 15½ inches (39 cm.), while the Agram measures 15½ by 12 inches. A high ridge, 5 inches high at the highest point (12·5 cm.), runs through the centre. One half of the mass is not over 3 inches (7·5 cm.) thick, part of it is only 2 inches (5 cm.), and around the edge it is only 1 inch, or less. It is only exceeded in size, among the irons seen to fall, by the Nejed, Central Arabia, now in the British Museum, which fell in the spring of 1865, and weighs 59·420 kgm. The weight is 107½ lbs. (48·750 kgm.), and it is intact with the exception of three small points, weighing not more than 2 ounces in all, which were broken off. One of these is seen in the etched figure, another was sent to Prof. Clarke by Colonel Betten to be analyzed, and the third piece was lost.

The two sides are wholly dissimilar (see Figs. 1 and 2[1]). In fact, one would scarcely suppose that they belonged to the same mass. The upper side is ridged and deeply dented, while the lower side is flat and covered with shallow but very large pittings. On top the colour is in many places almost tin white without any coating whatever, and the pittings are very deep, and usually quite long, like finger depressions made in potters' clay. These depressions measure from 2 cm. to 4 cm., and from 1 cm. to 4 cm. This side is remarkable for striæ showing the flow and burning, and all running from the centre toward the edge, identical with those in the Rowton, Nedagolla, and Mazapil irons, but on a larger scale. Some of them are thinner than a hair, and yet twice as high (like a high knife-edge), and they are from 1 to 4 inches long. In one space of 5 cm. twenty are arranged side by side, and on one small part which is black, there are fifty lines in 1 inch of space (25 mm.), all running in the same direction. Near all the pointed edges the fused metal has flowed and cooled, so as to hang like falling water. The striæ and marks of flowing are around the edges of the upper surface (Fig. 1). On the under side pittings are very shallow,

FIG. 3.

but much broader, one depression, apparently made up of four pittings, being 20 cm. long, and 9·5 cm. wide. The whole side is coated with a black crust, 1 mm. thick, and having minute round bead-like markings. On one of the indentations of the lower edge the crust has a strikingly fused appearance, as if a flame had been blown on it from the other side. In reality this edge is undoubtedly the place where a greater amount of burning took place when the body was passing through the air. Seven small, bead-like lumps, from 5 mm. to 10 mm. in size, which are visible on this side, are drops of metal that were entirely melted, and flowed and cooled so that they resemble drops of a thick liquid. There are also to be seen what appear to be cracks, fifteen in number, and nearly as thin as a hair. One of these is 10 cm. long, and extends from the highly-fused edge above mentioned towards the centre. The others are from 3 cm. to 5 cm. long. These are so evenly arranged that they are without doubt *Reichenbach lamellen*, in which the inner troilite has been burnt out. If such is the case, they are as abundant as in the Staunton (Va.) meteoric iron.

On the upper side ten nodules of troilite are exposed, measuring from 33 mm. in diameter, to 55 mm. long, and 25 mm. wide. On the lower side there are twelve such nodules exposed, 13 mm. in diameter, while the largest measures 19 mm. by 39 mm. On the upper side these nodules are coated in spots with a black crust, similar to that found on the mass, but on the lower side the crust extends completely around the side of the nodules, showing the fusion very plainly. The troilite is very bright and fresh, like a newly broken mineral, and on the upper side one of the nodules shows deep striation, suggesting that the entire nodule is one crystal, and the exposed part is only one side of it. In some cases where the nodules were broken, they were found to be iridescent. This is one of the octahedral irons showing the Widmanstätten figures beautifully on etching (see Fig. 3), and is one of the Caillite groups of Stanislas Meunier and of the *mittlere lamellen* of Brezina. The lamellæ are 1 mm. wide, and the markings more closely approach the Rowton[1] and Mazapil[2] irons. Fig. 4 shows the etching on the surface of the unpolished exterior, there being no crust. The lower end of the figure, which is flat, was produced by the hammering off of the piece; but the etching was really finer where it was done on the natural surface of the iron. The specific gravity of the small piece figured is 7·773. Troilite, as before stated, is very abundant in the mass. Schreibersite and carbon have also been found

FIG. 4.

between the laminæ. Chlorine is present only in slight quantity, as scarcely any deliquescence has been observed.

The following is a comparative table of analyses of meteoric irons most nearly approaching this in composition :—

	Cabin Creek (Whitfield).	Estherville (Smith).	Mazapil (Mackintosh).	Rowton (Flight).	Charlotte (Smith).
Iron ...	91·87	92·00	91·26	91·25	91·15
Nickel	6·60	7·10	7·845	8·582	8·05
Cobalt ...	trace	0·69	0·653	0·371	0·72
Phosphorus	0·41	0·112	0·30	—	0·06
C, S, &c.	0·54	99·902	100·038	100·203	99·98
	99·42				

GEORGE F. KUNZ.

¹ These figures were made by the Ives process, and are faithful reproductions direct from the photograph.

¹ "Meteoriten Sammlung des k.k. mineralogisches Hofcabinet in Wien." Wien, 1885, 8vo, Plate 2, Fig. 2.

² *American Journal of Science*, III. vol. xxxiii. p. 225, Fig. 2.

THE ROYAL HORTICULTURAL SOCIETY.

THE Council of the Royal Horticultural Society request the horticulturists of the United Kingdom to read and consider the following statement and appeal :—

1. The grounds at South Kensington, known as the Gardens of the Royal Horticultural Society, having been devoted to the Imperial Institute, the Council endeavoured, in obedience to the wishes so graciously expressed by Her Majesty the Queen, the Patron of the Society, to obtain from the Royal Commissioners of the 1851 Exhibition such a site as would justify them in advising the Fellows to remain at South Kensington.

2. The Royal Commissioners were, however, unable to offer any adequate site, and gave the Council distinctly to understand that the erection of offices, committee-rooms, &c., on their land would not be held to confer any claim whatever, either legal or moral, to the use of the Conservatory and Gardens for the purposes of the Society. The negotiations consequently came to an end. An informal offer has since been made by the Royal Commissioners to let a portion of the Gardens and the Conservatory to the Society at a guaranteed rent of £1000 a year, which with rates, taxes, and maintenance would involve an expenditure of £2000 a year at least, a sum far beyond the resources of the Society.

3. The Society has been in existence for eighty-three years, having been founded in 1804, and incorporated by Royal Charter in 1809. It has done much to advance the interests of practical and scientific horticulture, and it is the recognized authority on all horticultural questions. In addition to the valuable work of the Scientific Committee, presided over by Sir J. D. Hooker, K.C.S.I., C.B., F.R.S., new and rare plants, fruits, and vegetables, collected abroad or raised at home, have been continually submitted, in large and increasing numbers, to the judgment of the Fruit and Floral Committees, whose verdicts are accepted without question. The Society has also continuously carried on valuable trials of plants, fruits, and vegetables, at Chiswick. It has published during the last three years the following, viz. :— "Report of the National Apple Congress held at Chiswick, October 1883," "Report of the Orchid Conference held at South Kensington, May 1885," "Report of the National Pear

Conference held at Chiswick, October 1885," "Report of the Primula Conference held at South Kensington, April 1886, and of the Orchid Conference held at Liverpool, June 30, 1886," "Report on the Effects of Frost on Vegetation during the Severe Winters 1879-80, 1880-81, published in 1887."

4. The Council are of opinion that the connection of the Society with South Kensington, however promising at first, has proved adverse to its true interests and permanent welfare. They recognize that altered circumstances require a complete re-organization of the Society on a more popular basis. They believe that, while local Horticultural Societies attract local support, a central Metropolitan Society (to which local Societies may be affiliated) is, in the interests of horticulture, indispensable. Under analogous circumstances the Royal Agricultural Society prospers, although there are local Societies in every county of the Kingdom.

5. The Council do not believe that the Society can be carried on any longer under the trammels of the existing Charter, which was granted in 1860 in view of a wholly different state of things; nor do they think a Charter will be requisite for its future working. They believe that the numbers of the Council should be considerably increased and their mode of election modified and made popular, and that the ordinary work of the Society should be carried on by Committees, under powers delegated to them by the Council. They hold that the Society should henceforth devote itself strictly to the advancement of practical and scientific horticulture.

6. The view of the Council is that the expenditure of the Society should be reduced as much as possible, and its resources devoted to the following objects:—

(1) The maintenance of the Chiswick Gardens and the conduct of plant, fruit, and vegetable trials there; and possibly the establishment of a School of Gardening.

(2) The immediate engagement of such premises in a convenient and central situation as may suffice for office requirements, the safe housing of the Lindley Library, the meetings of the Society's Committees, and its fortnightly shows, to the maintenance of which they attach great importance.

(3) The publication of periodical Reports of the work done at Chiswick, and by the Society's Committees, and on horticultural subjects generally.

7. For many years the nature of the accommodation which the Society has been able to obtain at South Kensington has virtually prevented meetings being held for the discussion by the Fellows of points of interest in the practice of horticulture. It is essential that these meetings should be resumed, and it is believed that they will be of great value in bringing together those who take an active part in British horticulture. It is also hoped that such meetings would give an opportunity for the consideration of the numerous directions in which the rural economy of the country seems likely to be modified by the substitution of horticultural for agricultural methods.

8. The Council would recommend that the subscription should be in future £2 2*s.* for Fellows, and that a grade of Member or Associate, at £1 1*s.*, should be created for professional and practical gardeners, who have rarely hitherto belonged to the Society. They calculate that the maintenance of Chiswick will cost £1500 a year, and that for the other purposes of the Society a further sum of not less than £1500 a year will be required. During 1887, 150 Fellows have paid £4 4*s.*, and 623 Fellows £2 2*s.*, making a total of £1938 6*s.*, a sum altogether insufficient for the working and requirements of the Society.

9. In conclusion, the Council believe that the extinction of the Royal Horticultural Society would be regarded by all interested in horticulture as a national loss. The history of the Society, and the good work it has done and is doing, entitle it to the consideration and support of the horticultural world, to whom the Council make this appeal. They address it with equal confidence to amateurs and to the trade, in the belief that their interests are identical, and that for the protection and advancement of these interests the maintenance of the Royal Horticultural Society is essential. The Council have had difficult duties to perform. While they are willing to continue to discharge these duties, if desired, they believe that the best course would be for them to place their resignations in the hands of the Fellows, at the end of the year, so as to leave the Society entirely unfettered. But they consider it due both to the Fellows and to themselves to say that, unless they receive assurances of adequate support, in response to this appeal, the Society must necessarily come to an end.

10. The favour of an early answer is requested on the inclosed form. The Donations would be devoted to the cost of establishing the Society in its new home and to similar purposes.

On behalf of the Council,

TREVOR LAWRENCE, *President.*

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Thurston Prize at Caius College, value £54, for the best original investigation by a member of the College in the past three years in physiology, pathology, or practical medicine, has been adjudged to Mr. C. S. Sherrington, M.A., M.B., Fellow of the College.

The Sedgwick Memorial Committee having declined to assent to the building of rooms for teaching purposes with the Sedgwick Fund, while waiting the building of a complete museum; and other proposals having been made, a syndicate has been appointed to plan out the entire disposal of the sites surrounding the new museums, so as to satisfy as many scientific requirements as possible.

Mr. E. C. Dowson has been appointed Demonstrator of Mechanism and Applied Mechanics in succession to Mr. Ames.

Next term the General Board of Studies will nominate a University Lecturer in Pure Mathematics, in consequence of the resignation of Mr. Macaulay. The stipend is £50 per annum, and the appointment will be for five years. A preference will be given to a lecturer who would take subjects not at present represented. Among these are theory of equations, theory of numbers, and projective geometry.

Scholarships in Natural Science will be competed for this month or next at Gonville and Caius, King's, Jesus, Christ's, St. John's, Trinity, Emmanuel, and Sidney Sussex Colleges. The tutors will give full information.

A Clothworkers' Exhibition for Natural Science, tenable at Oxford or Cambridge for three years, will be awarded next July by an examination under the Oxford and Cambridge Schools Examination Board. Particulars may be obtained from the Censor of Non-Collegiate Students, Cambridge.

Another general modification of examinerships in natural science is proposed, which we shall refer to when it has been discussed by the Senate.

SCIENTIFIC SERIALS.

American Journal of Science, November.—On the relative motion of the earth and luminiferous ether, by Albert A. Michelson and Edward W. Morley. A complete and satisfactory explanation of the aberration of light is given by Fresnel's undulatory theory, which assumes, first, that the ether is supposed to be at rest except in the interior of transparent media; secondly, that in this case it moves with a velocity less than that of the medium in the ratio $\frac{n^2 - 1}{n^2}$, where n is the index of refraction. The second hypothesis having been fully established by Fizeau's celebrated experiment, the first alone is dealt with in this paper. From the delicate researches here described, which have been carried out by the aid of the Bache Fund, it is inferred that, if there be any relative motion between the earth and the luminiferous ether, it must be small, quite small enough entirely to refute Fresnel's explanation of aberration. It is further shown that the theories of Stokes and Fresnel also fail, and that it would be hopeless to attempt to solve the question of the motion of the solar system by observations of optical phenomena at the surface of the earth.—On the existence of carbon in the sun: contributions from the physical laboratory of Harvard University, by John Trowbridge and C. C. Hutchins. Without discussing the well-known observations of Abney on the absorption-bands in the solar spectrum at high altitudes, or Siemens's hypothesis of the presence of carbon vapour in interplanetary space, the authors here study the remarkable character of the carbon spectrum formed by the voltaic arc in air between carbon terminals, drawing attention to the evidence presented by the juxtaposed solar spectrum of the existence of carbon in the sun. They conclude that at the point of the sun's atmosphere where the carbon is volatilized, the temperature of the sun approximates to that of the voltaic

arc.—History of the changes in the Mount Loa craters, by James D. Dana. A recent visit of ten weeks to Hawaii has enabled the author to carry out the purpose expressed in his communication of last August. Here are presented only such facts as bear on the history of Kilauea since 1832, the general summary and conclusions being reserved for future numbers of the journal. The subject is illustrated with plates of Kilauea Crater, its lava floor, and the Halema'uma'u basin.—Is there a Huronian Group? (continued), by R. D. Irving: For the extensive region stretching from the north side of Lake Huron to the Mississippi it is here concluded that the succession of rocks in ascending order is from the great complex of crystalline schists, gneiss, and granite through the Huronian Group, mainly of detrital rocks, to the Keweenaw, of interleaved detrital and eruptive beds and the Potsdam, or Upper Cambrian Sandstone, with great structural breaks between the first and second, and second and third groups. The Huronian series itself, traceable throughout the Lake Superior province, is shown to be of clastic and sedimentary nature, of great volume, and structurally and chronologically separated from all other rock formations. The term *Agnotozoic*, originally suggested by Chamberlain, is proposed to cover the whole geological interval lying between the base of the Cambrian and the summit of the Archæan crystallines.—Description of an iron meteorite from St. Croix, County Wisconsin, by Davenport Fisher. This specimen, discovered in 1884 on a farm in Hammond Township, weighed 53 pounds, and yielded, on analysis: iron 89·78, nickel 7·655, cobalt 1·325, phosphorus ·512, silica ·562, with traces of carbon, copper, and tin.—The Rockwood meteorite, by J. Edward Whitfield. Picked up in March 1887 in a field in Cumberland County, Tennessee, this meteorite yielded, on analysis: iron 87·59, nickel 12·09, with traces of cobalt and copper.—Principal characters of American Jurassic Dinosaurs, by O. C. Marsh. This paper, forming Part 9 of the whole series, deals with the skull and dermal armour of Stego aurus, a nearly complete skeleton of which has lately been discovered. The specimen here described constitutes a new and very distinct species, for which the name of *S. duplex* is proposed.

THE *Journal of Botany* for September commences with an important paper, by Mr. Geo. Massee, on the growth and origin of multicellular plants. He describes the structure and mode of formation of the gelatinous membrane exterior to the true cellulose-wall, and extending continuously over the whole plant, which is not uncommon in Algæ, and nearly universal in the Florideæ. It can be easily shown that the formation of the cellulose-wall never precedes that of this mucilaginous sheath, and its function is rather a supporting than a protecting one. The mucilaginous sheath is composed of protoplasm, or of a substance very nearly allied to protoplasm. It is usually homogeneous, even after the appearance of the cell-wall; but in *Pandorina* the innermost portion consists of parallel rods placed end to end on the cell-wall. The portion composed of rods stains readily with methyl-violet and other aniline dyes, while the homogeneous portion does not. The remainder of the space in this number, and in those for October and November, is chiefly occupied by monographs or descriptive papers on new exotic species, or to others mainly of interest to English botanists. It is a remarkable evidence that the old-fashioned species-botany is not altogether dead in this country, that no fewer than three species of flowering-plants have been added to the flora of these islands during the past year—all in Scotland.

WE have received the numbers of the *Botanical Gazette*, published at Crawfordsville, Indiana, for August-November 1887. They furnish satisfactory evidence of the activity of botanical science in the Western States of North America. The articles and shorter paragraphs, where they are original, chiefly concern the flora of the district; but we may mention as of more general interest:—Vegetable parasites and evolution, by W. G. Farlow; development of the Umbellifer fruit, by J. M. Coulter and J. N. Rose; and plant odours, by A. J. Stace. The first of these papers is the Presidential Address given by Prof. Farlow before Section F of the American Association for the Advancement of Science. In it he treats specially of the phenomenon of "symbiosis" in lichens, and of "mycorrhiza." As to the former he doubts whether there is any sufficient evidence of the usual statement that the lichen-gonidia derive benefit from their association with the fungus.

THE *Nuovo Giornale Botanico Italiano* for October contains two papers only—on the Muscineæ of the Island Giglio, off the coast of Tuscany, by Signor A. Bottini; and an enumeration of plants gathered in the Balearic Islands in 1885, by Signor P. Porta. To the latter is prefixed an account of the physical geography and natural productions of the islands, and a *résumé* of previous botanical explorations.

Revue d'Anthropologie, troisième série, tome ii., sixième fasc., 1887 (Paris).—On the stature of the ancient inhabitants of the Canary Islands, by Dr. R. Verneau. The writer draws attention to the discrepancies to be found in the narratives of older chroniclers and travellers as to the stature of the islanders at the time of the discovery of the Canarian Archipelago. Thus while the Portuguese explorers sent out by Alphonso IV. of Portugal in 1341 described the natives as of the same medium height as the Portuguese, some of the Spaniards who took part in the conquest of the islands 200 years later maintained that they had seen the skeleton of a man 24 feet long, and spoke of living men who were respectively 9 and 14 feet in height. Setting aside the obvious absurdity of such estimates, Dr. Verneau is of opinion that in regard to some of the islands, as Lancerotte and Fortavente, it may be fairly assumed that the Guancho natives of pre-Spanish times were a tall, well-developed race, since such is still the character of the people in isolated villages in those islands which have been the least exposed to contact with strangers and invaders, while he found that the bones recovered from ancient local burying-grounds of the latter island indicated a mean height of 1·84 metre for men, and 1·60 for women. Amalgamation with invading races of lower stature seems to have lowered the mean height of the people, more especially in the south-east of the archipelago. Dr. Verneau finds that in regard to cephalic characteristics, the ancient Guanchos closely resembled the Cro-Magnon type, and he believes he has found incontrovertible proof that Numidian, Semitic, and other North-African races were among the earliest invaders of the Canaries.—On criminal anthropology, by M. Topinard. This is virtually a review of the Italian writer C. Lombroso's work on "Criminal Man," to whose theory of the physical and atavic character of criminality he is strongly opposed. Signor Lombroso believes that the criminal is born with irrepressible tendencies to crime, and that certain physical anomalies characterize the born malefactor. M. Topinard disputes not merely his mode of reasoning, but the facts which he adduces in support of his theories, and the accuracy, or applicability, of his numerous statistical tables. In conclusion, he not only shows the unscientific methods of inquiry followed by Lombroso, but he attacks the use of the denomination of "criminal anthropology," since the term implies the possibility of grouping together as fixed characteristics a number of phenomena which depend upon endless complications of psychical and social causes whose varied action on physical conditions does not admit of strict scientific determination.—Contributions to the sociology of the Australians, by M. Elisée Reclus. In this continuation of a series of papers which appeared in this journal last year, M. Reclus treats of spirits and sorcerers. The author uses his materials dexterously, and has compiled a highly interesting memoir on the superstitions and mythological fancies of these races, but as the greater part of the narrative has been derived from English sources it has little novelty or interest for English readers, who will find few facts in it with which they are not already familiar through the writings of Taplin, Woods, Grey, &c. —On lacustrine and lake-villages and pile-dwellings, by M. Pompeo Castelfranco. After a general consideration of the subject, more especially in regard to Italy, and the references bearing on it in the writings of Italians from the middle of the sixteenth century to the present times, the author gives the history of the discovery of lacustrine dwellings in Northern Italy which was made in 1862. Since that period almost all the lakes of that region have supplied rich yields of flints and pottery and bronzes, although none more so than Lake La Garda. The most interesting of these pile stations is that of La Lagozza, whose area of 2400 square metres was not wholly revealed till 1880. On examining the various piles which he had caused to be extracted from the superincumbent peat, Signor Castelfranco recognized that some were of birch (*Betula alba*) and others of fir and pine (*Pinus picea*, *P. silvestris*). Various flint and polished stone implements were found, but with the exception of a bronze fibula, which probably belongs to a later age than the original pile-dwellings, not a vestige of metal has been discovered at Lagozza. Potsherds and shreds of linen fabric have been found, but the most remarkable thing is the complete absence of bones, or any other animal remains; and while the

abundance of seeds, grains, nuts, acorns, &c., plainly indicates the vegetable character of the diet of these lake-dwellers, the appearance of masses of husked wheat and barley proves that they practised agriculture, and understood how to thrash and winnow the grain. Considerable interest attaches to the discovery below the peat, in what is characterized as the archaic bed, of large masses of seeds, determined by Prof. Sordelli as identical with those of the cultivated so-called Indian poppy (*Papaver somniferum*). Heer has recorded in the Swiss pile-dwellings the presence of poppy seeds which he referred to *P. seligerum*, but whether the Italian and the Swiss remains belong to the same or different species of poppy, the use to which they were put by primæval men in the two countries remains an unsolved problem.—On the Polynesians, their origin, migrations, &c., by MM. Lesson and Martinet. The purpose of this work is to refute the three most generally accepted theories regarding the origin of these races, viz. whether they are survivals from an almost wholly submerged continent, or whether they are of American, or of Asiatic descent; and to maintain the novel hypothesis that they are descendants of Maoris of the Middle Island of New Zealand. These views the authors endeavour to support by showing close analogies of language between the two peoples, affinities between certain names of places and of deities used by both, and frequent identity in forms of belief, rites, and superstitions. They further point out that the natives of the Marquesas, who are regarded as of the purest Polynesian race, use the same word, Havaiki, as the Maoris to denote their original ancestral home. From these and numerous other linguistic affinities the writers conclude that the Maoris are the autochthonic ancestors of the Polynesians, and that the Maori language is the mother speech of all the Polynesian dialects.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 24.—"On the Motion of a Sphere in a Viscous Liquid." By A. B. Basset, M.A. Communicated by Lord Rayleigh, D.C.L., Sec.R.S.

The determination of the small oscillations and steady motion of a sphere which is immersed in a viscous liquid, and which is moving in a straight line, was first effected by Prof. Stokes in his well-known memoir "On the Effect of the Internal Friction of Fluids on the Motion of Pendulums" (Camb. Phil. Soc. Trans., vol. ix. part 2, p. 8); and in the appendix he also determines the steady motion of a sphere which is rotating about a fixed diameter. The same subject has also been subsequently considered by Helmholtz and other German writers; but, so far as I have been able to discover, very little appears to have been effected with respect to the solution of problems in which a solid body is set in motion in a viscous liquid in any given manner, and then left to itself.

In the present paper I have endeavoured to determine the motion of a sphere which is projected vertically upwards or downwards with given velocity, and allowed to ascend or descend under the action of gravity (or any constant force), and which is surrounded by a viscous liquid of unlimited extent, which is initially at rest excepting so far as it is disturbed by the initial motion of the sphere.

In solving this problem, mathematical difficulties have compelled me to neglect the squares and products of velocities, and quantities depending thereon, which involves the assumption that the velocity of the sphere is always small throughout the motion; and I have also assumed that no slipping takes place at the surface of the sphere. The problem is thus reduced to obtaining a suitable solution of the differential equation—

$$D\left(D - \frac{1}{\mu}\frac{d}{dt}\right)\psi,$$

where

$$D = \frac{d^2}{dr^2} + \frac{\sin\theta}{r}\frac{d}{d\theta}\left(\operatorname{cosec}\theta\frac{d}{d\theta}\right).$$

ψ is Stokes's current function, and μ is the kinematic coefficient of viscosity. The required solution is obtained in the form of a definite integral by a method similar to that employed by Fourier in solving analogous problems in the conduction of heat; the resistance experienced by the sphere is then calculated, and the equation of motion written down and integrated by successive approximation on the supposition that μ is a small quantity. The values of the acceleration and velocity of the sphere to a third approximation are found to be

$$v = f\epsilon^{-\lambda t} .. V\lambda\epsilon^{-\lambda t} -$$

$$fka\sqrt{\frac{\mu}{\pi}}\{(\tfrac{1}{2} - \lambda t)\phi(t) + \sqrt{t}\} + fk^2a^2\mu t\epsilon^{-\lambda t}(1 - \tfrac{1}{2}\lambda t),$$

$$\dot{v} = \frac{f}{\lambda}(1 - \epsilon^{-\lambda t}) + V\epsilon^{-\lambda t} -$$

$$fka\sqrt{\frac{\mu}{\pi}}\left\{\left(t + \frac{1}{2\lambda}\right)\phi'(t) - \frac{\sqrt{t}}{\lambda}\right\} + \tfrac{1}{2}fk^2a^2\mu t^2\epsilon^{-\lambda t},$$

where

$$f = \frac{(\sigma - \rho)g}{\sigma + \frac{1}{2}\rho}, \quad k = \frac{9\rho}{a^2(2\sigma + \rho)}, \quad \lambda = k\mu,$$

$$\phi(t) = \int_0^t \epsilon^{-\lambda\tau}(t - \tau)^{-\frac{1}{2}}\,d\tau,$$

ρ being the density of the liquid, σ that of the sphere, and a its radius.

It thus appears that, after a very long time has elapsed, the acceleration will vanish and the motion will become steady. The terminal velocity of the sphere is $f\lambda^{-1}$, which is seen to agree with Prof. Stokes's result.

If the sphere were projected with velocity V, and compelled by means of frictionless constraint to move in a horizontal straight line, the values of the acceleration and velocity would be obtained from the preceding formulæ by expunging the terms $f\epsilon^{-\lambda t}$, $f\lambda^{-1}(1 - \epsilon^{-\lambda t})$, in the expressions for $\dot{v}$ and v respectively, and then changing f into $-V\lambda$.

The preceding results can only be regarded as a somewhat rough representation of the actual motion, for (1) the square of the velocity has been neglected; (2) no account has been taken of the possibility of hollow spaces being formed in the liquid; (3) if the velocity of the sphere became large, the amount of heat developed would be sufficient to vaporize the liquid in the immediate neighbourhood of the sphere, and the circumstances of the problem would be materially changed.

In the latter part of the paper I have considered the problem of a sphere, surrounded by a viscous liquid, which is set in rotation with given angular velocity, Ω, about a fixed diameter, and similar results are obtained. To a first approximation the angular velocity is equal to $\Omega\epsilon^{-\lambda t}$, where λ is a positive constant, which shows that the motion ultimately dies away.

December 8.—"The Sexual Reproduction of *Millepora plicata*." By Dr. Sydney J. Hickson.

Considerable attention has of recent years been paid by naturalists to the phenomena connected with the sexual reproduction of the Hydromedusæ. Stimulated by the brilliant results obtained by Allman and Weismann, several naturalists have investigated the structure of the various Medusæ and medusoid gonophores found in the group, the origin of the sexual cells, and the development of the embryo. These results have, on the whole, been so interesting and important that it was confidently anticipated that an investigation of the phenomena connected with the sexual reproduction of Milleporidæ would yield results of considerable interest. The systematic position of this family has always been a doubtful one, and naturalists were agreed that until the sexual reproduction was described, the position assigned to them could only be considered a temporary one.

It was my good fortune when in Talisse Island, North Celebes, to find on the reef just opposite my hut a fine specimen of *Millepora plicata* in vigorous growth. I visited it whenever the tide allowed, in the hopes of seeing the polyps fully expanded, and of being able to search them for any form of gonophore they might possess. In this, however, I was disappointed. Notwithstanding all my precautions, I never succeeded in finding the polyps more than partially expanded, and I could find no gonophores.

Having collected some specimens and dissolved the calcareous skeleton in strong acid, I discovered in the canals of the cœnosarc both the ova and the spermospores; but the unforeseen difficulties to be met with in working in a hot little bamboo hut in a tropical island prevented me from making any satisfactory series of sections, and I was reluctantly obliged to leave the further investigation of the subject until I returned to a laboratory in Europe.

Since my return home I have made a large number of prepa-

rations, and the results I have obtained may be summed up as follows :—

Both the male and female sexual cells arise in the ectoderm of the cœnosarcal canal system. At an early stage they perforate the mesoglœa and take up a position in the endoderm.

The ova at an early stage become stalked. The stalk of the ovum, which is simply a modified pseudopodium, serves to keep the ovum attached to the mesoglœa. The stalk is sometimes completely withdrawn, and the ovum by amœboid movements migrates along the lumen of the canals to a more favourable locality.

Maturation and impregnation occur while the ovum is still in the canals.

The mature ovum is very small (1/100 mm. in diameter), and is alecithal; nevertheless, it does not segment.

The germinal vesicle of the fertilized ovum splits up into a number of fragments, which, after a curious series of movements in the ovum, are eventually scattered over its substance.

By the time these fragments are thus scattered over the ovum, they have reached a considerable size, and, from faint markings in the substance of the ovum, no doubt can be retained that they are in reality the true nuclei of a morula stage in the development of the embryo. The embryo next assumes the form of a solid blastosphere, and its subsequent history is lost.

It will be a very interesting point to determine the precise mode of discharge of the embryo. I am very strongly of opinion that the embryo is discharged by the mouth of the gastrozooid, but I was, of course, unable to observe this in the living state. Whether this is correct or not, the fact remains that I have been unable to find in any of my preparations any trace of a free or fixed gonophore, containing either embryos or ova.

In the development of the spermatoza, a similar phenomenon is found to that in the development of the embryo. The spermospore does not divide into a sperm-morula, the nucleus alone fragments, and the subsequent formation of spermoblasts does not occur until a very late stage. When the spermoblasts are mature they are found in simple *sporosacs* on the dactylozooids. The sporosacs exhibit no traces of any medusoid structure.

These researches tend to prove that the Milleporidæ belong to a separate stock of the Hydrozoa from the Hydromedusæ, a stock which probably never possessed free-swimming medusiform gonophores.

There seems to be no true relationship between Millepora and Hydractinia. The absence of segmentation in the developing embryo may probably be accounted for by the amœboid movement which it exhibits after development has commenced. The evidence before us does not support the view that the ovum of Millepora formerly contained much yolk, and has subsequently lost it.

Physical Society, November 26.—Dr. Balfour Stewart, President, in the chair.—Mr. Asutosh Makhopadhyay was elected a member of the Society.—The following communications were read :—On the analogies of influence-machines and dynamos, by Prof. S. P. Thompson. The author pointed out that in nearly all influence-machines there are two stationary parts ("inductors") electrified oppositely, which are analogous to the field-magnet of dynamos, and a revolving part carrying "*sectors*" which correspond to the "*sections*" of an armature. To prevent ambiguity Prof. Thompson proposes to call the inductors "*field plates*," and the revolving parts as a whole an "*armature*." In the Wimshurst machine both field plates and armature rotate, and each acts as field plates and armature alternately. In the two field plate influence-machines there are four and sometimes six brushes. Two of these act as potential equalizers, two as field plate exciters, and the remaining two (if any) are generally placed in the "discharge" or external circuit. The Holtz machine having only four brushes, two serve the double purpose of potential equalizers and discharge circuit, and this machine excites itself best when the discharging rods are in contact. In this respect it resembles a series dynamo which only excites itself when the external circuit is closed, but on opening the circuit (say by inserting an arc lamp) produces remarkable effects. So in the Holtz machine on separating the discharging knobs a shower of sparks results. The Toepler machine (made by Voss) having six brushes resembles a shunt dynamo, and excites itself best on open external circuit. Analogies were traced between Thomson's replenisher and the Griscom motor. Armatures of influence-machines, as in dynamos, can be divided into ring, drum, disk, and pole armatures, and examples of each kind were mentioned. The "Clark Gas Lighter" is a good example of a drum armature, and a diagram showing the internal arrangements was exhibited. An example of an analogue to the compound dynamo was mentioned as existing at Cambridge, in the form of a Holtz machine believed to have been modified by Clerk Maxwell. Another analogue with dynamos is found in the displacement of the electric field when the armature is rotated, just as the magnetic field of a dynamo is shifted round in the direction of rotation. Further analogies were traced between "critical velocity" of dynamos (which depends on the resistances in the circuit) below which they do not excite themselves, and a similar critical velocity of influence-machines; *e.g.* in a Wimshurst or Voss machine, the potential equalizing circuit should have a low resistance if they are to excite themselves readily. Self-exciting dynamos excite better when the iron is bad and retains the magnetism, and influence-machines excite better when the field plates are made of paper or such substance as can well retain a residual charge. Finally an apparatus analogous to Thomson's "water-dropping accumulator" was exhibited, in which an electric current was generated by mercury falling down a tube between the poles of a magnet.—On the effect produced on the thermo-electric properties of iron when under stress or strain by raising the temperature to a bright red heat, by Mr. Herbert Tomlinson. In June last the author described some remarkable "effects of change of temperature on twisting and untwisting wires which have suffered permanent torsion," of which the present paper is a continuation. It is found that at or about the critical temperature (a bright red heat) mentioned in the previous paper, a sudden E.M.F. is generated at the junction of two iron wires, one of which is under stress or has suffered permanent strain, and the other in an unstrained state. By suddenly bringing a red-hot iron wire in contact with cold iron, an E.M.F. of about 1/20 volt is produced. If copper be used the E.M.F. is about $\frac{1}{3}$ volt. The author also showed that if one part of an annealed iron wire is heated to a bright red by a bunsen flame, an E.M.F. is generated if the position of the flame is slightly altered, the direction of the E.M.F. depending on the direction of the displacement. Prof. Ayrton believed the high E.M.F. exhibited by hot and cold copper was really due to oxide of copper; and Prof. S. P. Thompson said that different effects could be produced by using the oxidizing or reducing parts of the flame in heating the wire.—On the method of discriminating real from accidental coincidences between the lines of different spectra, with some applications, by Mr. E. T. J. Love.

December 10.—Prof. W. E. Ayrton, Vice-President, in the chair.—Mr. E. A. C. Wilson, and Mr. W. E. Sumpner were elected members of the Society.—Mr. H. G. Madan described the optical properties of phenyl-thio-carbimide. This body, derived from aniline, is a colourless liquid, density 1·35° C., and of high boiling-point 222° C. The refractive indices for the A and G lines are 1·639 and 1·707 respectively. It is thus seen to be a highly refractive liquid, and to have about the same dispersive power as carbon-bisulphide, whilst its use in prisms is unattended by many of the risks and inconveniences experienced with carbon-bisulphide. The dispersion at the blue end of the spectrum is very marked. Being less mobile than carbon-bisulphide, it is less affected by convection currents. The "refractive equivalent" calculated from its chemical constitution differs considerably from the observed value, and this difference the author believes due to the presence of the phenyl radicle and sulphur atom. A polarizing prism made on Jamin's plan, but using phenyl-thio-carbimide as the liquid, gives a fairly wide angular field (about 25°). Mr. Hilger stated that there was no great need of liquid prisms now, for very dense flint glass could be obtained with mean index of about 1·8. Dr. Perkin has recently supplied him with Canada balsam perfectly colourless, and which does not tarnish the polished faces of spar; hence one of the greatest objections to the use of Canada balsam in spar polarizing prisms has been removed. Dr. Gladstone pointed out that the constants for the phenyl radicle and for sulphur atoms had been determined, and thought the calculated "refractive equivalent" obtained by including these would be much nearer the observed value than the one given by Mr. Madan.—On the recalescence of iron, by Mr. H. Tomlinson. If an iron bar which has suffered permanent strain be heated to a white heat and allowed to cool, the brightness at first diminishes and then reglows (recalesces) for a short interval. Under favourable circumstances as many

as seven reglows have been observed during one cooling. Generally two decided ones are observed, one between 500° and 1000° C., and the other below 500° C. The effects the author believes due to "retentiveness" of the material, somewhat similar to the causes of residual magnetism and residual charge of a Leyden jar. A table of experimental results, giving the torsional elasticity and internal friction at different temperatures, for iron wire, showed sudden increases in internal friction at temperatures of about 550° and 1000° C. The table also shows that the torsional elasticity slowly decreases as the temperature increases, whereas the internal friction increases enormously. This explains why bells cease to emit musical notes when heated. The author finds that the recalescence at the higher temperature is not appreciably accelerated by mechanical vibration such as hammering, &c., but those occurring at lower temperatures are greatly influenced by such treatment and by magnetic disturbances. Prof. Forbes believed the explanation of recalescence given by himself about 1873 is sufficient to account for the effects observed. This explanation postulates a sudden increase in thermal conductivity about the temperature at which recalescence occurs, which permits the heat from the inside to reach the outside more readily, and thus raise the temperature of the surface. The subsequent reglows observed by Mr. Tomlinson he believes due to convection currents of air. Prof. Rücker suggested that calorimetric experiments might determine which view was the true one, and Prof. Ayrton thought the question might be decided by having two half-round bars nearly in contact at their flat sides, heated up and allowed to cool, and noting whether any sudden change in the bending of each bar (due to unequal temperature at the inner side and outer sides) took place about the critical temperature.—On the rotation of a copper sphere and of copper wire helices when freely suspended in a magnetic field, by Dr. R. C. Shettle. The author exhibited the apparatus with which his experiments "on the supposed new force" were made, the results of which were published in the *Electrician*, vol. xix. Dr. Hofford has recently made similar experiments, using brass disks, and his results seem to point to "diamagnetic non-uniformity" of the disks as the cause of the phenomena he observed.

Linnean Society, December 1.—W. Carruthers, F.R.S., President, in the chair.—There was exhibited for Mr. O. Fraser, of Calcutta, a specimen supposed to be a weather-worn seed of a palm, picked up on the Madras coast. Opinions given at the meeting referred it to the consolidated roe of a fish, doubts being thrown on its vegetable nature.—Sir John Lubbock read a paper, an account of which we have already printed, on the habits of ants, bees, and wasps.—A paper was read by Mr. C. B. Clarke, on a new species of *Panicum* with remarks on the terminology of the Gramineæ.

Geological Society, November 23.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—Note on a New Wealden Iguanodont, and other Dinosaurs, by R. Lydekker.—On the Cae Gwyn Cave, by Prof. T. McKenny Hughes, who contended that the drift outside the cave was a marine deposit *remanié* from older beds of glacial age, but was itself post-glacial and of approximately the same date as the St. Asaph drift. He maintained that the marine drift was deposited before the occupation of the cave by the animals whose remains have been found in it; that at the time of the occupation of the cave the upper opening now seen did not exist, but the animals got in by the other entrance; that against the wall of the cave where it approached most nearly to the face of the cliff, the drift lay thick as we now see it; that by swallow-hole action the cave was first partially filled, and then the thinnest portion of its wall gave way gradually, burying the bone-earth below it, and letting down some of the drift above it, so that some of it now looks as if it might have been laid down by the sea upon pre-existing cave-deposits. The reading of this paper was followed by a discussion, in the course of which Dr. Hicks argued strongly against the author's conclusions.

Mathematical Society, December 8.—Sir J. Cockle, F.R.S., President, in the chair.—Messrs. W. B. Allcock, J. W. Mulcaster, and I. Beyens, Cadiz, were elected members.—The following communications were made:—The algebra of linear partial differential operators, by Capt. Macmahon, R.A.—On a method in the analysis of ternary forms, by J. J. Walker, F.R.S.—Confocal paraboloids, by A. G. Greenhill.—Note on the solution of Green's problem in the case of the sphere, by A. R. Johnson.—Uni-Brocardal triangles and their inscribed triangles, by R. Tucker.

Chemical Society, November 17.—Mr. William Crookes, F.R.S., President, in the chair.—The following papers were read:—Zinc-copper and tin-copper alloys, by A. P. Laurie.—The halogen substituted derivatives of benzalmalonic acid, by C. M. Stuart.—Note on a modification of Traube's capillarimeter, by H. S. Elworthy.—The formation of hyponitrites: a reply, by Edward Divers, F.R.S.—Reply to the foregoing note, by W. R. Dunstan.

Royal Microscopical Society, November 9.—Rev. Dr. Dallinger, F.R.S., President, in the chair.—Mr. E. M. Nelson called attention to a suggestion for supplying a want which many had felt of a really good achromatic single lens or loupe for microscopic purposes, of ½-inch focus. He had found that the want was met by a Seibert No. III. objective, having its adjusting screw removed.—Mr. Nelson further said that, having lately obtained an improvement in optical power, he had been able to do a little more in the matter of resolution, and one of the first objects he had tried was striped muscular fibre. In the early days of microscopy a muscular fibril used to be represented as a series of light and dark bands, the dark band being about twice the diameter of the white band. In 1854 Messrs. Huxley and Busk discovered a dark stripe in the middle of the bright band, and subsequently Hensen placed a similar darker stripe in the middle of the dark band. With his latest optical appliances he had been able to see a faint white stripe on either side of Hensen's dark stripe. He estimated the diameter of the stripes to be all equal. Although he saw evidences of longitudinal breaking up, he could see nothing of Schäfer's "beads."—The third point noticed by Mr. Nelson was Mr. Francis' method of improving definition of such an object as *Amphipleura pellucida* by using the analyzer. He had tested the plan, and found that it did intensify the resolution in a very marked degree.—Mr. Nelson also exhibited and described a new portable microscope made by Messrs. Powell and Lealand from his drawings, and the new photomicrographic camera designed by Mr. C. L. Curties and himself.—Mr. Nelson further exhibited a new eye-piece which he had devised. Having for some time past made a great many experiments with achromatic eye-pieces of double, triple, and other forms, he had not succeeded in producing any combination whose defining power surpassed that of the Huyghenian. The best results were obtained by achromatizing the eye-lens—*i.e.* by making it of a biconvex and a plano-concave, with its convex side towards the eye. The aperture of the diaphragm was reduced until the diameter of the field was equal to that of the Abbe compensating eye piece. This eye-piece, with the achromatized eye-lens, gives the sharpest images he had seen. It works perfectly well with the 24 mm. and 3 mm. Zeiss apochromatic objectives.—Mr. C. R. Beaumont then exhibited and described his new form of slide for observing living organisms, and read a paper on the metamorphoses of *Amœbæ* and *Actinophrys*, in which he claimed to have observed the development of an *Amœba* into an *Actinophrys*, and then into a *Difflugia* and an *Arcella*.—Mr. H. B. Brady's paper, a synopsis of the British recent Foraminifera, was communicated to the meeting by Prof. Bell.

PARIS.

Academy of Sciences, December 5.—M. Janssen in the chair.—Letter to M. Bertrand in connection with his previous note on a theorem relative to errors of observation, by M. Faye. It is pointed out that, if we consider all the combinations of errors, the relations of the sums corresponding to the greatest and smallest of these errors are comprised between the extremes 1 and 3·915. Both of these are infinitely improbable in themselves, while their mean, 2·457, differs little from the number 2·414 given by M. Bertrand.—Reply to M. Mascart on the subject of the deviation of the winds on the synoptical charts, by M. Faye. The author insists that he has nothing to modify in what he has written during the last thirteen years on the descending spiral motion of cyclones. The synoptical charts, which have been multiplied during the last few years, when properly interpreted, are shown to be in no way opposed, but, on the contrary, lend additional support, to his theory.—On the synchronism of accurate time-pieces, and on the distribution of time, by M. A. Cornu. A description is given of the construction and properties of a very simple electric appliance, which is applicable to all kinds of oscillating apparatus, and which

realizes the theoretic conditions under which the problem of synchronism has been solved. This system has already been at work for several years in the École Polytechnique, and has been applied with complete success in the Paris Observatory for the synchronizing of the two clocks in the Department of Longitudes. The problem of the distribution of time with a precision approaching the hundredth part of a second is thus satisfactorily solved. The apparatus is extremely simple and easily regulated, and may be worked with feeble currents.—Remarks in connection with a work entitled "Les Ancêtres de nos Animaux dans les Temps géologiques," presented to the Academy by M. Albert Gaudry. In this work the fossil mammals are tabulated in the ascending order according as they appeared on the earth from the Lower Miocene through all the intervening geological epochs up to the present time. A concluding chapter is devoted to an historic survey of palæontology in the Paris Museum.—On magnetizing by influence, by M. P. Duhem. The questions here discussed are: the quantity of heat liberated in the transformation of a system including magnets, and the heat liberated in the displacement of a magnetic mass.—New nebulæ discovered at the Paris Observatory, by M. G. Bigourdan. The right ascension and polar distance, with miscellaneous remarks, are given of the nebulæ consecutively numbered 51 to 102. Observations are appended on thirteen other nebulæ previously discovered.—On the division of an arc of a circle, by M. A. Pellet. The approximate division of an arc in a given relation is determined by means of rule and compass.—On the expansion of compressed fluids, and especially on that of water, by M. E. H. Amagat. The compressibility and expansion of water, ordinary ether, methylic, ethylic, propylic, and allylic alcohols, acetone, chloride, bromide and iodide of ethyl, sulphide of carbon, and chloride of phosphorus, have been studied between zero and 50°, and from the normal pressure up to 3000 atmospheres. For all except water, which behaves exceptionally, the coefficient of expansion diminishes with increased pressure, the decrease being still very perceptible at the highest point. The coefficient of water increases very rapidly at first, but afterwards diminishes gradually, disappearing altogether towards 2500 atmospheres.—On a new method of quantitative analysis for carbonic acid in solution, by M. Léo Vignon. By the process here described the presence may be detected of 1 cubic centimetre of carbonic acid in 1 litre of water.—Influence of natural or superinduced sleep on the activity of the respiratory combustions, by M. L. de Saint-Martin. It is shown that, apart from the state of fasting, natural sleep lowers by about one-fifth the quantity of carbonic acid exhaled, and by only one-tenth the quantity of oxygen absorbed; also, that in sleep brought about by morphine the proportion exhaled falls to one-half, and in sleep caused by chloral or chloroform to one-third, of the quantity exhaled during the same lapse of time in the normal state.—On the absence of microbes in the human breath, by MM. J. Straus and W. Dubreuilh. These researches fully confirm the conclusions already arrived at by Lister and Tyndall regarding the freedom of exhaled breath from the presence of pulmonary or other microbes.

Berlin.

Physical Society, Nov. 11.—Prof. von Helmholtz, President, in the chair. —Dr. Weinstein spoke on the determination of the electrical resistance of tubes of mercury. He employs two methods for measuring the length of the tubes, one in which the tube is completely filled with mercury, the other in which it is only partially filled, and in which the convexity of the ends of the column of mercury is taken into account. The first method is the more exact, but is less simple; the difference between the methods is small. The measurement of the diameter of the tube is of great importance, and is made under the assumption that the tube is either a cylinder or a cone; the latter is the more correct assumption when the tube is long, and necessitates calibrational corrections, for which Dr. Weinstein deduced the formulæ. Taking into account the want of accuracy in the constants involved in the above, he considers it far better to determine the volume from the heights of the capillary rise of fluids in the tube.—Prof. Pictet, who was present as a guest, gave a detailed account of the experiments he has made with his ice-machines, which have led to results which do not agree with Carnot's theories as far as the second law of thermodynamics is concerned. He described the action of a perfect ice-machine, consisting of a refrigerator, pump, and condenser. In the refrigerator a quantity of heat is taken from the salt-water bath surrounding it, which causes some of the fluid to evaporate; this vapour, at the temperature of the surroundings, passes unchanged into the pump, where it is compressed, and forced, at high pressure, into the condenser, where it at once becomes a liquid, and gives up all its heat to the surroundings. This condensed fluid then flows back to the refrigerator. In a real machine of finitely small dimensions, the temperature in the refrigerator falls, the vapour meets with resistance in passing over into the pump, and in passing from the latter into the condenser, and there is a fall of temperature as the heat passes out into the surroundings from the liquid formed in the condenser. The speaker determined by careful experiments the tension of the vapour with which he worked between − 20° C. and + 30° C., and then he measured the temperatures in the several parts of the working machine by means of manometers which registered the pressures in the several parts, and from this he arrived at the result stated above. The measurements were made when the pump was working both rapidly and slowly, and also when it was stopped. Prof. von Helmholtz drew attention to two sources of error which cannot be avoided in Prof. Pictet's experiments, and which might account for the results obtained being in opposition to Carnot's law. In the first place, the vapour might contain air; this would influence the pressure existing in the machine, without itself undergoing any condensation, and hence it is impossible to determine the temperature of the vapour accurately from measurements of its pressure. The second source of error is, however, still more important. In Pictet's ice-machines, the liquid used is a mixture of liquefied carbonic acid gas and sulphur dioxide. From such a mixture as this the more volatile carbonic acid gas must pass over into the refrigerator in larger quantities than the less volatile sulphur dioxide. Hence both the vapour and the liquid resulting from its condensation have a composition markedly different from that of the original liquid. Now the calculations are made on the assumption that the liquid undergoes no change of composition, hence the temperatures determined from the pressures cannot correspond to those really existing in the several parts of the apparatus. Prof. Helmholtz hence considers that the temperatures in the refrigerator and condenser should be measured with thermometers, in which case only it would be possible to test the truth of Carnot's laws on the basis of the heat-values obtained in the experiments.

November 25.—Prof. von Helmholtz, President, in the chair.—Dr. Stapff spoke on his measurements of the temperature of the earth in South Africa. From his observations on the temperature in the St. Gothard Tunnel, and a comparison of these with the temperatures observed at the earth's surface, he had deduced an empirical formula for the difference of temperature between the air and the earth: according to this formula, the difference is greater the lower the temperature of the air, and disappears when the temperature of the air rises to 11° C. It hence became a matter of interest to determine whether the difference is negative when the temperature of the air is very high. Dr. Stapff had made use of a sojourn in South Africa, near Whale Bay, while engaged in geological studies, for the purpose of carrying out observations on the temperature of the earth. The district in which he worked lies in the Tropic of Capricorn, about in the same meridian as Berlin, and the soil is sandy with a current of water running beneath it towards the sea. The observations were made in borings with English mining-thermometers, which were allowed to remain about twelve hours at the depth where the temperature was to be determined, thus insuring that they had taken up the temperature of the surroundings. The measurement of the temperature at the earth's surface presented very great difficulties, and was only rendered possible by covering the bulb of the thermometer with a layer of sand 5 cm. thick. The greatest depth at which the temperature of the earth was measured was 17 metres. From the determinations thus made it appeared that the temperature diminished down to that depth, a result undoubtedly dependent upon the fact that the measurements were made during the hottest time of the year. The speaker found that the depth down to which the temperature varies with that of the air is about 13·6 metres, the temperature at this depth being about 25° C. The changes in temperature of the earth were very considerable, greater than those of the air, amounting in the sand to some 30° to 40° C. His measurements, however, did not show any negative value for the difference in temperature of the air and earth.—Dr. Sieg gave an account of his experiments for the determination of the capillary constants

for large drops and bubbles. On account of the marked divergence in the results obtained by Quincke as compared with the older measurements, the speaker was led to subject Quincke's method to a detailed examination. He found that the determination of the height of the drop is exact, but that the measurement of its width by means of the micrometer is too uncertain. Instead of this method, he therefore employed the reflection of a flame from the side of the drop in order to determine the convexity of the same, and using Poisson's method of calculating the results instead of that of Quincke, he obtained as the value of the capillary constant, not 54 as given by Quincke, but 44·5, thus agreeing with the older determinations. The mercury was purified and examined by Quincke's method. In addition Dr. Sieg has determined the capillary constants for water, alcohol, oils, and a series of salt-solutions of varying concentrations. One result may be mentioned as shown by these experiments, that the capillary constant of mercury sinks to forty-two when the mercury has stood for some time, and that the same fall is observed if the mercury is put to earth; the constant is also altered if the drop is electrified or is impure. With salt-solutions the constants were dependent upon both composition and concentration. Water was also found to be very sensitive to the presence of any impurities, and while the solution of salts in water was not found to alter its capillary constants, the solution of gases produced a very appreciable alteration

Physiological Society, November 18.—Prof. du Bois Reymond, President, in the chair.—After the statutory election of the Council, Dr. Benda demonstrated a malformation as occurring in a three-months' embryo, in which two strongly marked prominences on the lower portion of the forehead gave to its countenance a curiously contemplative appearance.—Prof. Kossel next spoke on adenin. The most recent researches on the importance of the nucleus to the life of the cell, especially the knowledge that when unicellular organisms are artificially cut into pieces only those parts exhibit a complete regeneration which contain a portion of the nucleus, and the importance of the nucleus in impregnation have given an increased importance to the chemistry of the nucleus. Among the chemical substances which compose the nucleus, adenin, which has recently been discovered by the speaker, appears to possess a special importance, since, on account of its composition, $C_5H_5N_5$, it belongs to the cyanic group of bodies. This substance was obtained from tea-leaves in large quantities, and from it a series of compounds were obtained, which were exhibited as extremely fine preparations; namely, the salts with hydrochloric, sulphuric, and nitric acids, as also some compounds with platinum. Adenin was found to be extremely resistant to feebly oxidizing agents, but on the other hand to be easily acted upon by reducing agents. The substances which are produced by these means were not very well characterized from a chemical point of view. The speaker however thinks that, owing to the ease with which it can be reduced, adenin plays an extremely important part in the physiological action of the nucleus. When adenin is reduced in presence of oxygen, a brownish-black substance is obtained, which appears to be identical with the azocuminic acid which is produced when hydrocyanic acid is exposed to the air for a long time. In conclusion, Prof. Kossel pointed out that adenin makes its appearance in large quantities under certain pathological conditions, and that he has succeeded in detecting it in the urine of persons suffering from leuchæmia. —Dr. Rawitz gave an account of his investigations on mucous cells in Invertebrates. He has found in the mantle of mussels goblet-cells, of which some are small with a large central nucleus and granular protoplasm; others are large with a small central nucleus, the rest of the cell-contents being uniform in appearance; and others again are large, with a small nucleus situated at the base of the cell, the protoplasm having oily granules scattered throughout itself. This last kind of cell allows the oily granules and mucous contents to pass out at the apex of the cell into the surrounding water. A careful investigation has shown that the above three different kinds of cells are merely different stages in the secretory activity of the mucous cells, and that during this activity the cell-contents not only undergo a change of minute structure, but also of chemical composition, the latter being evidenced by the changed reactions which they give with staining agents. During secretion the cell itself is not broken down, but only a portion of its protoplasm is excreted, in the form of oily drops and mucous threads, the nucleus remaining intact. Dr. Rawitz considers that special importance must be assigned to the nucleus in connection with the nutrition of the cell, as during the secretory activity of the cell it undergoes changes not only in its shape, but in its behaviour towards taining reagents.

STOCKHOLM.

Royal Academy of Sciences, November 9.—Plantæ vasculares Yenessenses inter Krasnojarsk urbem et ostium Yenisei fluminis tractenus lectæ, by Dr. N. J. Scheutz.—On additive characters of diluted solutions of salts, by Dr. S. Arrhenius.—On the theory of the unipolar induction, by Dr. A. Rosén.—Some formulæ of electrodynamics, by the same.—The phænogamous plants of Bergjum, enumerated in the sequence of their inflorescence, by the Rev. B. Högrell.—On hyalotekit from Långbau, by G. Lindström, Assist. Min. Cab. State Mus.—On the scientific results of the expedition of the *Vega*, by Baron Nordenskiöld.—Contributions to the theory of the undulatory movement in a gaseous medium, by Prof. A. V. Bäcklund.—Contributions to the knowledge of the exterior morphology of the Acridioideæ, especially with respect to the specimens found in Scandinavia, by Dr. B. Haij.—Generalization of the functions of Bernouilli, by Dr. A. F. Berger.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Les Ancêtres de Nos Animaux: A. Gaudry (Baillière et Fils).—British Journal Photographic Almanac, 1888 (Greenwood).—The Elements of Chemistry: Ira Remsen (Macmillan).—British Discomycetes: W. Phillips (Kegan Paul).—Vaccination Vindicated: J. C. McVail (Cassell).—Flower Land, an Easy Introduction to Botany: Rev. R. Fisher (Heywood).—A Course of Quantitative Analysis: W. N. Hartley (Macmillan).—Teneriffe and its Six Satellites, 2 vols.: O. M. Stone (Marcus Ward).—Annual Report on the Working of the Registration and Inspection of Mines and Mining Machinery Act during the year 1886 (Melbourne)—Digging, Squatting, and Pioneering Life: Mrs. D. D. Daly (Low).—China; its Social, Political, and Religious Life: from the French of G. Eug. Simon (Low).—Through the West Indies: Mrs. G. Layard (Low).—A Text-book of Paper Making: Cross and Bevan (Spon).—Proceedings of the Linnean Society of New South Wales, vol. ii part 2.—Quarterly Journal of the Geological Society, vol. xliii. pt. 4, No. 172 (Longmans).—Annals of Botany, vol. i. No. 11 (Clarendon Press).

CONTENTS.

THURSDAY, DECEMBER 22, 1887.

THE STAR OF BETHLEHEM.

THE fact that a little more than a month ago the planet Venus arrived at its maximum brilliancy when to the west of the sun, and therefore when the planet rises before the sun, has given rise to a flood of superstitious fears in this country, only to be equalled in modern times by that which the members of the Eclipse Expedition observed in Grenada last year, and chronicled in these columns, as having been met with among the semi-civilized inhabitants of that island.

In spite of School Boards and all the present stock-in-trade of elementary education, perhaps partly because that elementary education deals so little with natural science; and because before School Boards so many children scarcely went to school at all, the planet Venus, one of the most stable and the most brilliant member of the solar system, is being regarded as a second appearance of the star of Bethlehem!

This being the idea which ignorance has conjured up, superstition next comes in to bear her part, and hence very naturally all sorts of woe and desolations, the end of this world being naturally included among them, have been predicted, and in some places a considerable amount of alarm has really arisen. Nor is this all: thousands of people who ought to be able to look up pocket-books and almanacs for themselves have been for the last month pestering everybody who is known to possess a telescope for information on the subject.

We think it, therefore, worth while to refer to this subject, for we have in this ignorant fright an additional reason, which it may be worth while to dwell upon, why the young population of a country like England should not be allowed to grow up without some knowledge, however slight, of the natural phenomena which are always being unfolded around them—phenomena which will always delight, instruct, and interest them if understood, but which will be apt to cause alarm so long as they are shrouded in mystery.

As before stated, the brilliant body in the east which is the innocent cause of all the alarm is nothing but the planet Venus near that position in her orbit in which she can send the greatest amount of light towards us.

If our youngest reader will place a candle in the middle of a table, and support a little ball some six or eight inches away from the candle, on the same level, and then retire some little distance away, to represent a spectator on the earth, the reason why Venus sometimes appears to the right or to the west of the sun and at other times to the east or left of it will be at once clear to him, if the ball be imagined to go round the candle in a direction contrary to that of the hands of a watch. Further, the fact that when the ball is on the other side of the candle it is further away, and therefore appears smaller than it is when exactly between the candle and the spectator, will give a reason why in neither of these cases will the maximum brilliancy be observed, because in one case the planet is as far away as it can be, and in the other, though the planet is as near to us as it can be, it has its dark side turned towards us; for it must be clearly understood that Venus, like the earth, receives its light from the sun, represented in our experiment by the candle; and when the spectator is on one side of the little ball, representing Venus, and the candle is on the other, naturally the non-illuminated side of the ball alone is turned towards the spectator. The period of maximum brilliancy will be when the planet is to the right or left of a line adjoining the spectator and the candle, and nearer the observer than the candle is. When the planet is to the right of this line, and therefore to the westward of it, speaking celestially, the planet must set before the sun, and therefore rise before the sun: it will be a morning star. On the other hand, when to the left of it, it must set after the sun, and therefore it will be visible as an evening star; and because it sets after the sun it will rise after it, and therefore be invisible as a morning star on account of the overpowering light of the sun. We might apologize to the readers of NATURE for referring to such elementary astronomy as this, were it not quite possible that many of them will have an opportunity, if the scare continues, of showing several young minds how to make the experiment for themselves.

The accompanying diagram will show the positions of Venus and the earth for the last few months, and will

Venus as "Evening Star."

Venus as "Morning Star."

Diagram showing the paths of the Earth and Venus from July 13 to December 1, 1887, with the points of maximum brilliancy on August 16 and October 28. Synodic period of Venus, 583·92 mean solar days.

indicate why it was at its brightest *as a morning star*, on October 28, and *as an evening star* on August 16.

It will be in the memory of some of our readers that on the appearance of the new star observed by Tycho Brahe in 1572 the general opinion was that that also was the star of Bethlehem returned. It mattered little to the vulgar that the latter was called "the star in the East," and that the new star was nearly in the zenith, and at about the same time of the year (November).

A reference to Grant's admirable history of physical astronomy will show us that such new stars were also recorded in 130, 390, 945, and 1264. The authority for these statements is Cyprian Leowitz, whose work was published in 1573. Although his statements have been discredited, there is nothing improbable in them. The "new star" of which we have heard the most, because there was a man living who was capable of chronicling and more or less understanding the phenomenon, was that to which we have referred above as having appeared in the year 1572. This was carefully watched by Tycho

Brahe. It suddenly appeared brighter than any of the stars, and brighter than Jupiter, though not brighter than Venus. This star remained visible for nearly two years. Its colour changed as it grew dimmer: first it was white, then yellow, then red, and finally, according to the record, exhibited a leaden hue like the planet Saturn. Tycho Brahe imagined it generated from the ethereal substance of which he held the Milky Way to be composed, and when it disappeared it was thought to have dissolved spontaneously from some internal cause.

It is not a gratifying thing to find, when we come to inquire further into the state of public feeling at the time when Tycho's star appeared, that after all we have advanced very little beyond the sixteenth century in matters relating to superstition. The world was to end in 1532, according to Simon Goulart, because a mountain in Assyria had been seen to open, and exposed to the gaze of those present a scroll with letters written in Greek stating that the end of the world was at hand.

Goulart was followed by a famous astrologer, Leovitius, who put on the date to 1584; and Gayon reports that the fright at that time was almost universal, and the churches would not hold those who sought shelter in them.

This end of the world mania was not confined to the unlearned, for a famous mathematician, Stoffler, who was actually engaged on the reform of the calendar undertaken by the Council of Constance, put down the end for February 1524. According to him, the end was to be by water and not by fire, and the basis of his prediction was that Saturn, Jupiter, and Mars would then be together in the sign Pisces. It was a rare time for the boat-builders, for many "arks" were built; a doctor of Toulouse, named Auriol, making himself immortal by building the biggest.

Stoffler and Regiomontanus were not, however, discouraged by the fact that not a drop of rain fell during the whole of that month in Central Europe: they merely put the date on to 1588.

It must be remembered that in those days of unusual superstition these predictions were carried broadcast through the land, and it was the consternation of the ignorant which caused everybody to believe that Tycho's star, which appeared in 1572, was really the star of Bethlehem, returned to announce the second coming of Christ.

But as a matter of fact this star of Tycho's is really connected with the present excitement, and again the idea of the return of the star of Bethlehem has been associated with it—although the year 1572 passed off quite quietly, and the planet still survives—for the following reasons. The star appeared between the constellations of Cassiopeia and Cepheus—that is, in the same part of the heavens in which in former times, in 945 and 1264, similar appearances had been recorded. Argelander, who inquired into the matter, found a 10½-magnitude star catalogued by D'Arrest, but seen some years before, when the same part of the heavens (R.A. 4h. 19m. 58s., Decl. + 63° 23′ 55″) was under scrutiny. It was suggested, therefore, that the star in question might be a variable one with a period of 314 years: this would very closely account for appearances in the years 0, 945, 1264, 1672, and 1887! and if it were really the star of Bethlehem, it would be naturally seen about Christmas-time. Nothing is more curious than to watch how a piece of scientific knowledge has thus settled down to form a nucleus for a haze of sensational nonsense.

But it is not impossible that, after all, we are really again in presence of the star of Bethlehem; for if we read the account in St. Matthew, and assume that some celestial body is really alluded to, and not a miraculous appearance similar to those recorded by St. Luke (chapter ii. 8–15), then it would seem that Venus, as she has been seen lately—that is, at her maximum brightness—will do as well as any other, and there is no necessity to assume either a "new star," or a comet, as giving rise to the phenomena recorded.

We give that part of the narrative which chiefly concerns us, and it is necessary to bear in mind that Bethlehem lies nearly due south of Jerusalem, and is about five miles distant.

". . . There came wise men *from the east* to Jerusalem, saying, . . . we have seen his star *in the east*. . . . When they had heard the king, they departed [to Bethlehem]; and, lo, the star, which they saw [had seen] in the east, went before them, till it came and stood over where the young child was. When they saw the star, they rejoiced."

The fact that the star was stated to be seen "in the East" would imply that it was not seen anywhere else. This is best explained by supposing a *morning* observation of a body soon rendered invisible by the light of the sun. A star seen in the East at evening would be visible all night, and could no longer be properly designated as a "star in the East." This is against the views which have been held and supported by Kepler, to the effect that a conjunction of superior planets was in question; and indeed they have already been demolished by Prof. Pritchard.

If we assume that the star was Venus at maximum brightness seen in the East in the morning, and that it rose, say, two hours before the sun, it would be about south at 10 a.m. It would seem not improbable that the journey to Bethlehem should be made before noon. The gathering of the priests and scribes would probably last till sundown, and it would be natural that the journey should be undertaken next morning. Journeys in the East are not generally now, and were probably not then, undertaken in the evening. The latter part of the extract indicates that the "wise men" did not see the star till they got to Bethlehem, and that the statement that "the star went before them" is rather an attempted explanation of its change of place than a reference to any actual observation.

The simple facts, then, seem to be that the "wise men"—no wiser, it would appear, than the average Englishman of the present day, in astronomical matters—being struck by the exceeding brilliancy of Venus, which they did not recognize, felt sufficient interest in it, or, more probably, were so soundly frightened at it, that they went to the nearest important town, Jerusalem, to find out something about it. It has been assumed that the Magi came from a *great* distance, but there is nothing to justify this, apparently; and if we go beyond the record at all we may as well accept them at once as Melchior, Balthazar, and Jasper, the kings respectively of Nubia, Chaldea, and Tarshish, whose bones are supposed to be at Cologne, though their connection with the Biblical narrative is not clear, as it

is not on record where these personages joined company before they set out *westwards* for Jerusalem.

As comets long afterwards were supposed to presage disaster, so the star may have been regarded as an indication of the approaching death of King Herod. This would start the question as to his successor, whom the "wise men" would desire to stand well with, or to "worship." With what happened at Jerusalem we have nothing to do. On approaching Bethlehem about noon, they again recognized the star over the town, as Venus would be at that time, on the supposition that the "star in the East" which they had first seen was really that planet.

Another point connected with this matter relates to the question of new stars. Supposing there were a new star in the east, why should the population be affrighted? The records of astronomy, as we have seen, tell of a considerable number of such stars, and during the last few years we have been favoured with our fair share of such appearances, and yet the world is none the worse for them. The view which has recently been put forward, with an amount of evidence to back it which almost puts it beyond question, is that in new stars we see only such phenomena as we must expect; we see the result of no unnatural dealings with the regulated order of the universe, but simply the collisions of swarms of meteorites, these meteorites being not only not in our own system, but lost, it may be, in the very depths of space. Why should such a thing as this affright us? It is simply what happens at a level crossing when a train runs into a cart, and it does not seem likely that such an ordinary piece of mechanism as this would be chosen as a means of frightening or ringing the death-knell of a world.

Modern science, while thus abolishing mystery from the skies, is only enhancing the majesty of all created things. The universal law and order are more clearly seen in every great advance; and yet, with a population so superstitious that the least uncomprehended thing affrights them, our statesmen are still on the side of ignorance, and hinder rather than aid the introduction of science into our schools.

THE MICROSCOPE.

The Microscope in Theory and Practice. Translated from the German of Prof. Carl Naegeli and Prof. S. Schwendener. (London: Swan Sonnenschein and Co., 1887.)

THIS book opens to English readers an entirely new page in microscopical literature. It leads the way in supplying a want which every thorough microscopist has realized for the last twenty years. In a complete form this treatise has been accessible to the German reader for at least ten years. The absence of it, or an equivalent, in the English language has been a most serious drawback to the advancement of the highest optical work in English microscopes. In optical manipulation, the English optician at his best proves not only equal to any in the world, but, in the highest class of work, has shown lately that he takes a foremost place. But with no attempt on the part of English mathematicians and microscopists to become masters and expounders of the theory of the microscope and of microscopic vision, the practical optician can make no real advance. English "stands," and those made in America on English models, are of exquisite construction, and are quite equal to our present necessities; but, for all the great advances and improvements that have been made in *English* object-glasses during the last fifteen years, we are, for all practical purposes, primarily indebted to Germany. And this is readily explained by the fact that the German specialists have made a systematic and persistent study of the theory of the microscope.

It is not forgotten that it was to the suggestion of Mr. J. W. Stephenson that we are indebted for the invaluable improvements that belong to the homogeneous system of lenses.[1] But, without doubt, it was on account of the insight which a study of the theory of microscopic vision brought with it, that Mr. Stephenson perceived at once the advantages of great numerical aperture, and the new way to obtain it. Moreover, it is certain that Prof. Abbe was approaching this very method of employing lenses, though from another point, and not in so direct a way. It would have been shortly reached by him there can be but little question; but when it was reached, what did a constant, enthusiastic, and laborious study of the theory of the microscope carry with it? A perception, that with glass of greater range of refractive and dispersive indices than any we possessed, we might not only secure great numerical apertures, but secure them devoid of all colour; that we could not only annul the primary, but also the secondary and tertiary, spectra. It need not surprise us then, that, in a country where such splendid theoretical and mathematical work had been done by experts on the principles of microscopic lenses and the laws of their construction and use, even the Government should be convinced that the time to aid the optical expert had come; that theory had demonstrated the practical possibility of a great improvement in the construction of lenses. The sum of £6000 was granted by the German Government to Abbe and his collaborateurs, and with, as we have reason to believe, an equivalent outlay on Abbe's own part, the new glass was prepared; and the new Apochromatic lenses with their systems of compensating eye-pieces devised.

It is in no spirit of boast, but rather in a spirit of humiliation and regret, that we say that we have examined many of these apochromatic objectives of all the powers made in Germany, and we have examined all the principal ones that have, since the new glass has reached London, been made there; and we are bound to say that the English work, based on the principles laid down by Abbe, is so fine as to make the regret immeasurably keener that English microscopical literature has been for all these years a blank, for practical purposes, on the theory and principles of optical construction, and on the theory of microscopical observation and interpretation. Such a paper as that of Prof. G. G. Stokes, P.R.S., on the question of a theoretical limit to the apertures of microscopic objectives (Journ. R.M.S., vol. i. p. 139) from its very loneliness only gives emphasis and point to our contention. Those who have any doubt of the full force of what we are here contending for, have only to compare a dry $\frac{1}{6}$-inch objective, say of twenty-five years ago, made

[1] "On a Large-angled Immersion Objective, without Adjustment Collar with some Observations on Numerical Aperture," by J. W. Stephenson F.R.A.S. (Journ. Roy. Micros. Soc. vol. i. p. 51).

by the best makers in London, with a well-chosen water-immersion of ten years ago; and both these with a recent homogeneous glass of the same power with a numerical aperture of 1·5. Or still better, a dry $\frac{1}{50}$-inch objective, of the same date and the same makers, of numerical aperture 0·98, with a water-immersion lens of the same power of say ten years ago, having an aperture of 1·04, and a recent homogeneous $\frac{1}{50}$-inch, with a numerical aperture of 1·38. Still more strikingly, let the same observations be made with a dry $\frac{1}{12}$-inch objective of twenty years ago, with a numerical aperture of 0·99, and a homogeneous lens of the same power, with numerical aperture 1·5; and, finally, both these with an apochromatic objective of the same power by the same London makers and an aperture of 1·40. We venture to say, to histologist, bacteriologist, diatomist, and all other serious workers with the microscope, that there can be no proper comparison of the results; or, rather, the comparison is odious indeed for the oldest, and even the elder, lenses.

But, as we have stated, it is to Germany we are indebted for the *knowledge* out of which, alone, these improvements could have arisen. In spite of the length and abundance of English treatises on the microscope, it has never been part of the scope of the respective authors to do other than make the scantiest reference to the principles of the microscope; and nothing is found that will elucidate the theory of the construction of objectives, and eye-pieces, and the possible and real relations of each to the other. There is nothing to be found indeed in our language, except in the invaluable translations published in the successive Journals of the Royal Microscopical Society, which discusses the phenomena of diffraction, of polarization, of the principles of the true interpretation of microscopical images, and the theory of work with the microscope. English workers with high powers have discovered painfully where their lenses during many years were at fault; they could show our opticians *what* they wanted; but it has been only as the result of the laborious mastery of the theory of lens-construction by German investigators, with Abbe at their head, that the English worker has been able to get his wants, in object-glasses and eye-pieces, supplied.

But like all advances in insight and analytical power, these very improvements, so welcome and so helpful to searchers in many important branches of science, only open up the horizon of the unknown more fully; and the very knowledge we get, through the inestimable improvements, only reveals new difficulties; and again creates optical wants. It is, then, with pleasure indeed that we hail this excellent translation of Naegeli's work on the theory and practice of the microscope The book has long been announced, and many have looked, year after year, eagerly for its coming. But a series of untoward circumstances have combined to make the delay inevitable. The translation was begun some ten years since by Mr. Frank Crisp, the Secretary of the Royal Microscopical Society, purely in the interests of microscopy in England. He wished to fill the blank in the microscopical literature of the country, which had, in fact, become almost a dishonour to us. This book of Naegeli and Schwendener is a thorough treatise on the theory of the microscope, giving a detailed theoretical exposition of the construction of objectives, eye-pieces, &c., with analytical determination of the path of the rays in refracting systems; discussing exhaustively chromatic and spherical aberration; the influence of the cover-glass; the flatness of the field of view; the centering of systems of lenses; the importance of aperture, with a discussion of the diffractional action of the aperture of the lenses; and the question of illumination.

With equal care and thoroughness there is a discussion of the testing of the microscope, in all its branches, which cannot but make the student conversant with every essential point in the construction of the instrument; and an absolutely invaluable monograph on the theory of microscopic observation, which no one attempting to publish results of any importance dare leave unread or even unstudied. The phenomena of polarization receive equal care in treatment and must prove of the utmost value.

To put such a book within the reach of English readers, Mr. Crisp rightly felt, would be to give the needed stimulus to English microscopical observation: it would put them on the same horizon with German specialists. But the first impediment to its appearing in print was, that Mr. Crisp was compelled, by the weight of other claims upon his time, to relinquish the task of preparing the translation for the press when only eighty pages were in type; and a large lapse of time ensued before the labour was at length resumed by Mr. John Mayall, Jun., one of the editors of the Journal of the Royal Microscopical Society. But, beyond this, when Mr. Mayall had done his work and the printing of the work was complete, a fire destroyed the premises of the printer and all but a small portion of the type was wholly lost. The present issue is therefore an entire reprint.

There is but one point that we can see in the book, as it now stands, that need call for the slightest reflection: it is that the authors adopt, and discuss at considerable length, a method of testing the resolving power of objectives which has had—in another connection—the honour of a mathematical refutation by the highest living authority on microscopical optics, Prof. E. Abbe, of Jena. This method consists of viewing, with the objective to be tested, what were assumed to be "miniatured images" of a network of wire gauze produced by minute globules of oil and other matters, which images were supposed to be reduced to the "limit of discrimination" by simply distancing the wire gauze from the oil globule. Prof. Abbe's demonstration (*vide* Journ. Royal Micros. Soc. 1882, pp. 693-96) of the fallacy of this method proves that the combination of a microscope with a minute oil globule, or its equivalent, for viewing a distant object—whether wire gauze, or a so-called "double star" arrangement as advocated by Dr. Royston Piggott—serves no purpose whatever in determining the limit of the resolving power of the objective; but merely produces a very low-power telescope; the power of which may easily be so low, indeed, that the eye fails to differentiate, or even to perceive, the image!

The adoption of this fallacious mode of reasoning, however, amounts only to a blemish in an otherwise most excellent work; and with the publication and accessibility of Abbe's correction can do but little harm.

It would have given a still higher value to the book if the chapters devoted to an exposition of Prof. Abbe's

views on the formation of images in the microscope had received the advantage of his personal and later revision; but it is none the less due to the authors to acknowledge the credit that is justly theirs, for the very early recognition of the value of his investigations; and for the earnest manner in which they endeavoured to embody those investigations in a popular text-book at a date (1877) when hardly more than the barest outlines of the subject had been published by Prof. Abbe himself.

We note that the authors give the preference to daylight over lamp-light, believing that it exerts less strain upon the eye. We suspect that the majority of English observers, especially at continuous work, and with high powers, will be inclined to reverse this judgment. Extremely white and intense light can be obtained from good modern lamps, and, unlike daylight, it is unvarying, devoid of caprice, and easy of manipulation. But this is a matter, perhaps, in some sense subjective, and not of vital moment.

The world of science generally and of microscopical science in particular, is deeply indebted to Mr. Crisp for initiating this translation, which, we have taken pains to find, is most carefully done; and to Mr. Mayall for his part of the laborious undertaking. We can only hope, in the interests of English and American science, that it will find a large circle of careful readers on both sides of the Atlantic; and we warmly concur in the hope expressed in the preface, "that the volume may be supplemented before long by an English version of the further researches in microscopical optics by Prof. Abbe, of Jena, which have extended so much our knowledge of the matters dealt with in Naegeli and Schwendener's work."

W. H. DALLINGER.

THE CRUISE OF THE "DIJUMPHNA."

The Cruise of the "Dijumphna." With Reports of the Zoological and Botanical Results of the Voyage. By R. Bergh, J. Deichmann Brandt, J. Collin, H. Hansen, T. Holm, C. Jensen, G. Levinsen, C. Lütken, L. K. Rosenvinge, M. Traustedt, and N. Wille. (Copenhagen, 1887.)

THE Danish Arctic Expedition of 1882-83 owes its initiative to its able commander, Lieut. Hovgaard, of the Danish Navy. This enterprising officer, whose practical experience of Arctic navigation gave great weight to his opinions on the subject, had early in 1882 published a pamphlet, entitled "Suggestions for a Danish Polar Expedition," in which he advocated his own theories regarding the distribution of land and water in the Arctic regions, and the feasibility of finding some hitherto untried route for circumpolar exploration.

In response to his appeal for means to test the accuracy of his opinions, a private individual, Herr Gamél, of Copenhagen, placed at his disposal a screw-steamer, since known as the *Dijumphna*, whose equipment for Polar explorations and scientific observations was supplemented at the expense of the Danish Ministry of Marine.

Thus well prepared, the Expedition left the Copenhagen Roads, July 18, 1882, but unfortunately the *Dijumphna* early encountered ice, which was found to be so dense south of Cape Tschernui Noss that it was only after a delay of more than four weeks off the south-west coasts of Nova Zembla, that an entrance could be made into the Sea of Kara, where, in accordance with Hovgaard's anticipations, the water was clear. The hopes of success to which this fact gave rise proved, however, delusive, for the ice began to re-form so rapidly that, within a few days of their passage into the Kara Sea, it had become apparent that the *Dijumphna* was fast bound for the coming winter; and it was only after nearly a twelvemonth's detention that the ice began to loosen, when the westerly trend of the drifts carried the ship, in the August of 1883, back towards the entrance of the Sea of Kara. With a broken screw and failing supplies, there was no alternative but to renounce all hope of advancing further east, and, accordingly, by help of sails the *Dijumphna* began its homeward voyage, which was so retarded by ice-drifts and storms that the harbour of Copenhagen was not reached till December 3, 1883.

In the course of the winter the sun remained below the horizon from November 20 to January 22, the temperature at the latter date falling as low as $-47^{\circ}\cdot9$ C.; while there was constant danger of being crushed in the ice, or carried with moving drifts on the shore. Yet, notwithstanding these drawbacks, the trawl and dredge were diligently used at 190 different stations, ten of which were in the Jugor Schar and in Olenje Sound, off the south-west coast of Nova Zembla. Most of the deep-sea soundings were carried on in the Sea of Kara, between 69° 42′ N. lat., 64° 45′ E. long., and 71° 46′ N. lat., 65° 14′ E. long., within which limits the ship was moved forward and backward by the ice-drifts. This ground proved specially rich, and Herr Holm, the efficient naturalist of the Expedition, was able to bring home an exceptionally large number of well-preserved botanical and animal collections, which now form a valuable addition to the contents of the Danish National Museum, to which they have been generously ceded by Herr Gamél, the owner of the *Dijumphna*.

Herr Holm's report of the flora of Nova Zembla, which he examined at twelve distinct localities during the *Dijumphna's* long detention off the coast, confirms the statement of Von Baer as to the abundance of vegetation on the tundras, but he differs from him in regard to the mode in which plants found their way into these high latitudes. According to Von Baer, to whose report of his scientific mission, undertaken for the Russian Government in 1837, we are indebted for our first acquaintance with the Nova Zembla flora, its plants have all been stranded from neighbouring shores through the agency of drifting ice. Herr Holm, on the other hand, believes that a few forms may be survivals from pre-glacial periods, but that the presence of the majority is due partly to the agency of birds, of which large numbers, more especially Tringa and other waders, frequent the shores, and partly to the winds, and to ice-drifts. Insects are too rare to affect the question of plant-propagation, and his observations—that most plants on the tundras have the corolla directed upwards, while pendent or drooping forms are very rare, and that the majority are scentless, and of one uniform colour—appear to favour these views; although it is possible that the existing flora may also to some extent be due to self-fertilization.

The general appearance of the tundras is that of a slightly irregular plain, the irregularities being due to the tuft-like character of the patches of vegetation, which are separated by pools and streams of melting ice, from which innumerable mosses emerge. When closely examined, these tufts are found to consist of plants dwarfed out of all resemblance to their more southern congeners; thus, *Salix polaris* never rises more than 2 inches in height, although the number of its annual layers of growth—consisting only of five to six cells—may indicate an age of thirty years. Considered generally, the Nova Zemblan flora consists of twenty-eight families of Dicotyledons, four Monocotyledons, and four Cryptogams. Among the Phanerogamæ the most largely represented are the Gramineæ, of which thirty-one species have been distinguished. Curiously enough, it is found that contrary to their habits in more southern regions the Dicotyledons flower earlier than Monocotyledons, which contribute the larger proportion of the flora of the tundras, both as regards species and individuals. The number of new phanerogamic forms derived from the *Dijumphna* Expedition scarcely exceeds a dozen, and of these the most interesting are *Salix arctica*, *Glyceria tenella*, *Potentilla emarginata*, and three species of Carex, viz. *C. incurva*, *lagopina*, and *hyperborea*. As many as eight species of Saxifraga were met with, while Phaca is the only representative of Papilionaceous plants.

Special interest attaches to the collection of mosses brought home by Herr Holm, and examined by Herr C. Jensen, whose report shows that among the entire sixty-four species, of which fifty-one belonged to the tundras and the cliffs of Nova Zembla, three were genuine Arctic forms, viz. *Voitia hyperborea*, *Bryum obtusifolium*, and *Amblystegium brevifolium*. In Wulfberg's report of the mosses collected in the Norwegian Expedition of 1872, which is the only other notice of the Arctic Cryptogams, only twenty-four are noted, so that we owe our acquaintance with forty species to the industry of the *Dijumphna's* collectors. Herr Holm was equally fortunate in finding hitherto unrecorded fresh-water Algæ in South-West Nova Zembla; but in regard to the marine Algæ he has little to record that had not been previously made known, while he corroborates the statements of earlier explorers as to the luxuriant profusion of gigantic Laminariæ, which fringe the coasts at a depth of from 1 to 5 fathoms, where he obtained fronds of *Alaria esculenta* more than 15 feet long.

In passing to the consideration of the zoological results of the *Dijumphna* Expedition, we must admit that excepting in regard to the Invertebrata, for whose capture no better hunting-grounds than such Laminarian forests can be wished for, the results are negative rather than positive. Of the higher marine Vertebrates only *Phoca fœtida* and *Odobænus rosmarus* were seen. A few foxes were noted, and a young she-bear was shot, which was the only specimen of big game attainable. Fishes, mostly belonging to Icelus, Lycodes, and Liparis, were taken so sparsely in from 49 to 106 fathoms that only twenty-eight out of the entire 190 trawls yielded a single specimen. In regard to the Invertebrates the yields were, however, enormous, showing an astonishing abundance of animal life in the Arctic waters. Thus, one haul brought up 928 specimens of *Glyptonotus entomon*, 300 of *G. Sabini*, besides enormous numbers of Alcyonidæ, Sponges, Actinias, and other Polyp forms. Nor was this an exceptional case. In the Sea of Kara the Echinodermata ranked first as to individual numbers, but Crustaceæ as to species, eighty-two of the latter having been determined, of which ten belonged to the family of the Pycnogonidæ. Among Crustaceans generally, seventeen new species have been established by Herr Hansen, whose report supplies much interesting and novel information in regard to the structure of the foot-jaws of the Isopoda, of which he proposes to treat more in detail in a special monograph on the buccal organs and antennæ of the most important Crustacean types.

Gastropods and Annelids were of rare occurrence, and only one genus of Cephalopods, Rossia, was observed. The Simple Ascidians, which have been carefully studied and reported on by Dr. Traustedt, have relatively speaking yielded many novel results, while five of the eight species collected are new, of which the most interesting are *Phallusia dijumphniana* and *P. glacialis*.

The volume in which the various reports on the *Dijumphna* collections are contained is well got up, like other works of a similar character that have been brought out under the joint co-operation of the authorities of the National Museum of Denmark, and of the Carlsberg Institute. The latter of these bodies has liberally advanced the funds necessary for meeting the expenses of publication, in anticipation of the grant of 10,000 kroner to be voted for the purpose in the next year's Parliamentary Budget.

The work has been carefully edited by Herr Lütken, who contributes the monograph on the fishes, and to him foreign readers are indebted for a French *résumé* of the report on the vegetation of Nova Zembla, and for a general summary of the fauna of the Sea of Kara in the same tongue. Besides his very complete botanical reports, Herr Holm contributes a short prefatory account of the cruise, which, if it unfortunately failed in adding to our geographical knowledge of the Arctic regions, has at any rate supplied naturalists with much valuable material towards a closer acquaintance with the conditions and forms of vegetable and animal life in those high latitudes.

EXERCISES IN QUANTITATIVE CHEMICAL ANALYSIS.

Exercises in Quantitative Chemical Analysis; and a Short Treatise on Gas Analysis. W. Dittmar, LL.D. (Glasgow: William Hodge and Co., 1887.)

IT has probably been the case with all books on practical chemistry, and especially quantitative analysis, that in the first instance a rough plan or outline of the work was used by the teacher in his laboratory, there to undergo a process of extension and development. In some cases this development has gone on until we have such classical compilations of tried analytical processes as Fresenius's quantitative or Crookes's special methods. This seems to be a natural plan. Try your plan on your own students, and, if there a success, publish for the possible benefit of a wider circle. There is only this difficulty,

that, outside certain fundamental operations and stages in teaching, teachers and schools differ considerably in detail, and it is precisely on this detail, or order of importance in some cases, of work that a teacher prides himself —or thinks he has the right plan—as being able to turn out the most satisfactory students or to save their time.

The author of this book in his preface tells us that a preliminary edition was issued a little more than a year ago for his own students, and that the work had been even before then in use as a typographed book for some time. He likewise makes some remarks about the drilling of students in the beginning of their quantitative exercises with which we fully agree. Our experience is that a student requires standing over during the first four or five quantitative exercises. If the author's production shortens that ever so little, it will be a service to teacher and student alike. As to the interpolation of a preparation, the importance of this has scarcely been recognized by teachers. There is no doubt that a judicious selection of preparations, the end product of which is to be analyzed, is one of the best methods of preparing young students for practical analytical work.

After the exercises in weighing and measuring and determination of specific gravities of solutions, the book proceeds to a series of exercises in analytical methods. In these methods lies at once the strength and weakness of the book. We have a considerable number of methods for the analysis of things—salts, &c.—of technical importance, the performance of which would leave a student in a strong position as regards practical knowledge; but it is very questionable indeed if the average student could work through the majority of these, in the absence of the instructor, from what is given in the shape of directions. The exercises under separation are very well selected. They include a number of ores and alloys, silicates, &c. In the process of separation of lead and antimony by chlorine (p. 137) the author might have improved on the use of manganese by using permanganate, the evolution of chlorine is more regular.

Then follows combustion analysis for C, H, and N, and gas analysis. The latter forms the largest and best section of the book. It is mostly taken from, and is after the style of, Bunsen's gasometry. Other methods or modifications are also discussed as far as is requisite in a book of this nature. We have, finally, a number of "promiscuous exercises" in applied analysis: sea-water —mostly after the author's report on the composition of ocean water—milk, butter, and other substances.

On the whole, the book is a careful compilation and arrangement of work for students, bearing unmistakable evidence of the author by the references to his work and methods. We take leave to object to "Knallgas" as not being very generally understood by English students. It is not much shorter than electrolytic gas, and although the employment of it is explained it serves no very useful end. But this and one or two other details are not great objections, and do not detract from the utility of the book, which attempts perhaps too much, but may be fairly commended to those students of chemistry intending to become analysts, especially of technical products.

W. R. H.

THE STUDY OF LOGIC.

A Short Introduction to the Study of Logic. By Laurence Johnstone. (London: Longmans, Green, and Co., 1887.)

THERE is naturally some interest attaching to a book on logic which bears the *imprimatur* of Cardinal Manning, and of which a responsible member of the Society of Jesus can say *nihil obstat.* The Jesuits have long been famous teachers, and it is possible that those who find elementary logic an unsatisfactory teaching-subject may glean some useful hints from this little volume.

From a point of view outside the Roman Church, the perennial difficulty in the study of logic consists in the fact that no firm line can be drawn between the most elementary logical doctrines and the highest possible flights of philosophical reflection. As logic is taught by and for free-thinkers, both student and teacher are in a constant state of climbing ladders only to kick them down. At all stages a higher and a lower logic are at variance, or rather the higher logic consists in nothing else than a criticism of the lower. Distinctions that have been our mainstay become mere obstacles: our later views are mostly not additions to the earlier ones, but subversions of them. Hence there is little beyond the bare history of the subject, and a few of the less important technicalities, that can be taught with any authority.

In Mr. Johnstone's book we find throughout a wholly different attitude taken. With quiet simplicity, questions over which modern philosophy has spent much heat and labour are boldly prevented from arising. Thus a student may read, on page 10, under the heading "Action of the Intellect," that "the mind is a *tabula rasa* before it receives any impressions from without. It receives impressions, or the matter for ideas, through the senses, upon which the impression is made. By means of the 'sensus intimus' man becomes conscious of these impressions, of which the imagination then forms a picture, or phantasm." And then "from the picture on the imagination the intellect draws that element which is akin to itself, that is the immaterial incorporeal element, throws it into its mould—so to say—and the result is the 'species intelligibilis,' formed in the intellect itself, and representative of the exterior thing." What could be more final and satisfactory? It is not everyone who is free to make so short a piece of work of one of the largest of all philosophical questions. And so the student gets something that he can definitely carry away, and produce on paper when required.

Another noticeable feature is the revival, throughout the work, of many distinctions which have dropped out of sight in our modern text-books, or are at most referred to vaguely there with a passing smile at the "fruitless subtlety of the schoolmen." These can plainly be made to serve two purposes,—they provide abundance of material for the student to exercise his memory upon, and their effect as a whole must be to keep as separate as possible the process of using the machinery of logic, and that of seriously criticising our own beliefs. It is only from the free-thinker's point of view that any real desire can be felt to make logical criticism practically effective to the utmost. If we are anxious above all to

guard some piece of faith as such, then the more wordy our logic the better. And the delight in "naming our tools" may be carried to any length without fear of unpleasantness. It need commit us to no more than did Mr. Micawber's plan of docketing his unpaid bills. Here, however, it must be left an open question whether the modern practice of ignoring so many carefully-made divisions is an improvement or the reverse. Both views are at least respectable. In any case the elaborate details of the machinery by which our religious creeds are to be kept sacred contain much that ought to be of interest to all. What with *criteria per quod* and *secundum quod* (pp. 168, 191), with different "spheres" of truth (pp. 175, 202, 203), and different kinds of certainty (pp. 161-68), with truths which are "not intrinsically evident, but nevertheless extrinsically evident, or, rather, evidently credible" (p. 200), one may learn to admire heartily the care and cleverness employed so freely in mediæval times by those who felt the need of warding off awkward questions. It is certainly no light problem, how logic may be taught without encouraging the dangerous practice of doubting what we are told.

There are other signs of hard work in this book, besides the patience with which the author has studied the scholastic doctrines. For teaching purposes there is nothing so useful as examples, and here the examples given are numerous, mostly new, and sure to be helpful to the learner. Only those who have tried know the real difficulty of clearly illustrating statements so general as those of logic without some appearance of triviality. In this respect also Mr. Johnstone has succeeded unusually well.

ALFRED SIDGWICK.

OUR BOOK SHELF.

Light and Heat. By the Rev. F. W. Aveling, M.A., B.Sc. (London: Relfe Bros., 1887.)

THIS is an elementary text-book intended to cover the syllabus of Light and Heat for the London Matriculation Examination. Being written more in the form of notes than as an ordinary book, it will be of considerable service for examination-purposes. Many of the definitions, however, are far from concise, and many phenomena which admit of easy explanation are left unexplained. On p. 98 we are told that the specific heats of gases are inversely proportional to the square roots of their densities, whereas they are in inverse proportion to their densities; had a simple explanation of this relation been given, the mistake would not have occurred. The important subject of thermo-dynamics is disposed of in four pages at the end of the book: this is not as it ought to be, seeing that the relation between heat and work often enters into previous discussions, and is, moreover, the basis of the modern theory of heat.

The sketches are of a rough-and-ready kind, such as a student would be expected to make in an examination, and, as such, give many useful hints. The coloured plate of spectra, however, is as useless as the majority of similar ones, as practically no explanation of the meaning of a spectrum is given; dark lines are shown in the spectrum of potassium, but these are no doubt due to a mistake of the lithographer. Such exhibitions as these, which are far too common, show a want of respect for the labours of those who have done so much to further our knowledge of spectrum analysis.

A large number of good numerical problems, with answers, are distributed throughout the text, and several typical ones are fully worked out.

A. F.

Animals from the Life. By H. Leutemann. Edited by Arabella B. Buckley. (London: Stanford, 1887.)

THIS work, which forms a charming introduction to the study of zoology, is just the thing for young children who have a turn for the subject, and at the present time, since presents are being made on all sides, would make a very useful and enjoyable gift. From it they will be able to become acquainted with the various forms of living creatures without having to make a laborious study of natural history, which few care to do. A great amount of knowledge can be gained by merely looking at the illustrations, which are got up in a very intelligent and accurate style; they are 255 in number, and well coloured, and represent animals, including birds, insects, fish, &c., as they are found in their natural state.

The accounts of the various forms and habits of the different animals (each plate having about a page and a half of letterpress with it), are written so very clearly and in such a natural way that anyone who peruses this book will find plenty that will be extremely interesting.

In adapting the original text to the wants of English children, Miss Buckley has had to alter it in many places, English examples and references being substituted for German ones.

The Vegetable Lamb of Tartary. By Henry Lee. (London: Sampson Low, 1887.)

IN former times it was generally believed that there existed in the East a mysterious "plant-animal," variously called "the vegetable lamb of Tartary," "the Scythian lamb," and "the Barometz," or "Borametz." The usual explanation of this notion is that it originated from certain little lamb-like toy figures constructed by the Chinese from the rhizome and frond-stems of a tree-fern. Mr. Lee, however, holds that the idea came into Europe from Western Asia, and that it referred in the first instance to the cotton-pod. This theory he works out thoroughly in the present little work, and in the course of his argument he has brought together many curious and interesting facts, the significance of which is made more plain by a number of good illustrations. In a separate chapter Mr. Lee treats of the history of cotton, its uses by ancient races in Asia, Africa, and America, and its gradual introduction among the nations of Europe.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

The Royal Horticultural Society.

THE affairs of the Royal Horticultural Society alluded to in the last issue of NATURE (p. 145) have lately obtruded themselves upon public attention, but it is probable that some readers of NATURE may consider that they have little concern with such a body. They may look on horticulture in the light of a pleasant pastime, or of a more or less profitable commercial enterprise, they may regard flower-shows as a means for the display of fashionably-dressed ladies, or they may look on the Royal Horticultural Society as an association for the production and distribution of medals and certificates of more commercial than scientific importance. But there are other considerations beyond these, and whilst naturalists may be indifferent to much of the past history and much of the present work of the Society, to the internal dissensions and to the action of the landlord Commissioners towards their unfortunate tenants, the scientific work of the

Society may well excite their sympathy. A moment's consideration will show that the progress of horticulture is largely based on the correct application of scientific principles. What is not so familar to most people is the extent of the obligation under which science generally lies to horticulture. Should any reader require an illustration of this, let him turn to the "Origin of Species," and specially to the "Variation of Animals and Plants under Domestication." There is scarcely a page in the chapters of those volumes relating to plants that does not abound in references to the practices and the discoveries of horticulturists. Fertilization, cross-breeding, hybridization, selection, grafting, the limits and nature of variation, are only a few of the subjects on which horticulture furnishes the largest and in many respects the most trustworthy body of evidence yet available. That these subjects are studied, and that the experiments are made, not so much from a scientific as from a utilitarian point of view, is surely no matter of reproach. On the contrary, it is the business of horticulturists to act as they do, but without the aid of a Society much of the experience gained would be lost to science. All this might be admitted as a general principle, but yet its concrete application to the Royal Horticultural Society might be from various causes inappropriate. The Society in question has, however, distinct and undoubted claims to recognition for the good work it has done in science for a long period of years. The services it has rendered to science by its collectors, and the still greater value of its work in all departments of practical horticulture, should have secured for it more sympathetic and respectful treatment from its landlords. Among Societies deserving of national support and encouragement on the ground of public utility, there are few, indeed, that have greater claims than this. For years it has unfortunately been hampered by the necessity of providing amusement for a body of Fellows and visitors who cared nothing whatever for horticulture in its higher aims. Now there is a chance of the Society bursting its bonds and confining itself to its proper work —the promotion of scientific and practical horticulture. The plan of catering for fashionable idlers has proved disastrous. While horticulture proper was starved, and thousands upon thousands of pounds were utterly wasted, the landlords retain the whole of the property on which their tenants expended so much, and the Society has to seek a new home. In spite of all this, however, a valiant attempt has been made throughout long years of depression to maintain the scientific traditions of the Society. There has always been a small body of Fellows who have been mindful of the obligation which Thomas Andrew Knight, so long the President of the Society, imposed upon his successors. Lindley for forty years maintained the scientific interests of horticulture in the Society, and he was assisted by Royle, by Bentham, and many others. Twenty years ago, or more, a Scientific Committee was appointed, and this body, recruited by new accessions each year, still continues its labours. Under its guidance experiments have been performed in the Society's Experimental Garden at Chiswick; under its sanction have been published numerous Reports of very great scientific interest and importance; and much more might and could have been done but for the lack of means, or rather their diversion for more questionable purposes. The Committee in question consists of some twenty or thirty naturalists of all denominations—botanists, chemists, geologists, entomologists—associated with amateur and professional gardeners interested in science. To this body are referred for discussion and investigation the most varied objects of natural history and vegetable pathology; before this body and its sister committees are brought all new introductions, whether of natural origin, or produced by the skill of the gardener, and which have any scientific interest. Sir Joseph Hooker is the Chairman of this Committee, the Rev. M. J. Berkeley was for many years its Secretary, and a large number of the most eminent biologists, chemists, and geologists have been or still are among its members, giving their services without fee or reward, simply in the interests of scientific horticulture. On these grounds, therefore, the sympathy and co-operation of those interested in science may be claimed on behalf of the Royal Horticultural Society. A new programme has been decided on in principle, a new home must be provided at once, for the purpose of the Society's meetings and for housing the Lindley Library. This library, it may be added, is held in trust for the benefit of the Society, and is from time to time enriched by donations and by purchase, so far as the very meagre income of the Trustees permits. The donations would be much more numerous were it generally known that the library, though primarily intended for the Fellows, yet is under certain restrictions available to outsiders, so that, though housed in the Society's rooms, it cannot be sold or made away with in any financial catastrophe which might overtake the Society. Such a fate, however, seems to be averted at present; the Society's debt is not large, and some members of the Council, or other friends, have made a good beginning by inaugurating a fund, to be used for the housing of the Society, so that ere long we may hope to see the old Society established on a more secure basis, and more potent than ever to advance those interests of science committed to its special keeping.

MAXWELL T. MASTERS.

Classification of Clouds.

As one who has been engaged for nearly forty years in working up the materials for a monograph on clouds, I suppose I deserve the name of a "specialist in clouds" as much as any one. Yet I decline, for reasons which I will hereafter state in an appendix to my volume, to be altogether bound by the outlines of classification which my friends Prof. Hildebrandsson and the Hon. Ralph Abercromby appear to lay down (NATURE, December 8, p. 129 *et seq.*), although they adopt several of the names which come from my mint. I fully adopt the opinion implicitly held by Mr. Abercromby, and stated by my friend Captain Barker (*ibid.*)—from whose classification, however, I differ in one important point—that all ordinarily careful observers will readily comprehend the broad and simple distinctions expressed in any fairly good classification. Nevertheless, I believe that the apparently slow progress of this branch of research, and the tediousness of the work thrown upon the classifier, are matters on which we should congratulate ourselves, since every year adds something to our knowledge of those physical and structural processes which form the basis of all true classification; and I trust that some years may pass before an International Congress may attempt finally to set its seal upon any nomenclature or classification of clouds.

W. CLEMENT LEY.

Effect of Snow on the Polarization of the Sky.

THE polarization of the sky has been shown experimentally by Tyndall and theoretically by Lord Rayleigh to be due to fine particles suspended in the atmosphere. According to both, the sunlight scattered at right angles to its original direction by very small particles is completely polarized in a plane through the sun. In observation, however, we find the light from a region of the sky distant 90° from the sun is only partially polarized. This is due to that portion of the atmosphere being illuminated not merely by the sun, but also by the rest of the sky and the surface of the earth, and partly also no doubt to some of the particles not being sufficiently small compared with a wave-length. From these considerations we may expect that a fall of snow would cause a considerable diminution of the polarization. This expectation has been fulfilled in some recent observations of mine here at 6000 feet above sea-level. My polarimeter consists essentially of two piles of glass plates to depolarize the sky light; and a crystal and Nicol prism to test the depolarization. Owing to the strength of the polarization at this altitude, I find it necessary to use two piles of glass plates separated by two or three inches. This arrangement diminishes the number of double internal reflections, and so is a much more powerful polarizer or depolarizer than the same number of plates combined into one pile. As I have not seen this important practical consideration noticed before, I may point out that, in addition to the light refracted directly through the pile, there are a number of portions twice reflected. One of these for instance is reflected first at the second surface of the last plate, and secondly at the first surface of the last plate. The number of such twice-reflected portions for n plates is $n(2n - 1)$. When, as in my instrument, the fixed pile is much inclined, no light can reach the edge after being reflected first by one pile and then by the other. If the two piles were combined into one, I should have 120 portions twice reflected; as it is, I have only 60. This increases the polarizing power of the instrument by at least one-third.

The crystal is a thick plate of Iceland spar cut so that the light passes along the optic axis. The fixed pile of three plates has its normal inclined at 47° to the axis of the crystal. The movable pile of five plates has an index attached, which gives the inclination of its normal to the axis of the crystal. This inclination is the reading of the polarimeter.

St. Moritz lies on the northern slope of a valley running from south-west to north-east. At the beginning of the observations the opposite slope was buried in snow, but the northern slope both above and below the point of observation was almost free from snow. Thus the most brightly illuminated part of the ground surface was of a dull brown or gray colour. Under these circumstances, the reading was about 50° in the middle of the day, being a little higher earlier and later, viz. about 52° at 10 a.m. (date October 21 and 22). These readings, as well as those mentioned below, refer to the highest point of the sky, which is distant 90° from the sun, and were taken when the whole sky was free from cloud. On October 26, after a five-inch fall of snow, the reading was 41° at 10.15 a.m.

By October 29 most of the fresh snow had gone, and I found at 11.40 a.m. the reading as high as 48°. After this we had several feet of snow, and at 12.50 p.m. on November 13, the reading was again 41°. Each of these readings is the mean of four, and I find two readings of the same thing seldom differ more than 2°. Hitherto I have not been able properly to evaluate the readings of my instrument in absolute measure, though I hope to do so later. But to gain an approximate idea of their meaning, I have calculated the polarizing power of the two piles on the assumptions—first that Fresnel's laws of the reflection of polarized light are accurate, and secondly that the index of refraction of my plates is 1·52. We may consider the light from the sky as consisting of two parts completely polarized, one in the plane of the sun, and the other perpendicular thereto. The ratio of these parts is ·376 for the reading 40°, and ·271 for the reading 50°. Again we may divide the light into a part unpolarized and a part completely polarized in the plane of the sun. The ratio of these parts is ·546 for 40° and ·428 for 50°. So it seems fair to conclude that the light reflected from the fresh snow was sufficient to increase the unpolarized part of the sky light by more than a quarter.

JAMES C. MCCONNEL.

St. Moritz, Switzerland, December 10.

The Ffynnon Beuno and Cae Gwyn Caves.

I WILL answer Dr. Hicks's question in as few words as possible. Nothing is to be gained by terming me a "highly prejudiced" observer, or by saying my views are of "no consequence" and "not worth anything." Your readers can form their own conclusions on these points. I am not "highly prejudiced" against, neither have I any "bias against," the existence of pre-Glacial man or of his "migrations"; on the contrary, I favour these subjects.

I *did* see the section of drift exposed at the Cae Gwyn Cave, and I can hardly describe it (from my own point of view) without giving offence. My view is this: the section showed nothing but rain-wash derived from the closely-adjoining non-Glacial drift. The section showed a re-made deposit, horizontally stratified, and with stones resting on their flat sides. No doubt there were Glacial stones in the rain-wash, derived from the ever-shifting post-Glacial marine drift close by; the latter being merely a re-laid Glacial drift. Stones with Glacial scratches may be found in the lower gravels of the Thames.

To me, the caves and their surroundings are in the highest degree suspicious, and in size insignificant, and not comparable with large and typical caves. They are small and painfully narrow tortuous passages only, on a hill-side, and *close to the surface*. The lower cave is furnished with a very large hole, opening up to the surface just above; and the upper cave had at one time a similar opening. The post-Glacial drift above is always on the move, and every shower of rain brings it down with its derived stones.

Since writing to NATURE, in November 3, I have referred to some of the papers published on these caves. I turned first to the list of mammalian remains, only however to find that the animals (like the implements) are entirely *characteristic* of the most recent post-Glacial deposits. Even near London we get in gravels of no great comparative antiquity the bones of *Elephas antiquus*, but in the caves merely *E. primigenius* is found. As regards antiquity, the animals no doubt overlap at both ends of the scale, but their meaning, as found in these caves, points in one direction only, and that is to the most recent and not to the most remote of Palæolithic times. None of the cave mammals are *characteristic* of pre-Glacial deposits.

It would seem that Dr. Hicks does not realize the nature of Dr. John Evans's criticism. La Madelaine is the newest of caves, and represents the most recent of Palæolithic times: it is a kind of connecting link between Palæolithic and Neolithic times. Therefore, if Dr. Evans's criticism is taken with mine, the two *clearly prove* that there *is* a distinct chronological value in the classification, not that there is "no chronological value" as concluded by Dr. Hicks. Dr. Hicks also appears not to realize the fact that river-drift and cave implements do not only differ in roughness and abrasion but in style. The cave men used different implements from the river-drift men, they were changing from savagery to barbarism. If Dr. Hicks produces implements made by pre-Glacial men, he must show us something obviously older than the oldest river-drift tools, not fall back upon refined tools which are, to re-quote Dr. Evans, "precisely like many from the French caves of the reindeer period, such for instance as La Madelaine." If Dr. Hicks abandons his scraper, he is still in no better position, for his finely re-trimmed knife and the implement in the British Museum are identical in age and character with it. So are the flakes: the one with long narrow facets is characteristic of the latest, not of the earliest work. So is the pointed and drilled bone. No drilled bones have been found in moderately old river-gravels, and what is more, no instrument suitable for boring a small hole through bone has ever been found in such a gravel. Drilled bones and small flint drills belong to the very latest of Palæolithic times. In the remains of my own collection of Palæolithic implements I have here over a thousand examples of the major class, and an equal number of minor forms illustrative of the development of knife and scraper forms, but they give no support whatever to Dr. Hicks's conclusions; they all, in fact, point in a diametrically different direction. I am acquainted with Prof. Prestwich's views, and I believe I was the first person to find implements in the highest terraces of the Thames Valley; but I do not see that Prof. Prestwich's conclusions have any direct bearing on the Ffynnon Beuno and Cae Gwyn caves.

I do not suppose that any opinion of mine will influence Dr. Hicks, and I have no wish to influence him or any other observer. I merely wish to put on record the fact that, after many years' experience amongst drifts, and implements, and fossil bones, my conclusions are entirely opposed to Dr. Hicks's.

Dunstable. WORTHINGTON G. SMITH.

P.S.—Since the above has been in type, I have seen the report in last week's NATURE (p. 166), but I prefer to let my letter stand just as written before the report was seen by me. Prof. Hughes has cut away the geological and palæontological supports; I shall be content to resist the idea of the pre Glacial age of these caves on purely archæological grounds.—W. G. S.

The Planet Mercury.

THE planet observed on the mornings of December 7 and 9 by your correspondent "G. F. P." (NATURE, December 15, p. 151), was probably not Mercury but Jupiter, as these bodies were near together at the time, and the latter was by far the brightest and most conspicuous. The circumstances, described by "G. F. P.," under which the object was noticed render it certain that it could not have been Mercury, for the latter was decidedly small, and might have been easily overlooked on the several mornings I saw it early in the present month. Jupiter, on the other hand, was very bright and plain, and might easily attract attention in the way stated by your correspondent. On the 9th instant the two planets were about 3° apart, Jupiter being situated to the west of Mercury.

Had "G. F. P." really observed the latter planet, he would have instantly remarked its half-moon phase in his $3\frac{3}{4}$-inch telescope, and must have mentioned Jupiter, as well as Venus, as visible at the same time.

There is no difficulty in observing Mercury with the naked eye if the planet is carefully looked for in the proper spot, at the times of his eastern elongations in the first half of the year and at the western elongations in the last half. I have seen the planet on certainly more than fifty occasions. In May 1876 I noticed Mercury on thirteen different evenings. Sometimes the planet is quite conspicuous in the twilight as a naked-eye object.

W. F. DENNING.

Bristol, December 16.

Meteor of November 15.

IN NATURE of December 1 (p. 105) Mr. B. Truscott writes of a wonderfully fine meteor seen at Falmouth on the night of Tuesday, the 15th ult, and asks in effect if it was seen by other

eyes than his: so perhaps it may be permitted to be said that it was seen in the parish of Llanefydd, Denbighshire, by a correspondent of mine, who writes:—"On Tuesday night, November 15, while returning homewards on foot, happening to look eastwards I saw a long train of brilliant light suddenly flash out of the sky. At first I thought it was lightning. But instead of vanishing it descended with great rapidity, the light increasing in brilliancy as it neared the earth. The night was rather dark, although the sky was thickly studded with stars, but in a few seconds so intensely brilliant had the light become that a pin might have been picked up from the road with the greatest ease. While I was looking, the object that accompanied the flash burst, and displayed a magnificent mauve and red fringe of light. I say fringe, as it would be impossible for me to describe otherwise the shape, for it appeared to me to project shafts of light, some long and some short, like what would be the rays of a great star. There was in the direction in which I was looking a thick wood, and the effect on the trees of the silvery light I first noticed was richly beautiful. But the effect of the mauve and red light was magnificently grand, and to me in no little degree awful. The whole wood was enveloped in a red lurid glare, which lasted as near as I can calculate some six or eight seconds. The effect altogether was like a brilliant transformation scene, and the meteor having passed away, the darkness of the night seemed to be in the last degree intense."

J. Lloyd Bozward.

A Correction.

In the footnote, p. 152, second column, there is an unfortunate transposition for which myself more than the printers are to blame, which it is important to correct.

After the words "by Aristotle, Probl. 1 E 3 and Metaph. A 5" occur the words "which he attributes to Pythagoras . . . . on the nature of the Beast."

These words should have come at the end of the subsequent paragraph where I say that "Muhamad-al-Sharastani assigns *reasons* for regarding all the numbers up to 10 inclusive as perfect numbers."

It is these *reasons* which I speak of as being by him attributed to Pythagoras, &c.

I may take this opportunity of giving, as another example of the use of the New Nomenclature, the well-known *extended* Theorem of Fermat, which may be expressed by saying:

"Every number must divide the Fermatian of which the index is its *totient*, and the base any one of its *totitives*."

Athenæum Club, December 15. J. J. Sylvester.

ISOLATION OF FLUORINE.

ONE of the most difficult problems of modern chemistry has at last been satisfactorily solved. After three years of incessant labour, occasionally interrupted by temporary feelings akin to despair, M. Henri Moissan has at length isolated in considerable quantities that most baffling of elements—fluorine, and has been enabled to determine its principal properties. The experiments themselves are among the most interesting ever performed, and their details, as described by M. Moissan in the December number of the *Annales de Chimie et de Physique*, form the most fascinating reading. They must of necessity have been extremely costly, for by far the greater portion of the apparatus employed was constructed of platinum, and it is not often that one hears of a platinum tube 80 centimetres long and of 1½ centimetre diameter being destroyed in each experiment, as happened in the earlier stages of these researches.

The isolation of fluorine has formed a worthy object of the attention of chemists ever since the first remarkable experiments of Sir Humphry Davy, who was rendered dangerously ill by being exposed to the corrosive fumes of hydrofluoric acid. Although Davy was not successful in obtaining free fluorine, yet he brought clearly to light the nature of hydrofluoric acid, and proved it to consist of hydrogen combined with an unknown but extremely active element—fluorine. The history of all the attempts which have since been made to effect the preparation of free fluorine might occupy a volume, and it will therefore only be necessary to refer to the later work of our countryman, Gore, who, in 1869, published his researches upon the electrolysis of hydrofluoric acid, and of certain fluorides, and left our knowledge of the acid itself in a most complete state. M. Moissan, working in the laboratory of M. Debray, now steps in and achieves the result so ardently sought after during the last eighty years—another example of the irresistible power of human perseverance.

In the light of the experience gained by former experimenters, it appeared that the action of a powerful electric current upon the compounds of fluorine with the non-metallic elements, such as hydrogen, phosphorus, and arsenic, would be most likely to yield the desired result; knowing also that fluorine must be an extremely energetic substance, it was absolutely essential to work at very low temperatures. Hence M. Moissan's first attack was made upon the fluorides of phosphorus and arsenic, but finding these to be practically impregnable, he diverted his attack, guided by certain indications afforded during his first attempt, upon hydrofluoric acid itself. Finding, however, that pure hydrofluoric acid is an exceptionally bad conductor of electricity, as has been stated by other workers—that even a current from fifty Bunsen cells would not pass through the liquid—he eventually, after several essays, succeeded in converting it into a conductor by dissolving in it a quantity of the double fluoride of potassium and hydrogen. On passing the current from twenty Bunsen cells through the now conducting medium, hydrogen immediately commenced to be evolved at the negative terminal, while fluorine was with similar rapidity evolved at the positive pole, and exhibited its tremendous activity upon everything that came near it: burning up hard crystalline silicon like tinder, setting fire to organic matter, and forming fluorides with incandescence with many other elements.

Having thus indicated the general course of these researches, it will no doubt be interesting to follow M. Moissan during the carrying out of his principal experiments.

The first series consisted in examining the action of electric induction sparks upon the gaseous fluorides of silicon, phosphorus, and arsenic. The gases were introduced into glass eudiometer tubes standing over mercury, and the spark was passed between two platinum wires connected with an induction-coil actuated by a few Grenet or Bunsen cells. On introducing dry silicon tetrafluoride, SiF_4, and passing sparks for an hour, no decomposition was effected, the result being discouragingly *nil.* Dry phosphorus trifluoride, PF_3, however, behaved quite differently, phosphorus being deposited upon the inner wall of the tube; but the fluorine liberated at once combined with the residual trifluoride to form the more stable pentafluoride, PF_5. Some time ago this pentafluoride of phosphorus was prepared by Prof. Thorpe, who also submitted it to the action of the induction-spark, unfortunately without effecting any decomposition. Precisely the same result has been arrived at by M. Moissan, using a 0·04m. spark; but on obtaining sparks 0·2m. long, a rapid etching of the walls of the glass tube occurred, and the meniscus of mercury entirely lost its brilliancy. After an hour's duration the experiment was concluded, and the apparatus allowed to cool, when it was noticed that the volume had diminished; moreover, the gas was found to have changed its properties, yielding a precipitate of silica in contact with water, while the residual gas consisted of the trifluoride of phosphorus. Hence $PF_5 = PF_3 + F_2$, which latter forms, with the glass, silicon tetrafluoride, and, with the mercury, fluoride of mercury. So here again the experiment was disappointing, and although fluorine was for the moment liberated, this method was certainly not suitable for the preparation of free fluorine.

Fluoride of arsenic, AsF_3, the next fluoride experimented upon, was first prepared by M. Dumas, who was severely injured in the experiment. It is a liquid which boils at 63° C., and may be easily maintained in a gaseous condition, by use of a steam jacket, and submitted to the action of the spark. It is, however, a most disagreeable substance to work with, as it produces most terrible sores when by any mischance it comes in contact with the operator's skin. On passing sparks through it for an hour, as in case of the pentafluoride of phosphorus, the platinum wires became covered with a black incrustation of arsenic, while the walls of the tube were strongly corroded. On testing the gas, it was found to contain a large quantity of silicon tetrafluoride mixed with a smaller quantity of free fluorine, which displaced sufficient iodine from a solution of potassium iodide to give a good coloration to several cubic centimetres of chloroform. Clearly, progress was being slowly made, though still far from the isolation of fluorine.

And now a remarkable experiment of a new type was performed. It had been noticed that, on passing an electric current through a platinum wire in an atmosphere of phosphorus trifluoride, the platinum fused owing to the formation of a fusible phosphide of platinum; at the same time the glass of the containing vessel was etched and the mercury attacked. So the experiment was repeated on a grander scale. A quantity of spongy platinum, previously washed with hydrofluoric acid and calcined, was placed in a platinum tube 80 cm. long, and of 1·5 cm. diameter; that portion of the platinum tube which required to be heated was incased in a second outer tube of glazed porcelain, so that between the two a current of nitrogen could be kept circulating, and so prevent access of furnace gases. The tube was then heated in a furnace, and pure hydrogen passed through it for some time to remove all other gases; afterwards pure nitrogen was substituted, and finally phosphorus trifluoride. After passing a short time, the current of fluoride was suddenly stopped with a most singular result: a partial vacuum was caused, owing to absorption by the platinum.

When, however, the current of trifluoride was passed more rapidly, a small quantity of pentafluoride was formed; the fluorine liberated, when the absorption of phosphorus by the platinum occurred, having combined with the trifluoride just as in the spark experiment. But, on examining the gas which passed out of the tube under these conditions, it was found to liberate iodine from potassium iodide, attack mercury, and etch glass. In fact, it was proved that free fluorine was liberated, and mostly absorbed by the platinum, causing the diminution of pressure on stopping the current, but being more or less carried away when the current was more rapid. The fluophosphide of platinum formed was found to contain only 70 to 80 per cent. of platinum, and the formation of this substance was so rapidly effected that every experiment required a new tube. The action of pentafluoride of phosphorus upon platinum was next tried, and with still more encouraging results. On sweeping the tube, heated in a coke blast-furnace, with a rapid stream of the pentafluoride for some minutes, then moderating the rapidity, and five minutes later again increasing the speed, the issuing gas was found to blacken solid potassium iodide by liberating free iodine, inflame phosphorus, and attack crystalline silicon, glass, and mercury. It was, in fact, free fluorine drowned in excess of trifluoride of phosphorus. This was a decided advance, and the outlook was becoming considerably more hopeful.

The next experiments were made with liquid fluoride of arsenic, AsF_3, a quantity of which was placed in a platinum crucible, which served as the negative electrode. A platinum wire, dipping into the liquid in the crucible, and reaching to within 5 millimetres of the base, served as the positive electrode. The current from three Grenet cells was then passed through the liquid, causing a deposition of arsenic upon the interior surface of the crucible, but no gas could be perceived at the positive pole. However, on dipping the platinum wire into a solution of starch paste and potassium iodide, blue striæ were at once formed in the solution, showing the presence of a condensed gas sheath of fluorine around the platinum wire. Following up this indication, the current from twenty-five Bunsen cells arranged in series was next employed, and immediately the deposition of arsenic commenced upon the walls of the crucible, while bubbles of gas were evolved around the platinum wire. Unfortunately the action soon ceased, owing to the bad conductivity of the liquid and of the thick deposit of arsenic. The wire, however, was strongly attacked. So attempts were next made to increase the conductivity of the fluoride by the addition of metallic fluorides, and it was soon discovered that the best results were obtained by use of the double fluoride of hydrogen and potassium, HF . KF. It was probably this discovery which led to the grand success with which these efforts have been finally crowned, for, as has been previously mentioned, it was by the electrolysis of this double fluoride that M. Moissan eventually succeeded in preparing free fluorine.

Before leaving the experiments upon arsenic fluoride, it may be mentioned that it was eventually electrolyzed in a continuous manner by use of seventy to ninety Bunsen cells, the arsenic liberated remaining in suspension in the liquid, instead of adhering to the tube, but the bubbles were rapidly seen to diminish in size in passing through the liquid, and scarcely a trace of gas escaped; instead of permitting its isolation, the fluorine preferred to form a new fluoride, the pentafluoride of arsenic, thus once more baffling the ingenious experimenter.

But success was not now far away. The wonderful manner in which the double fluoride of potassium and hydrogen increased the conductivity of arsenic fluoride determined M. Moissan in employing it for the same purpose in an attempt to electrolyze pure anhydrous hydrofluoric acid. Faraday long ago showed that the electric current will not pass through the anhydrous acid, and Gore more recently came to the same conclusion. The current from fifty Bunsen cells was found by M. Moissan to be absolutely powerless to penetrate the acid used in these later experiments. But, on dissolving a few fragments of the double fluoride HF . KF in the acid, the current at once passed freely, and the experiment thus became possible. The apparatus used in the first attempts with this mixture consisted of a platinum U-tube, of which each branch was closed by a paraffined cork, through which the rods of platinum forming the poles were passed. Upon each branch, just above the level of the liquid and beneath the cork, was soldered a little platinum delivery-tube to lead off the gases evolved. As hydrofluoric acid boils at 19°·4 C., the apparatus was immersed in a bath of methyl chloride, which boils at −23°, but which could be reduced in temperature to −50° by driving through it a current of dry air. Hence the electrolysis could be conducted without fear of the gaseous products being drowned in excess of vapour of hydrofluoric acid, and the activity of the liberated fluorine was at the same time moderated. On passing the current, a gas was at once produced at each electrode, a regular evolution of hydrogen at the negative pole, and a continuous disengagement of gas at the positive pole. But still affairs were not satisfactory: crystalline silicon did not take fire when held in the gas coming off from the positive pole; so the apparatus was taken to pieces an hour later, in order, if possible, to find a clue to the source of failure. The paraffined cork at the negative branch was intact, but, behold the mischief, the other was carbonized to the depth of a centimetre; so the liberated fluorine had extracted hydrogen out of the cork, and passed on as hydrofluoric acid. The positive platinum rod was also much corroded. Closely-fitting stoppers of fluor-spar were next tried, coated with melted

gutta-percha, but the latter again soon melted on passing the current, and was put *hors de service*. Gum-lac and many other substances were tried, but all to no purpose, and much precious time was lost. Finally, however, the difficulty was overcome by using stoppers of fluor-spar, carefully inserted in hollow cylinders of platinum carrying fine screw-threads upon their outer surfaces, which engaged with corresponding threads upon the interior surfaces of the two branches of the U-tube. The platinum rods passed through the axis of each cylinder of fluor-spar: the rods themselves were of square section, of 2 millimetres side and 12 centimetres long, and passed to 3 millimetres from the base of the U-tube; they were made of irido-platinum, containing 10 per cent. iridium, which is less attackable than pure platinum. The U-tube simply consisted of a platinum tube, bent twice at right angles, 1·5 centimetre diameter and 9·5 centimetres high, and was fitted with side tubes and immersed in methyl chloride as before.

The pure anhydrous hydrofluoric acid, which was the next necessity, was prepared in the following manner. A known volume of commercial acid was treated with sufficient potassium carbonate to neutralize about a quarter of it, and then distilled in a leaden retort over an oil bath at 120°. At this temperature the fluosilicate of potassium, formed from the hydrofluosilicic acid, contained as impurity in the commercial acid, was not decomposed, and the distillate was therefore free from silica. This distillate was then divided into two parts, and one half, saturated with pure potassium carbonate, forming neutral potassium fluoride, was then added to the other half, and transformed into HF . KF. The double fluoride was then dried at 100°, and afterwards kept for some days in the vacuous receiver of an air-pump, containing also strong sulphuric acid and a few sticks of fused potash. When absolutely dry it fell to powder, and was then ready for the preparation of hydrofluoric acid, which was always freshly prepared immediately before each experiment. The dry fluoride was in each case introduced into a recently ignited platinum retort, and maintained at a moderate heat for some time so as to commence the decomposition slowly; the first portions of distillate were rejected, as they would contain the last traces of water. The platinum receiver was then adapted and surrounded by ice and salt; on heating the retort more strongly, pure hydrofluoric acid condensed in the receiver as a limpid liquid boiling at 19°·4, very hygroscopic and fuming in the air.

While the preparation of the acid was in progress, the U-tube and electrodes were drying at 120°. From 6 to 7 grammes of the dry double fluoride were now introduced into the apparatus, the stoppers were screwed in and covered with gum lac. The whole was then fixed in the methyl chloride bath, and, until the introduction of the acid, the delivery-tubes were connected with desiccators containing fused potash. A constant supply of methyl chloride at − 23° was maintained in the outer cylinder, as a slight rise of temperature allowed of the volatilization of some of the acid. About 15 to 16 grammes of the anhydrous hydrofluoric acid were then gently aspirated into the apparatus, and the current from twenty Bunsen cells allowed to pass, when immediately a regular evolution of gas occurred at each pole. At the negative pole pure hydrogen was evolved, which burnt with its characteristic flame, forming water. At the positive pole was liberated a colourless gas of penetrating and very disagreeable odour, somewhat resembling that of hypochlorous acid, and rapidly irritating the mucous membranes of the throat and eyes. It was no other than pure fluorine itself. All the trouble, all the expense, and all the disappointments were repaid. It must indeed have been a supreme moment for M. Moissan.

In order to study its action upon solids, they were placed in small glass tubes, and brought near to the orifice of the platinum delivery-tube at the positive side. The test was generally repeated, holding the solids in small platinum capsules.

Sulphur, brought thus near the orifice, at once melted and inflamed; selenium behaved in like manner; as did also tellurium, with incandescence, forming fumes and becoming coated with a solid fluoride.

Phosphorus at once took fire, forming tri-, penta-, and oxyfluorides. Powdered arsenic and antimony combined with incandescence, the former yielding drops of AsF_3.

A fragment of iodine placed in the gas combined with production of a pale blue flame; in an atmosphere of iodine vapour fluorine itself burnt with a similar flame. Vapour of bromine lost its colour and the combination was sometimes accompanied by detonation.

Cold crystalline silicon at once became incandescent, and burnt with great brilliancy, sometimes with scintillations. On closing the little tubes containing it with the thumb and opening under water, the silicon tetrafluoride formed was absorbed and decomposed with precipitation of silica. Any undecomposed silicon was found to have been fused.

Debray's adamantine boron also burnt in the gas, becoming incandescent and giving off fumes.

Fluorine has a most extreme affinity for hydrogen; they combine in the dark with explosion. In one of the experiments the electrolysis was allowed to continue several hours, so that eventually the small quantity of undecomposed acid remaining in the U-tube was insufficient to keep the two gases apart; the experimenters were consequently suddenly startled by a violent detonation. The hydrogen and fluorine had combined in the dark at the low temperature of − 23°. The same detonation was afterwards brought about on a smaller scale by reversing the current. On bringing the wide-mouthed delivery-tube of a hydrogen generator near the orifice, the detonation at once occurred, and the hydrogen inflamed.

Metals are all attacked with more or less energy by fluorine, forming fluorides. Cold sodium and potassium were at once rendered incandescent. Calcium, magnesium, and aluminium acted similarly, in a more modified manner, becoming incandescent when slightly warmed. Powdered iron and manganese, on gently warming, burnt with bright scintillations; lead was attacked in the cold, and tin at a slightly elevated temperature. Mercury, as suspected, entirely absorbed the gas, forming yellow protofluoride. Silver at a gentle heat became coated with a beautiful satin-like fluoride, soluble, unlike the chloride, in water. Gold and platinum at 300°-400° became coated with their respective fluorides, which were decomposed again at a red heat, with evolution of free fluorine.

Perhaps the strongest evidence of the intense chemical activity of fluorine is exhibited in its action upon cold potassium chloride: the chlorine was at once expelled, filling the air with its disagreeable odour, and was identified by the usual chemical tests. Chlorine was also expelled from its combination with carbon in carbon tetrachloride.

All organic compounds are violently attacked by fluorine: a piece of cork at once carbonized and inflamed; alcohol, ether, benzene, and turpentine took fire immediately in contact with it.

Glass, as might have been expected, is at once corroded by fluorine; some very delicate experiments were carried out with perfectly dried glass, with the same result.

Many other reactions, all interesting and all showing the immense energy with which the atoms of fluorine are endowed, were performed, but one especially ought to be noticed, viz. the action of fluorine upon water. It is a singular fact that, whenever oxygen is liberated in the cold, there is a great tendency to form ozone: hence when fluorine is attempted to be collected over water, the gas collected is not fluorine, but ozonized oxygen; water is decomposed by the fluorine forming hydrofluoric acid,

while the oxygen is set free, and a considerable quantity of it is converted into the more condensed form of ozone.

On taking the apparatus to pieces after each experiment, the hydrofluoric acid remaining was found to contain a small quantity of platinum fluoride in solution, and a black mud consisting of a mixture of iridium and platinum in suspension. The negative electrode was not attacked, but the platinum rod forming the positive pole was eaten away to a point, so that one rod only served for two experiments. The average delivery of gas was about 1·5 to 2 litres per hour.

With regard to the chemical processes involved in the electrolysis, it appears probable that potassium fluoride is first decomposed into fluorine, which is evolved at the positive pole, and potassium, which decomposes hydrofluoric acid, liberating its equivalent of hydrogen at the negative pole, and re-forming potassium fluoride, which may again be electrolyzed. Hence a small quantity of the double fluoride can serve for the decomposition of a comparatively large amount of hydrofluoric acid.

The double fluoride HF . KF is very soluble in hydrofluoric acid, forming a crystallizable compound, richer in hydrofluoric acid than HF . KF, and which gives off no acid vapour at the boiling-point of the anhydrous acid, 19°·4. It is this compound which one ought to seek to obtain for electrolysis, as it is very soluble in excess of acid, forming a liquid of good conductivity.

The double fluoride HF . KF itself was finally electrolyzed by M. Moissan. It fuses at 140° to a colourless liquid which is quite suitable for electrolysis. The experiment was performed, as before, in a platinum U-tube, and, on passing the current, fluorine was again liberated at the positive pole, and at once set fire to crystalline silicon; but the platinum was strongly attacked, so the experiment was stopped in order to save the tube. On plunging a couple of platinum wires connected with the battery into a quantity of the fused double fluoride contained in a platinum crucible, gas was evolved in abundance at each pole, and on bringing the wires in contact, even in the dark, detonation occurred, owing to the combination of the evolved hydrogen and fluorine. At the same time the platinum wire from which the fluorine was evolved was almost entirely eaten away.

In concluding these remarkable researches, which have happily terminated so successfully, M. Moissan discusses very fully the question of the identity of the gas liberated at the positive pole with the element fluorine; and there can be no doubt that he has completely proved this identity, at the same time showing that fluorine occupies the place of honour as the most intensely active chemical element with which we are at present acquainted, and that it assumes its rightful position, theoretically destined for it, at the head of the group of halogens.

A. E. TUTTON.

TIMBER, AND SOME OF ITS DISEASES.

I.

ON carefully examining the clean-cut end of a sawn log of timber, it is easy to convince ourselves of the existence of certain marks upon it, which have reference to its structure. These marks will vary in intensity and number according to the kind of tree, the age at which it is felled, and some other circumstances, which may be overlooked for the present; but in a given case it would be possible to observe some such marks as those indicated in Fig. 1. In the specimen chosen there is a nearly central spot, the pith, around which numerous concentric lines—the "annual rings"—run. Radiating from the pith towards the periphery are cracks, the number, and length, and breadth of which may vary according to the time the log has been exposed to the weather, and other circumstances; these cracks are due to the contraction of the wood as it "shrinks," and they coincide with medullary rays, as lines of weakness. Between these cracks are to be seen numerous very fine radiating lines indicating the course of the uninjured medullary rays, which again will vary in distinctness, &c., according to the species of timber.

FIG. 1.—A log of timber, showing radial cracks after lying exposed for some time. *a*, a large crack extending from pith to circumference; *b*, the cortex; *c*, medullary ray; *d*, cambium; *e*, annual ring; *f*, outer bark, proper. Reduced.

This log of wood, with its annual rings and medullary rays, is clothed by a sort of jacket, consisting of cork and softer tissues, and termed the cortex, or, more popularly, the "bark" (an unfortunate word, which has caused much trouble in its time). The largest of the cracks is seen to traverse the whole radius of the face of the wood from centre to circumference, and also to pass through the cortex, which gapes widely.

The remaining cracks, however, stop short at a line which marks on the one hand the inner face of the cortex, and on the other the outer face of the wood: this line also represents the cambium, a thin sheet of generative tissue which remains after giving rise to practically the whole of the wood (a very little in the centre excepted) and cortex visible in the woodcut. Since we are not concerned with the cortex and bark at present, it will be convenient to regard the log as "barked," and only deal with the wood or timber itself, in the condition to which the woodman reduces it after removing the cortex with certain implements.

If now we split such a log as Fig. 1 along the line of the big crack, neatly and smoothly, the well-known "grain" so often observed on planks of wood will come into view, and it will be noticed that the lines which mark the "grain" are continuations of the lines which mark

the annual rings, as shown in Fig. 2, which represents on a larger scale a segment such as could be cut from a log in the way described. It is clear from comparison of what has been said, and of the two figures, that the "annual rings" are simply the expression in cross-section of cylindrical sheets laid concentrically one over the other, the outermost one being that last formed. But on examining the medullary rays in such a piece of timber as that in Fig. 2, it will be noticed that they also are the expression of narrow radial vertical plates which run through the concentric sheets: the medullary rays are in fact arranged somewhat like the spokes of a paddle-wheel of an old steamer, only they differ in length, breadth, and depth, as seen by comparing the three faces of the figure. It is to be noticed that the medullary rays consist of a different kind of tissue from that which they traverse, a fact which can only be indicated in the figure by the depth of shading. It is also to be observed that the "annual rings" show differences in respect to their tissue, as marked by the darker shading near the boundary lines on the outer margin of each ring. In order to understand these points better, it is necessary to look at a piece of our block of timber somewhat more closely, and with the aid of some magnifying power. For the sake of simplicity it will be convenient to select first a piece of one of the timbers known as "deal" (firs, pines, &c.), and to observe it in the same direction as we commenced with, *i.e.* to examine a so-called transverse section.

The microscope will show us a figure like that in the woodcut (Fig. 3). There are to be seen certain angular openings, which are the sections of the long elements technically called *tracheides*, shown in elevation in Fig. 4. It will be noticed that whereas along some parts of the section these openings are large, and as broad in one direction as in the other, in other parts of the section the openings are much smaller, and considerably elongated in one direction as compared with the other. The band of small openings naturally looks more crowded and therefore darker than the band of larger openings, and it is to this that the differences in the shading of the annual rings in Fig. 2 are due. But it is not simply in having larger lumina or openings that the dark band of tracheides is distinguished from the lighter one: the walls of the tracheides are often also relatively thicker, and obviously a cubic millimetre of such wood will be denser and contain more solid substance than a cubic millimetre of wood consisting only of the larger, thin-walled tracheides. It is equally obvious that a large block of wood in which the proportion of these thick-walled tracheides with small lumina is greater (with reference to the bands of thin-walled tracheides) will be closer-grained, and heavier, than an equal volume of the wood where the thin-walled tracheides with large lumina predominate.

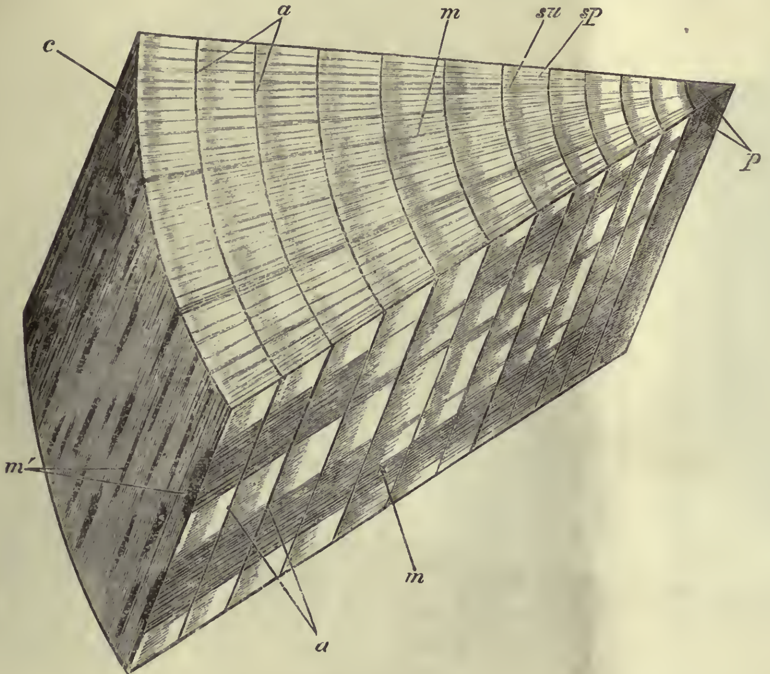


FIG. 2.

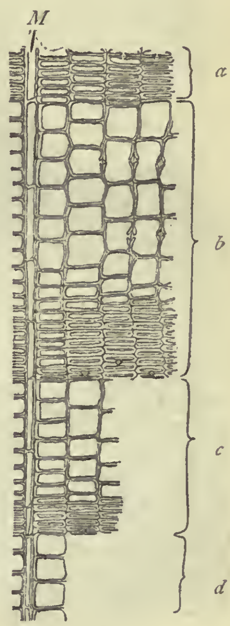


FIG. 3.

FIG. 2.—Portion of segment of wood from a log such as Fig. 1, supposed to be slightly magnified. *a*, annual ring; *m*, medullary rays; *m′*, the same in vertical section; *c*, the boundary line between one annual ring and another; *su*, autumn wood; *sp*, spring wood; *p*, the pith.

FIG. 3.—Portions of four annual rings from a thin transverse section of the wood of a Conifer, such as the Spruce-fir. *M*, a medullary ray; *b* and *c* show the entire breadth of two annual rings; *a*, autumn wood of an annual ring internal to *b* (and therefore older than *b*); *d*, spring wood of an annual ring external to *c* (and therefore younger than *c*). Bordered pits are seen in section on some of the tracheides. Magnified about 100 times.

Returning now to the section (Fig. 3), it is to be observed that the differences in the zones just referred to enable us to distinguish the so-called "annual rings." The generally accepted explanation of this is somewhat as follows. In the spring-time and early summer, the cambium-cells begin to divide, and those on the inner side of the cylinder of cambium gradually become converted into tracheides (excepting at a few points where the cells add to the medullary rays), and this change occurs at a time when there is (1) very little pressure exerted on the inner parts of the trunk by the cortex and corky bark, and (2) only comparatively feeble supplies are derived from the activity of the leaves and roots, in the still cool weather and short days with little sunlight. In the late summer, however, when the thickened mass of wood is compressed by the tightened jacket of elastic bark which it has distended, and the long, hot, bright sunny days are causing the numerous leaves and roots to

supply abundance of nutriment to the growing cambium-cells, it is not surprising that these cells cannot extend themselves so far in the radial direction (*i.e.* in a line towards the centre of the compressed stem), and that their walls are thickened by richer deposits of woody material supplied quickly to them.

As the winter approaches, the cambium ceases to be active, and it then remains dormant for several months. When its cells are awakened to renewed growth and division in the following spring, they at once begin to form the tracheides with thin walls and large lumina, and it is the sharp contrast thus displayed between the newly-formed tracheides with thin walls and large lumina, and the compressed denser ones on which they suddenly abut, that produces the impression of the "annual ring."

It is now time to attempt to give some clearer ideas of what this "cambium" is, and how its cells become developed into tracheides. But first it is necessary to point out that each tracheide is a long, more or less tubular and prismatic body, with bluntly tapering ends, and the walls of which have certain peculiar markings and depressions on them, as seen in Fig. 4. We cannot here go into the important signification and functions of these markings and depressions however, since their study would need an article to themselves. It must suffice for the present to state that the markings have reference to the minute structure of the cell-walls, and the depressions are very beautiful and complicated pieces of apparatus to facilitate and direct the passage of water from the cavity of one tracheide to that of another. Now, the cambium is a thin cylindrical sheet of cells with very delicate walls, each cell having the form of a rectangular prism with its ends sharpened off like the cutting edge of a carpenter's chisel: this prism is broader in the direction coinciding with the plane of the sheet of cambium—*i.e.* in the tangential direction, with reference to the trunk of the tree—than in the

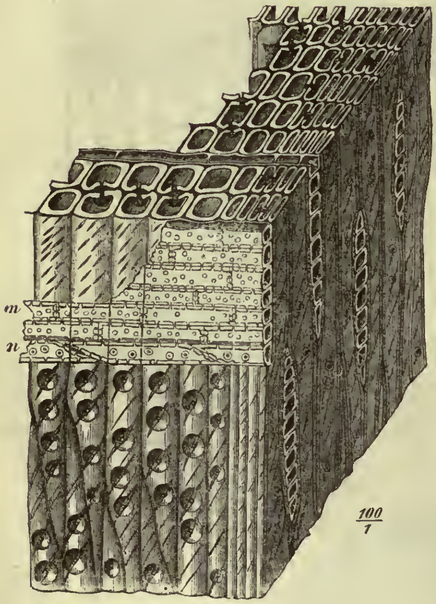


Fig. 4.

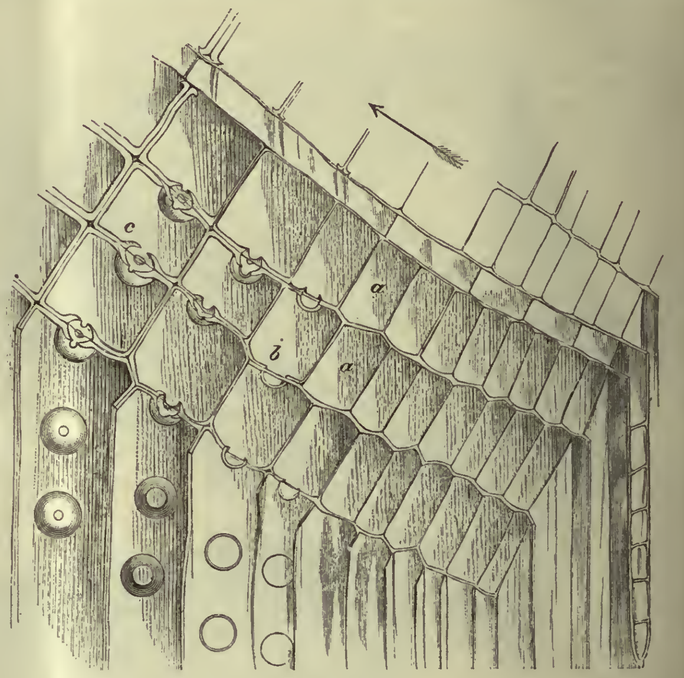


Fig. 5.

Fig. 4.—A small block of wood from a spruce-fir, supposed to be magnified about 100 times, showing elevation and sectional views of the tracheides of the autumn (to the right) and spring wood, and medullary rays (*m n*) running radially between the tracheides. (After Hartig.)

Fig. 5.—Portion of cambium of a fir, showing the development of the young wood tracheides from the cambium-cells. The arrow points to centre of the stem. The cambium-cells at length cease to divide, and the walls become thicker (*a*), except at certain areas, where the bordered pits are developed (*b* and *c*). To the right is a medullary ray. Highly magnified, and the contents of the cambium-cells omitted for clearness.

direction of the radius of the stem; and the chisel-edge must be supposed to run in the direction parallel to that of a medullary ray, *i.e.* radially. From the first, each cambial cell contains protoplasm and a nucleus, and is capable of being nourished and of growing and dividing. It is only at or near the tips of the branches, &c., that these cambium-cells are growing much in length, however; and in the parts we are considering they may be for the most part regarded as growing only in the radial direction; more rarely, and to a slight extent, in the tangential direction also, as the circumference of the cylinder enlarges. After a cambial cell has extended its walls by growth in the radial direction to a certain amount, a septum or division wall arises in the longitudinal tangential plane, and two cells are thus formed in place of one: this process of division may then be repeated in each cell, and so the process goes on. This is not the place to lay stress on certain facts which show that a single layer of cells initiates the division: it suffices to point out that by the above process of division of the cambial cells there are formed radial rows of cells, as indicated in Fig. 5, where the arrow points along a radius towards the centre of the stem. It is true such radial rows of cells are also developed in smaller numbers towards the outside of the cambium cylinder (*i.e.* to add to the cortex), but we are only concerned with the wood, and therefore only regard those cells which are developed on the inside (*i.e.* towards the centre of the stem). After a time the oldest of these cells (*i.e.* those nearest the centre of the stem) cease to divide, and undergo changes of another kind: the process of division is still going on in the younger ones, however; and so the radial rows are being extended by additions of cells at their outer ends. Of course, this is normally proceeding along the whole area of the cylindrical sheet of cambium, and therefore over the whole of the stem and roots, with their branches.

Confining our attention to one of the innermost, oldest cells of the cambium, which has ceased dividing (*aa* in Fig. 5), we find that it enlarges somewhat in the radial direction, and then its hitherto very thin walls become thicker; in fact, the protoplasm in its interior absorbs food-materials, and changes them into a peculiar substance which it plasters or builds on to the inner sides of the cell-wall, so to speak, until the wall is much thicker. This thickening process is withheld at certain places only—the thin depressions already referred to. Two chief changes result now: (1) the whole of the living contents of the young wood-cell gradually become used up, and eventually disappear without leaving any trace; and (2) the thickening substance built on to the inside of the walls undergoes changes which convert it into true wood-substance—in botanical language, the walls become lignified. The cells *b* and *c* in Fig. 5 illustrate what is meant.

During all these changes, which occupy several or even many hours or days, according to circumstances, it will be observed that the definitive shape of the cell is gradually completed, and then alters very little: the prismatic cambium-cell has become a prismatic tracheide, with thicker, lignified walls, and containing air and water (with minute quantities of mineral substances dissolved in it) in place of protoplasm and nutritive substances. It is not necessary here to speak of other and more subtle changes which cause slight displacements, &c., of these cells.

If I have succeeded in making the chief points in this somewhat complicated process clear, there will be little difficulty in explaining what occurs in other parts of the cambium-cylinder. The cambium-cells which happen to stand in the same radial row as the cells of a medullary ray, simply go on being converted into cells of the medullary ray, instead of into tracheides; cells which differ from the tracheides chiefly in retaining their living contents and nutritive materials—*i.e.* substances like starch, proteids, sugars, &c., which are used as food by the plant. Again, those cells of the cambium which are divided off on the outer side of the cylinder (they are always fewer in number) are gradually transformed into elements of the cortex, and finally enter into the composition of the bark proper.

Now and again, but much more rarely, a radial row of cambial cells which, from their position, it would appear should be converted into tracheides of the wood, alter their destiny, so to speak, and become the originators of a new medullary ray. But I must pass over these and some other minor peculiarities, and refer to the illustrations for further details.

If now, instead of a log of deal, or coniferous wood, we direct attention to the timber of a dicotyledonous tree, such as the oak, ash, beech, chestnut, poplar, &c., the differences in detail will not be found very great in relation to the broad features here under consideration. Turning again to Fig. 1, it would be possible to select a cut log of any of these timbers which presented all the salient characters there exhibited. The bark would present external differences in detail—such as in roughness, colour, thickness, &c.—but it could still be described, as before, as a more or less corky jacket around the whole of the wood: the cut face would show the timber marked by more or less numerous and prominent "annual rings," traversed by smaller or larger medullary rays, radiating from the central pith, and passing across the cambium to the cortex. Moreover, cracks would be apt to form on exposure, as before; the opening occurring along the lines of medullary rays—lines of weakness.

Again, if we cut a segment of the wood, like Fig. 2, the chief features would present themselves as there shown, and the lines of demarcation indicating the annual rings would be found to be due to the sharp contrast between the spring wood and the autumn or summer wood, as before.

On closely examining a transverse section of such a piece of timber, however, we should find differences which at first sight appear profound, but which on reflection and comparison turn out to be of more relative significance, from the present point of view, than might be expected.

Selecting a given example, that of the beech for instance, the first difference which strikes us (Fig. 6) is a number of relatively very large openings on the transverse section: these are the vessels—pitted vessels—long tubular structures which are not formed by the cambium of the conifers. Between these vessels are much more numerous elements with very small lumina and thick walls: the latter are the wood-fibres proper, and have to be technically distinguished from the apparently somewhat similar wood-tracheides of the pines, firs, &c. Here and there, scattered in small groups, are certain rows of shorter cells, which, however, are not very numerous in the beech: they are called wood-parenchyma (Fig. 6, *wp.*), and occur particularly in the vicinity of the vessels.

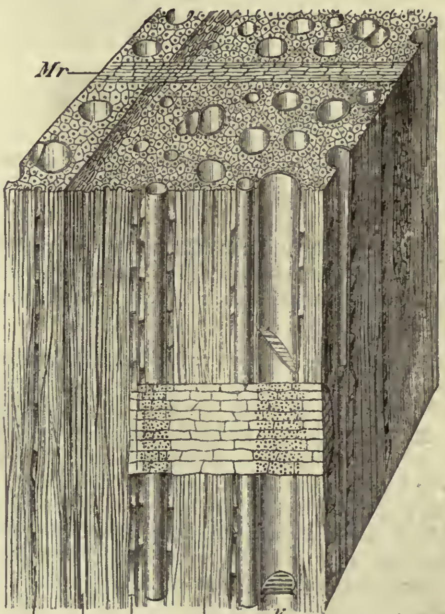


Fig. 6.—A piece of wood from a dicotyledonous tree (beech), supposed to be magnified about 100 times. *Mr*, a medullary ray running across the transverse section: the dark band crossed by this ray is the autumn wood (*a*), formed of closely-crowded wood-fibres and tracheides; *v*, a large vessel in section: others are seen also—they are smaller and fewer towards the autumn wood; *a′*, wood-fibres, of which most of the timber is composed; *wp*, wood-parenchyma cells.

It is beside the purpose here to describe in detail the histology of the beech-wood, and reference may be made to the figures for further particulars. It may suffice to say that all the elements—cells, fibres, and vessels—are formed as before by the gradual development of cambium, cells; and the same is true, generally, of the medullary rays here that is true of those of the pines and firs, &c.

Attention is to be directed to the fact, which is here again evident, that the line of demarcation between any two "annual rings" is due to the sudden apposition of non-compressed elements upon closely-packed and apparently compressed elements: the latter were formed in the late summer, the former in the spring. Moreover, the spring wood usually contains more numerous vessels, with larger lumina than the autumn wood: in this particular case, again, the fibres of the autumn wood are darker in colour. It should be stated, however, that many dicotyledonous trees show these peculiarities much more clearly than the beech; others, again, show them less clearly.

Now it is obvious that, other things being equal, the spring wood, with its more numerous and larger vessels, and its looser tissue generally, will yield more readily to lateral pressure and strains than the denser autumn wood; and the like is true of the pines and firs—the closely-packed, thick-walled tracheides of the autumn wood furnish a firmer and more resistant material than the larger, thinner-walled tracheides of the spring wood. To this point we shall have to return presently.

H. MARSHALL WARD.

(*To be continued.*)

NOTES.

WE deeply regret to announce the death of Prof. Balfour Stewart, one of our most eminent men of science. Last Friday morning he left the Owens College, apparently in his usual health and in good spirits, intending to spend the holidays at his Irish home. He died on Sunday night. Next week we shall have something to say about his character and work.

THE death of Carl Langer, the well-known Professor of Anatomy at the University of Vienna, is announced. He was in his sixty-eighth year.

DR. ARTHUR FARRE, F.R.S., died on the 17th inst., in his seventy-seventh year. He was elected a Fellow of the Royal Society in 1839.

THE Royal Society has been admitted to the number of those public bodies to which is conceded by prescription or otherwise the privilege of presenting their addresses to the Sovereign on the throne.

THE Curatorship of the Natural History Department of the Science and Art Museum, Dublin, rendered vacant by the resignation of Mr. A. G. More, has just been filled by the promotion of Dr. R. F. Scharff, who had been one of the assistants in the Museum for some months. Dr. Scharff has already proved himself to be a diligent student of zoology in Edinburgh, where he took the degree of Bachelor of Science. In London he studied under Prof. Ray Lankester, and worked in the British Museum for some time under the Director, Prof. Flower, F.R.S., and he obtained the degree of Doctor of Philosophy at Heidelberg University.

MR. JOHN M. THOMSON has been appointed to the Chair of Chemistry in King's College, vacant through the death of Prof. Bloxam.

AT the Central Institution, Exhibition Road, South Kensington, Dr. A. K. Miller, Demonstrator and Assistant in the Chemical Research Laboratory, will deliver, during the spring term, a course of ten lectures on the chemistry of oils and fats. The course will be delivered on Mondays at 4 p.m., and will begin on January 23, 1888.

THE third annual meeting of the American Association for the Advancement of Physical Education was held at Brooklyn on November 25. It was well attended. Papers were read by Prof. Edward Hitchcock, of Amherst College, who presided; by Prof. E. H. Fallows, of the Adelphi Academy; and by Prof. J. W. Seaver, of Yale College.

Science (December 9, 1887) notes, as a fact which may be of interest to Americans, that in England the point of view of those who argue in favour of technical education is almost exclusively the economic. "But little is heard," it says, "of the educational nature of manual training." Speaking of the state of things in the United States, *Science* says:—"There is now, as is well known, a very general movement throughout this country in favour of what is known as manual training in education. After much misapprehension and tedious explanation, the leaders of this movement have finally managed to make the educational public understand that they advocate manual training mainly for its educational value, and only incidentally for the economic benefits which will undoubtedly flow from it."

THE twentieth annual meeting of the Kansas Academy of Science was held in the Capitol Building, Topeka, on October 26, 27, and 28. *Science* says that there was an excellent attendance of members, but that the local attendance was not quite equal to that of last year. The papers read, according to *Science*, were unusually valuable. The annual meeting next year will be held in Wichita, in October.

THE tenth general meeting of the German Society of Analytical Chemists was held at Frankfurt, on November 30. Dr. Schmitt, of Wiesbaden, was President.

THE tenth meeting of the German Geographical Society will be held at Berlin next Easter. In future the meetings will be held only once in two years.

FIFTY shocks of earthquake are reported to have occurred at Silveric, in Dalmatia, on November 29. On the same day, at 7.30 a.m., severe shocks occurred at Oran, Mascara, and Relizante, in Algeria.

ON the evening of November 21, from 8.30 p.m. to about 9, a remarkable luminous phenomenon, viz. a broad band of light right across the sky, was seen throughout the whole of central and southern Sweden. It caused much speculation, chiefly on account of its luminous immobility. Dr. N. Ekholm, of the Upsala Meteorological Observatory, and well known for his researches on the aurora borealis at Spitbergen, has now pronounced the phenomenon to be a so-called auroral band. Dr. Ekholm states that such bands are very uncommon in Sweden, but that they are often seen at Spitzbergen. He saw the phenomenon during a journey from Stockholm to Upsala, at 8.45 p.m., and noted its position. The band ran then just north of the northernmost stars in Orion, through Aldebaran, then a little south of the Pleiades, further through the Ram, and then a little north of the two southernmost stars in the square of Pegasus. He calculates its height above the earth at about 80 miles, its zenith being perpendicular above the two provinces of East and West Gothia. The bands moved from north to south at the rate of about 50 metres per second. In Upsala it seemed south of the zenith. Dr. V. C. Gyllenskiöld made similar observations at Upsala. Dr. Ekholm invites all who may have observed the phenomenon to communicate their observations to him in the interests of science.

ON the afternoon of November 26, at 4.30, a splendid meteor was seen at Laurvik in the Christiania fjord. It went from east to west, and apparently low in the horizon. In spite of the moonlight its tail was visible for some seconds afterwards.

ONE morning last week, the Teusfjord, a little to the north of Bergen, on the west coast of Norway, was covered with ice three-quarters of an inch thick, as far as the eye could reach. Ice, in consequence of the influence of the warmth of the Gulf Stream, has hitherto been unheard of on the west coast of Norway.

Symons's Monthly Meteorological Magazine for December contains an investigation of what was reported in the newspapers to have been an earthquake-shock in Central England on November 20 last. At the more western stations the reporters spoke chiefly of noise, and at the eastern ones of earth tremor. From evidence collected, it appears that the disturbance, as Mr. H. G. Fordham pointed out in NATURE last week (p. 151), was caused by the explosion of a large meteor. Further particulars

are requested, especially as to the locality where the meteor burst, which seems likely to have been between Thame and Abingdon.

THE Pilot Chart of the North Atlantic Ocean for December reports the occurrence of two interesting phenomena. (1) The formation of a very large waterspout on October 6 in latitude 39½° N., longitude 69° W., during a thunder squall. The lower end of the spout did not reach the surface of the ocean. Water could be seen rushing down through the centre of the funnel and ploughing up the surface of the sea to a height of about 50 feet. (2) One of the rare and inexplicable cases of globular lightning. On November 12, at midnight, near Cape Race, a large ball of fire seemed to rise out of the sea to a height of about 50 feet, coming against the wind close up to the ship, and then running away to the south-east, lasting altogether about five minutes.

THE Meteorological Report published for the year 1886 by the Surveyor-General of Ceylon shows that rainfall observations are now taken at eighty-three stations. General observations are made at sixteen stations. The Report contains a map showing the mean annual rainfall of the island, and a diagram of the mean monthly fall at the principal stations. An important discussion of the Ceylon rainfall observations will be found in the Quarterly Journal of the Royal Meteorological Society for October last.

THE Russian Government does good service to meteorology by publishing observations taken for several hours daily on some selected cruises of its men-of-war. A volume has just been issued containing the observations of three such voyages, being Nos. 52-54 of the series. The information is rendered more available for ready use by the weather observations being expressed in the international symbols, and by the data being printed on one side only, to allow of being cut up and pasted in districts as required.

IT is reported from India that, in connection with a plan for improving the system of storm-warnings, new meteorological stations are to be opened, on the Coromandel coast, at Bimlipatam, Nellore, and Cuddalore, and one on the Burmah coast, probably at Tavoy. Mr. Elliott, Superintendent of the Bengal Meteorological Department, was to leave on an inspection tour to visit the coast stations, and to select sites for the new observatories.

THE *Observatory* will in future be edited by Mr. Turner, of Greenwich Observatory, and Mr. Common, of Ealing.

A FLORA of Hertfordshire by the late Alfred R. Pryor, edited by Mr. B. Daydon Jackson, with notes on the geology, climate, and rivers of the county, by Mr. John Hopkinson, will be published in a few days by Messrs. Gurney and Jackson, Mr. Van Voorst's successors. The book will consist of about 600 pages with a map.

THE twenty-third annual volume of the *Zoological Record* will be issued shortly. This valuable book of reference, which was established by Mr. Van Voorst, under the editorship of Dr. Günther, has been for some years supported by an Association. It is now taken over by the Zoological Society. Messrs. Gurney and Jackson will continue to publish the volumes.

MESSRS. GEORGE PHILIP AND SON have in the press, and will shortly publish, "Emin Pasha in Central Africa: Letters and Journals," collected and annotated by Dr. G. Schweinfurth, Dr. Ratzel, Dr. G. Hartlaub, and Dr. Felkin. The work has been translated from the German by Mrs. Felkin. It is illustrated with a portrait, and with two maps specially compiled by E. T. Ravenstein.

A SIXTH edition of Prof. Alleyne Nicholson's "Introductory Text-book of Zoology" (Blackwood) has just been issued. The book is intended for the use of junior students. It has been thoroughly revised, and the author explains that the general arrangement of certain of the larger groups of animals has been altered in accordance with the views now most generally accepted by naturalists. Some of the illustrations have been changed, and a few new engravings have been added.

WE have received from Mr. F. Enock some "Autocopyist" pen-and-ink sketches of bodies and parts of insects, together with examples of the prepared mounts of the objects delineated. The latter call for no special comment. The drawings, however, are exceedingly clear and well printed, scrupulously accurate, and highly commendable. The admiration of the beautiful in Nature must precede the study of the more useful; and, this being so, we can heartily recommend these drawings to the legion of microscopical *dilettanti*. Mr. Enock is practical in his work in that he introduces the Hessian fly, together with a sketch of the infected barley. By way of giving the brief notes which accompany the sketches an authoritative air, he introduces occasional bibliographical references. As pertaining to the aforenamed pest, an important paper by Prof. Fream, read before the British Association this autumn, and duly reported in these pages, may be recommended to Mr. Enock's notice.

M. VAYSSIÈRE, of Marseilles, has begun what promises to be an important publication—an atlas of the anatomy of invertebrates. The first quarter of the book has already been issued.

MESSRS. MACMILLAN AND CO will publish early in January a revised and extended edition of the well-known "Practical Biology" of Prof. Huxley and Dr. H. N. Martin. The work of revision has been carried out by Messrs. G. B. Howes and D. H. Scott, of the Normal School of Science. Besides other improvements, including the addition of the Earthworm and the Snail in the series of animal, and of *Spirogyra* in the series of vegetable, types, the order of the subjects is completely changed. Whereas in the original edition the lowest forms of life were first dealt with, and then the rest in ascending scale, the course is now reversed, beginning with the Frog and proceeding thence to the less familiar regions of invertebrate organizations until the borderland between animals and plants is reached, and a natural ascent can be made to the most complicated vegetable organisms. Prof. Huxley explains in the preface to the new edition that after two or three years' trial of the road from the simple to the complex he became thoroughly convinced that the way from the known to the unknown was easier for students.

AN exhibition embracing every branch of science or manufacture connected with the art of photography will be opened at the Crystal Palace in February next. Valuable exhibits have already been promised, and there is every reason to believe that the collection of pictures and apparatus will be larger than at any previous exhibition, while the classification will be far more complete. Medals and certificates will be awarded for competitive photographic lantern slide entertainments.

WE are informed that the Committee appointed by the Pari Academy of Medicine to investigate the influence of fluorhydric acid on tuberculosis has reported very favourably on the subject. It seems that the Bacilli of tuberculosis are speedily destroyed by minimal proportions of fluorhydric vapours. This fact is an important one for the therapeutics of that very common and fatal disease, tuberculosis.

THE additions to the Zoological Society's Gardens during the past week include a Common Wolf (*Canis lupus*), European, presented by Mr. C. S. Hardy; a Spotted Crake (*Porzana maruetta*), British, presented by Mr. F. W. Proger; two Golden Plovers (*Charadrius pluvialis*), British, purchased.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 DECEMBER 25-31.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on December 25

Sun rises, 8h. 7m.; souths, 12h. 0m. 14·9s.; sets, 15h. 53m.: right asc. on meridian, 18h. 15·0m.; decl. 23° 24′ S. Sidereal Time at Sunset, 22h. 8m.

Moon (Full on December 30, 8h.) rises, 13h. 32m.; souths, 20h. 31m.; sets, 3h. 41m.*: right asc. on meridian, 2h. 46·7m.; decl. 10° 40′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	7 5	11 0	14 55	17 15·1	23 8 S.
Venus.....	4 3	8 51	13 39	15 5·2	14 32 S.
Mars......	0 30	6 24	12 18	12 38·2	1 54 S.
Jupiter....	4 57	9 22	13 47	15 36·5	18 28 S.
Saturn....	18 29*	2 18	10 7	8 31·1	19 23 N.
Uranus...	1 16	6 49	12 22	13 3·3	6 2 S.
Neptune..	13 47	21 27	5 7*	3 43·6	17 59 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Dec.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. °	°
27	75 Tauri	6	18 26	19 35	55	274
27	B.A.C. 1391	5	20 8	near approach	352	—
29	119 Tauri	5½	2 3	near approach	41	—
29	68 Orionis	6	17 58	near approach	148	—
31	B.A.C. 2683	6	18 37	near approach	141	—

Dec.	h.	
26	8	Mars at greatest distance from the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Dec. 26,	23 24	*m*
			,, 31,	23 3	*m*
Algol	3 0·8	40 31 N.	,, 29,	1 17	*m*
			,, 31,	22 6	*m*
λ Tauri	3 54·4	12 10 N.	,, 26,	20 39	*m*
			,, 30,	19 31	*m*
S Tauri	4 23·0	9 42 N.	,, 31,		*M*
ζ Geminorum	6 57·4	20 44 N.	,, 30,	2 0	*m*
R Canis Majoris	7 14·3	16 11 S.	,, 25,	21 54	*m*
			,, 27,	1 10	*m*
U Canis Minoris	7 35·2	8 39 N.	,, 28,		*M*
W Virginis	13 20·2	2 48 S.	,, 30,	21 0	*M*
R Hydræ	13 23·6	22 42 S.	,, 29,		*M*
V Boötis	14 25·2	39 23 N.	,, 31,		*m*
U Coronæ	15 13·6	32 4 N.	,, 25,	5 20	*m*
R Draconis	16 32·4	67 0 N.	,, 29,		*M*
β Lyræ	18 45·9	33 14 N.	,, 27,	18 0	*M*
U Cygni	20 16·1	47 32 N.	,, 29,		*M*
V Cygni	20 37·6	47 44 N.	,, 31,		*M*
Y Cygni	20 46·6	34 10 N.	,, 26,	21 39	*m*
			,, 29,	21 32	*m*
δ Cephei	22 25·0	57 50 N.	,, 26,	20 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near δ Aurigæ	92°	56° N.	Slow; bright.
,, ζ Ursæ Majoris	200	57 N.	Slow.

THE U.S. COMMISSION OF AGRICULTURE.[1]

"RESOLVED by the Senate and House of Representatives of the United States of America, in Congress assembled, That there be printed 310,000 copies of the Annual Report of the Commissioner of Agriculture for the year 1885; 200,000 for the use of the Members of the House of Representatives, 80,000 for the use of the Members of the Senate, and 30,000 copies for the use of the Department of Agriculture.

"Sec. 2. That the sum of 200,000 dollars is hereby appropriated out of money in the Treasury to defray the cost of the publication of the said Report."

If the British Government desires to assist poor languishing agriculture, it would be well for it to look across the Atlantic Ocean for suggestions as to possible action. A "Commissioner of Agriculture" and an Annual Report from him is in itself enough to arrest attention. The very gilt letters on the back of this volume supply a text upon which a profitable and edifying sermon might be preached. The subject-matter of the Report, its practical or unpractical nature, the sort of topics handled, and the manner of their handling, all ought to arouse curiosity in the minds of those who doubt the utility of Commissions, and prefer *laissez faire* to stirring up with the long pole.

A more scattered flock than the agriculturists of Great Britain it would be difficult to find. Sheep without a shepherd, soldiers without a leader, a fleet without sailing orders, are the metaphors we should use if it were our purpose to portray their present condition. It is not so in America. There the interests of agriculture are watched by a Department of Agriculture, and the splendid Report of the proceedings of this Department serves as a mouth-piece of the whole agricultural community, and exercises the functions of a heart in keeping up a healthy circulation of knowledge and a brain in receiving impressions from all parts of the body agricultural. No wave of pleasurable sensation arising from salubrity of climate or rise of values but causes a smiling paragraph. No twinge of pain caused by insect attack or disease but is at once chronicled and investigated in this excellent Department. The cost is all defrayed by the Government, who are not afraid to spend 200,000 dollars on the mere publication of the Report. Whether the Report is worth the immense sums of money that its material with the large staff of persons employed in collecting the same must have cost is a question of much importance, and not altogether easy to answer. One thing, however, we may be certain of: that if it pays the American Government to undertake the investigation of problems connected with the productive powers of Nature, still more would it pay us with our complicated agriculture and extensive system of cultivation. It might be said in extenuation of our supineness with regard to agricultural science that we have an Agricultural Department of the Privy Council. So far as this Department may prove a nucleus for further expansion it is good, but we cannot conceal from ourselves the narrowness of the scope of our Agricultural Department as compared with the breadth of the aims and objects of the American Agricultural Department. The energies of our Department are chiefly devoted to what is included in the Report before us as the Bureau of Animal Industry, but with this great difference: the English Agricultural Department deals chiefly with inspecting and regulating the ports of debarkation, reporting on outbreaks, and proclaiming infected districts. It is intimately connected with and assisted by the police. The American Bureau of Animal Industry deals in rules and regulations for the suppression and extirpation of contagious diseases, but in addition spends large sums upon investigations into the nature, causes, and remedies of diseases. Its Reports are replete with information as to liquid cultures of the Bacterium of swine fever and other diseases. The American Government have not only set themselves the task of preventing the spread of disease, but are doing excellent work in tracking diseases of all sorts to their source, with a fair and improving prospect of being able to stop their devastations at the fountain-head. Not only is this the case with regard to the diseases of animals, but also of plants, under the sections respectively headed "Report of the Botanist" and "Report of the Microscopist."

Before endeavouring to lay before the readers of NATURE any of the facts recorded in this deeply interesting volume, I will mention the various sections under which information is collected and investigations are prosecuted, feeling confident that by so doing I shall show the many sides from which agriculture obtains direct assistance from the progress of pure science. First in order stands the Report of the Superintendent of Gardens and Grounds, containing valuable information upon mildews and blights, the peach-leaf blister, cracking of pears, and the potato-disease. Next comes the Report of the Chief of the Seed Division, dealing with crossing and hybridization, the production of new varieties of wheat by cross-fecundation, improvement by selec-

[1] "Report of the Commission of Agriculture, 1885." (Washington Government Printing-Office.)

tion and cultivation, vitality of seeds, germination of seeds, and changing seed. The Report of the Botanist consists in an illustrated descriptive list of certain economic plants, and a chapter upon the fungous diseases of plants. Next comes the Report of the Microscopist, dealing with textile fibres, parasites of domestic fowls, crystals of fats—butter, beef, and lard—beautifully illustrated with coloured plates, and highly interesting in connection with the adulteration of butter. I must content myself with a mere enumeration of the Reports of the Chemist, the Chief of Division of Forestry, the Entomologist, the Statistician, the Chief of the Bureau of Animal Industry, a Report on wheat-culture in India, and truck-farming, "or the growing at the South, exclusively for the Northern markets, as a distinct business, of all (or a selection of) such fruit and vegetables as would be likely to arrive at their destination in good condition."

Before concluding this first notice, I must mention the fine manner in which the work is illustrated by tables, diagrams, maps, engravings, and coloured plates. The illustrations of fat-crystals have been already mentioned. Other illustrations are a delicately-tinted and beautifully-drawn collection of twelve edible mushrooms common in the United States; a coloured picture of the transformation of *Cicada septemdecim*, taking place in every stage, on a leafy branch of oak; and splendidly-executed coloured plates illustrative of verminous bronchitis and of ulcerated cæcum in the Section of Animal Industries.

To an Englishman, probably the most interesting portion of the book will be the Report of the Statistician, with its tables of exports and imports, area, and productive power of the United States. The control which the States exercise over the wheat trade of the world is indicated by a diagram showing that, as the yield per acre of the States rises, the prices of wheat all over the world fall; and as the average yield diminishes the prices rise. This correspondence between yield and price is even more precise in the case of oats and maize than in the case of wheat, because, as the Statistician remarks, "we make our own prices for corn and oats, while Liverpool has much to do with the price of wheat."

A very striking diagram, which cannot fail to be of deep interest to those who feel themselves cramped for room in over-crowded England, is one showing the proportion of forests, farms, and unimproved or waste lands in the United States. The vast and almost appalling extent of the first and last sections shows the inexhaustible resources of the country. In Texas, California, Dakota, Montana, New Mexico, Arizona, Nevada, Colorado, Wyoming, Oregon, Idaho, Utah, Kansas, Minnesota, Nebraska, and even Washington, the amount of cultivated or farmed land is quite insignificant compared with the vast tracts of forest and of unreclaimed land. One cannot but reflect upon the fact that a country so wealthy in the raw material of the soil should yet find it advisable to spend money lavishly upon scientific investigation of agricultural difficulties, while England, with her restricted area and dense population, should allow her agriculture to drift, as though its welfare were of no importance, or its downfall no cause of anxiety.

JOHN WRIGHTSON.

WEIGHTS AND MEASURES.

THE Annual Report of the Standards Department of the Board of Trade on its proceedings and business during the past year has been recently issued. It would appear that the Department has of late been pressingly engaged on ordinary work under the several Acts of Parliament which govern its proceedings, but there are some matters of scientific interest referred to in the Report to which we might invite attention.

Standards of various kinds, for determining capacity, length, weight, and volume, continue to be verified for official purposes, or for private use in aid of scientific research or otherwise, without fee or any charge.

Further representations have been made as to the want of a standard hydrometer, accurately adjusted to the legal units of weight and measure in force in this country, for determining the specific gravities of liquids heavier than alcohol.

The sanction of the Treasury has been obtained to the purchase, at an estimated cost of £1000, of copies of the new metric standards of length and weight, which are being prepared at Paris, under the directions of the Comité International des Poids et Mesures.

An exhaustive series of comparisons of the geodetic standards of New South Wales with those of the Board of Trade and the Ordnance Survey has been made by Mr. H. C. Russell, Government Astronomer, Sydney, and Mr. H. J. Chaney, of the Standards Department; of which comparisons a separate Report has been prepared.

In a memorandum on the accurate definition of metrological units, which is attached to the Report, it is pointed out, with regard to metric units, that the relation of the metric unit of weight, the kilogramme, to the metric units of length and capacity is not based on a natural constant, as is generally taught. If the kilogramme prototype were lost, for instance, it would not be restored by reference to the weight of water contained in the cubic-decimetre. The latest experiments have shown that the cubic-decimetre of distilled water ($t = 4°$ C.) weighs, under given conditions, nearly 100 milligrammes less than a true kilogramme weighs. Hence the value of the unit of capacity, the litre, depends on the kilogramme weight, and not on the metre measure. There is *de facto*, it is stated, no more scientific relation between the metric unit of weight, the kilogramme, and the metric units of length and capacity, the metre and litre, than there is between the present conventional metre and the original natural standard of one ten-millionth part of the Paris meridian.

It is curious to note, session after session, how large an amount of purely technical work continues to be added to the ordinary duties of local officers such as inspectors of weights and measures, inspectors of gas, inspectors of petroleum, &c. During the past year we notice, for instance, that the Legislature has, amongst other things, made it necessary for all weighing-machines used at mines in determining colliers' wages, as well as machines used in weighing cattle to be examined and tested by the local inspectors, many of whom are simply constabulary officers. Now the testing of a compound lever weighing-machine requires some special knowledge, but many of the officers have, it would appear, no technical qualification whatever for such duty. Hence the duties imposed by the Legislature are in many districts carried out in an indifferent and perfunctory manner; and another practical instance is afforded of the necessity for increased technical education of that class from which the above local officers are drawn. Without requiring, as in Germany, that every such local officer—as an inspector of weighing-machines—should pass a technical examination, it would certainly appear to be desirable before such officer is appointed that the local authorities (as the County Justices and Town Councils) should see that he has had some proper scientific training.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The term has been very uneventful so far as the Natural Science School is concerned. There has been no new departure in scientific education, and no important conflict with the rest of the University. The most satisfactory feature of the term is the granting of £1200 to be expended during the next three years on the Pitt-Rivers Anthropological Museum. The collection has been enriched by the transference of a quantity of valuable objects from the Ashmolean Museum, and by private gifts; the arrangement and cataloguing of the whole collection is proceeding steadily under the superintendence of Mr. Balfour of Trinity.

Unfortunately Prof. Moseley's continued illness has made it necessary for him to apply for prolonged leave of absence. Dr. S. J. Hickson, Downing College, Cambridge, will act as his deputy next term.

The Millard Laboratory for Experimental Mechanics at Trinity is to be enlarged during the vacation by the addition of a lecture-room and drawing-room, which formed part of Mr. Bosanquet's Laboratory at St. John's.

CAMBRIDGE.—At the annual election of candidates not yet in residence, the following awards were made:—

Foundation Scholarships: H. H. Hough (£80, in Mathematics), St. Paul's School; A. G. Pickford (£60, in Mathematics and Physics), the Owens College, Manchester; E. F. Gedye (£50, in Mathematics), Leys School, Cambridge. Minor Scholarships: F. A. Leete (£50, in Mathematics), Wellingborough School; C. Robertson (£50, in Mathematics), Norwich School. Exhibitions: E. W. MacBride (£50, in Natural Science), Queen's College, Belfast; V. M. Turnbull (£33 6*s*. 8*d*., in Mathematics), St. Bee's School. Sizarships: A. W. Cuff, in Natural Science; R. E. Baker, in Natural Science; W. N. Maw and G. F. J. Rosenberg, in Mathematics.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for November 1887 (volume xxviii. part 2) contains the following papers:—On the development of *Peripatus novæ-zealandiæ*, by Lilian Sheldon, Bathurst Student, Newnham College, Cambridge. (Plates 12 to 16.) The ripe ova are large when compared to those of *P. capensis* or *P. edwardsii*, measuring about 1·5 mm. in their long axis; this size seems due to the enormous amount of food-yolk with which the eggs are charged; the segmentation is on the centrolecithal type; the protoplasm is in the form of a reticulum; there are no traces of cell outlines. The various stages, from a want of material, were not in all cases noted, but the authoress with great ability traces many stages of the development of the embryo, until that in which the food material is completely absorbed, so that the embryo lies just within the vitelline membrane and egg shell. We trust that she will continue her investigations as fresh specimens are obtained, until she is enabled to write the whole life-history of this very interesting form.—On some points in the anatomy of Polychæta, by J. T. Cunningham. (Plates 17 to 19.) This paper gives the results of some investigations into certain Polychæte structures; on the nephridia and gonads, with a criticism of Cosmovici's paper on the "Glandes genitales et Organes segmentaires des Annelides Polychetes"; on the cardiac body, and on the neural canals.—On Temnocephala, an aberrant monogenetic Trematode, by William A. Haswell. (Plates 20 to 22.) Four species were found; one, *Temn. novæ-zealandiæ*, found on *Paranephrops setosus*, from rivers of New Zealand; a second, *Temn. minor*, on *Astacopsis bicarinatus*, from streams of New South Wales; a third, *Temn. quadricornis*, on *Astacopsis franklinii*, from northern rivers of Tasmania; and a fourth, *Temn. fasciata*, on *Astacopsis serratus*, streams of New South Wales. Diagnoses of these species are not given, but it is possible to distinguish them by the comparative details given of their structure; they seem to differ from the type species of the genus, *T. chilensis*, Gay; and Wood-Mason is probably wrong in thinking that this latter species is to be found in New Zealand. When undisturbed, the Temnocephala adhere to the surface of the crayfish by means of a sucker.—Notes on Echinoderm morphology, No. xi.: on the development of the apical plates in *Amphiura squamata*, by Dr. P. Herbert Carpenter, F.R.S.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 8.—"The Post-embryonic Development of *Julus terrestris*." By F. G. Heathcote, M.A. Communicated by Adam Sedgwick, F.R.S.

With regard to the development of the cœlom and generative organs, I have obtained the following results. The somites divide into two parts, as described for *Strongylosoma* by Metschnikoff, one part remaining in the body and the other part projecting into the legs. The cavities in these two parts together constitute the cœlom. The part within the legs breaks up and the cells give rise to muscles. The part within the body passes dorsalwards along the thin sheet of mesoblast which unites it to its fellow of the other side, so that the two vesicle-like parts meet above the nerve-cord in the middle line. They join so as to form a single tube, the generative tube. The young ova, as well as the follicle cells surrounding them, are formed by cells proliferated from the walls of this generative tube. The body parts of the somites of the antennæ and mandibles break up and disappear, but those of the third pair of appendages give rise to the pair of salivary glands. There are two pairs of somites to each double segment.

In the development of the nerve-system, I find that there are two cerebral grooves formed as in *Peripatus*. They disappear early in the development. The ventral nerve-system, which at first consists of two separate cords united by a thin median part, undergoes a process of concentration which results in the presence of a single stout cord showing slight traces of its former double condition.

The heart is formed from mesoblast cells in the body-cavity. These cells, which were directly derived from the hypoblast in the early stages of development, form a network in the body-cavity. The heart is the result of a joining together of the meshes of this network, and thus is formed by the confluence of a series of spaces in the mesoblast, and has nothing to do with the development of the cœlom.

The body-cavity is a series of spaces between the gut and the body-wall, and is divided up by the mesoblast cells already referred to. It is distinct from the cœlomic cavities of the somites, and is therefore a pseudocœle.

The eye-spots are all formed in the same manner. The hypodermis thickens, and a cavity appears within it bounded by pigment. This cavity becomes a distinct vesicle. The front wall of the vesicle becomes very thin and furnishes the lens, while the cells of the back (*i.e.* most internal) wall and sides become elongated and form the retinal elements of the eye. The nuclei of the front wall become very faint and finally disappear.

The most striking feature of the development is the reduction of the ventral part of the young animal and the increase of the dorsal. In the just hatched animal the ventral region is nearly as large as the dorsal, and the legs are wide apart, having a distinct space between them. As development progresses the dorsal region is increased, while the ventral is contracted till the bases of the legs are close together. The corresponding concentration of the nerve-cord I have already mentioned. In a paper on *Euphoberia*, a Carboniferous Myriapod, Mr. Scudder points out that one of the principal points in which the genus differs from existing Diplopoda is the development of the ventral region. The relations of the dorsal and ventral regions of the body of the *Euphoberia* correspond exactly to the condition of the young *Julus*.

With regard to the double segments of *Julus*, Newport held that each double segment corresponded to two segments originally distinct which had fused together; subsequent writers have held that each double segment is a single segment which has developed a second pair of legs. Now considering the double segments with regard to the development as well as to the adult condition, we see that the mesoblastic segmentation is double, so are the tracheal, the nervous, and circulatory systems. The only part of these double segments which is single is the dorsal plate with its stink glands which arise as invaginations in it; this dorsal plate being so enlarged as to form a complete ring round the body of the adult. Looking at the palæontology, we find that in the Archipolypoda, a family including the Archidesmidæ, Euphoberidæ, and Archijulidæ, the dorsal plate did show distinct traces of a division. Therefore I think that each double segment represents two complete segments, the dorsal plates of which have fused together to make one plate.

Zoological Society, December 6.—Prof. W. H. Flower, F.R.S., President, in the chair.—Mr. Howard Saunders exhibited (on behalf of the Rev. H. A. Macpherson) a specimen of the Isabelline Chat (*Saxicola isabellina*) shot in Cumberland, being the first recorded occurrence of this species in Great Britain.—Prof. Bell exhibited and made remarks on specimens of the tegumentary glands from the head of the Rocky Mountain Goat (*Haplocerus montanus*).—A communication was read from Prof. H. H. Giglioli and Count T. Salvadori, containing notes on the fauna of Corea and the adjoining coast of Manchuria. The notes were founded on a large collection, principally of Vertebrates, made by order of H.R.H. Prince Thomas of Savoy, Duke of Genoa, whilst he was in command of the *Vettor Pisani*, on a voyage round the world, 1878–81. The collection was stated to be now deposited in the Royal Zoological Museum at Florence.—A communication was read from M. L. Taczanowski, containing a list of birds collected in Corea by M. J. Kalinowski between September 1885 and March 1887. A Woodpecker in the collection was considered to be new to science, and named *Thriponax kalinowskii*.—Prof. W. H. Flower read a paper on the Pygmy Hippopotamus of Liberia (*Hippopotamus liberiensis*), and its claims to distinct generic rank. The specimen of this animal in the National Collection possessed two incisor teeth on one side of the lower jaw. This and other considerations induced the author to question the advisability of separating it generically from *Hippopotamus*.—Mr. Francis Day, communicated a paper by Mr. J. Douglas-Ogilby, of the Australian Museum, Sydney, on a new genus and species of Australian Mugilidæ, which he proposed to designate *Trachystoma multidens*.—Mr. Day also read a second paper by Mr. Ogilby, giving the description of a new genus of Percidæ based on examples taken in the Gulf of St. Vincent, South Australia, which the author proposed to describe as *Chthamalopteryx melbournensis*.—A communication was read from Dr. M. Menzbier, of Moscow, describing a third

species of Caucasian Wild Goat. This he proposed to call *Capra severtzovi*, being the *C. caucasica* of Dinnik, but not of Guldenstaedt.—Mr. Blanford read some critical notes on the nomenclature of Indian Mammals, in which he treated of *Macacus ferox*, Shaw (*M. silenus*, auct., *nec* Linn.), *M. irus*, Cuv. (*M. cynomolgus*, auct., *nec* Linn.), *M. rhesus*, *Presbytes thersites* Blyth, *Semnopithecus chrysogaster*, *Felis bengalensis*, *F. jerdoni*, *Herpestes mungo* (*H. griseus*, auct., *nec* Geoffr.), *Vulpes vulgaris*, *V. alopex*, and the genera *Putorius*; *Mustela*, *Xantharpyia*, *Cynonycteris*, *Hipposiderus*, and *Phyllorhina*.

Geological Society, December 7.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—A letter from H.M. Secretary of State for the Colonies, inclosing an account of recent discoveries of gold in the Transvaal.—On the age of the altered limestone of Strath, Skye, by Dr. Archibald Geikie, F.R.S. The remarkable alteration of the limestone of Strath into a white saccharoid marble, first described by Macculloch, has hitherto been regarded as an instance of contact-metamorphism in a rock of Liassic age. The various writers who have described the geology of the district have followed Macculloch in classing the whole of the ordinary and altered limestone with the Secondary series of the Inner Hebrides. The author, however, saw reason in 1861 to suspect that some part of the limestone must be of the age of the Durness Limestone of Sutherland—that is, Lower Silurian; and he expressed this suspicion in a joint paper by the late Sir R. I. Murchison and himself, published in the eighteenth volume of the Quarterly Journal of the Society. He has recently returned to the subject, and now offers lithological, stratigraphical, and palæontological evidence that the altered limestone is not Lias, but Lower Silurian. In lithological characters the limestone, where not immediately affected by the intrusion of the eruptive rocks, closely resembles the well-known limestones of the west of Sutherland and Rosshire. It is not more altered than Palæozoic limestones usually are. It contains abundant black chert concretions and nodules, which project from the weathered surfaces of the rock exactly as they do at Durness. These cherts do not occur in any of the undoubted Lias limestones of the shore-sections. The limestone lies in beds, which, however, are not nearly so distinct as those of the Lias, and have none of the interstratifications of dark sandy shale, so conspicuous in the true Liassic series. The stratigraphy of the altered limestone likewise marks it off from the Lias. There appears to be a lower group of dark limestones full of black cherts, and a higher group of white limestones with little or no chert, which may be compared with the two lower groups of the Durness Limestone. A further point of connection between the rocks of the two localities is the occurrence of white quartzite in association with the limestone at several places in Strath, and of representatives of the well-known "fucoid beds" at Ord, in Sleat. These latter strata form a persistent band between the base of the limestone and the top of the quartzite, which may be traced all the way from the extreme north of Sutherland southward into Skye. Palæontological evidence confirms and completes the proof that the limestone is of Lower Silurian age. The author has obtained from the limestone of Ben Suardal, near Broadford, a number of fossils which are specifically identical with those in the Durness Limestone, and so closely resemble them in lithological aspect that the whole might be believed to have come from the same crag. Among the fossils are species of Cyclonema, Murchisonia, Maclurea, Orthoceras, and Piloceras. The relations of the limestones containing these fossils to the other rocks were traced by the author. He showed that the Lias rests upon the Silurian limestone with a strong unconformability, and contains at its base a coarse breccia or conglomerate, chiefly composed of pieces of Silurian limestone, with fragments of chert and quartzite. The metamorphism for which Strath has been so long noted is confined to the Silurian limestone, and has been produced by the intrusion of large bosses of granophyre (Macculloch's "syenite") belonging to the younger, or Tertiary series of igneous rocks. After the reading of the paper some remarks were made by Mr. Etheridge, Dr. Hicks, Mr. Marr, Dr. Hinde, and Mr. Bauerman. In thanking the Fellows for the reception they had given to his paper, Dr. Geikie said that a preliminary sketch of the results of the recent work of the Geological Survey in the north-west of Scotland would, he hoped, be presented to the Society early next year.—On the discovery of Trilobites in the Upper Green (Cambrian) Slates of the Penrhyn Quarry, Bethesda, near Bangor, North Wales, by Dr. Henry Woodward, F.R.S.—On *Thecospondylus daviesi*, Seeley, with some remarks on the classification of the Dinosauria, by Prof. H. G. Seeley, F.R.S.

Entomological Society, December 7.—Dr. David Sharp, President, in the chair.—Mr. Jenner-Weir exhibited, and made remarks on, twelve specimens of *Cicadetta hæmatoides*, collected last summer in the New Forest by Mr. C. Gulliver.—Mr. McLachlan exhibited a specimen of *Pterostichus madidus*, F., which he had recently found in a potato. It seemed questionable whether the beetle had been bred in the cavity or had entered it for predaceous purposes. Mr. Theodore Wood, Mr. Kirby, and Mr. Herbert Cox took part in the discussion.—Mr. McLachlan also exhibited two specimens of a species of Trichoptera—*Neuronia clathrata*, Kol.—which occurred rarely in Burnt Wood, Staffordshire, and elsewhere in the Midlands. On inquiry he was informed that the two specimens exhibited had been found in the Tottenham Marshes.—Mr. Porritt exhibited a series of *Cidaria russata*, from Yorkshire, the Isle of Man, the Hebrides, and the south of England. The specimens from the two first-named localities were almost black.—Mr. Verrall exhibited a specimen of *Mycetæa hirta*, Marsh., which was found devouring a champagne-cork. The Rev. Canon Fowler remarked that certain *Cryptophagi* had the same habit. The discussion was continued by Mr. McLachlan, Mr. Jenner-Weir, and Dr. Sharp.—Canon Fowler exhibited specimens of *Acronycta alni* and *Leiocampa dictæa*, which came to the electric light on Lincoln Cathedral during the Jubilee illuminations. He also exhibited a specimen of *Harpalus melancholicus*, Dej.—Mr. Billups exhibited, for Mr. Bignell, an interesting collection of British oak-galls. He also exhibited the cocoon and pupa case of a South American moth, from which he had bred 140 specimens of a species of Ichneumonidæ.—Mr. O. Janson exhibited, for Mr. C. B. Mitford, a collection of Lepidoptera from Sierra Leone.—Mr. White exhibited a curious structure formed by white ants at Akyab.—Mr. Waterhouse exhibited a series of diagrams of the wings of insects, and read notes of observations on the homologies of the veins—a subject to which he had given especial attention for some time past. Mr. Champion, Mr. Verrall, Mr. McLachlan, Dr. Sharp, and Mr. Poulton took part in the discussion which ensued.—Mr. G. T. Baker contributed descriptions of new species of Lepidoptera from Algiers.—Mr. Gervase F. Mathew communicated a paper entitled "Life-histories of Rhopalocera from the Australian Region." The paper was accompanied by elaborate coloured drawings of the perfect insects, their larvæ and pupæ.—Mr. F. Merrifield read a report of progress in pedigree moth-breeding, with observations on incidental points. Mr. Francis Galton alluded to the close attention Mr. Merrifield had given to the subject, and complimented him on the neatness, ingenuity, and skill with which his experiments had been conducted, and on the results he had obtained therefrom. Mr. Poulton, Dr. Sharp, Prof. Meldola, and others continued the discussion.

Chemical Society, December 1.—Mr. William Crookes, F.R.S., President, in the chair.—The following papers were read:—The alleged existence of a second nitroethane, by W. R. Dunstan and T. S. Dymond.—An extension of Mendeléeff's theory of solution to the discussion of the electrical conductivity of aqueous solutions, by Holland Crompton.—Note on electrolytic conduction and on evidence of a change in the constitution of water, by Henry E. Armstrong.—Bismuth iodide and bismuth fluoride, by B. S. Gott and M. M. Pattison Muir.—The action of hydrogen sulphide on arsenic acid, by B. Branner, Ph.D., and F. Tomicek.—Note on the constitution of mairogallol, by C. S. S. Webster.

PARIS.

Academy of Sciences, December 12.—M. Janssen in the chair.—On the law of errors of observation, by M. J. Bertrand. Two propositions are affirmed: first, that, if a given magnitude be repeatedly measured, and the measures grouped by twos in haphazard order, by selecting in each group the greatest of the two errors committed the relation of the mean of the squares of these greatest errors to the mean of the squares of all the errors will converge towards the value $1 + \frac{2}{\pi}$, when the number of essays increases indefinitely; second, if the measures be similarly grouped in threes, the mean of the squares of the

greatest errors in each group divided by the mean of the squares of all the errors has for probable value $1 + \frac{2\sqrt{3}}{\pi}$, and approaches indefinitely towards this value according as the number of essays is increased.—Comparison of the various systems of electric synchronization for astronomical clocks, by M. C. Wolf. These remarks are made in reference to M. Cornu's recent communication on a new process of synchronizing time-pieces. The various systems hitherto employed for this purpose are passed in review, and their respective merits and defects are studied with exclusive reference to their suitability for employment for synchronizing time in observatories, towns, &c.—On the various modes of explosive decomposition of picric acid and the nitric compounds, by M. Berthelot. This inquiry, instituted in consequence of the doubts recently raised regarding the property of picric acid to explode by heating alone, throws much light on the various modes of decomposition of the nitric compounds properly so called, showing how these various modes depend on the initial temperature of the decomposition.—Remarks in connection with the presentation of a work entitled "Collection des anciens Alchimistes Grecs," by M. Berthelot. This "Collection," just issued by M. Berthelot with the co-operation of the Greek scholar, M. Ch. E. Ruelle, embodies much information regarding the origin of alchemy, the precursor of the modern science of chemistry. Compiled from unedited Greek manuscripts scattered over the public libraries of Europe, it comprises about 400 pages of Greek texts dating from the Alexandrine and Byzantine periods, and mostly anterior to the writers who stimulated chemical studies in the West. Amongst the treatises here for the first time edited are those of the pseudo-Democritus, of Synesius, and Olympiodorus, with French translation, copious notes and commentaries. In the introduction of 300 pages is embodied much information on the mystic relations of metals and planets, on the spheres of astrologists, on the old chemical signs and notations here reproduced by the photogravure process, the whole forming a sequel to the author's work "Sur les Origines de l'Alchimie," presented to the Academy two years ago.—On the application of photography to meteorology, by M. J. Janssen. These remarks are made in connection with the presentation of a series of large photographs which were taken on the Pic du Midi last October with a view to recording meteorological phenomena by the photographic process. The series comprises four views of the section of the Pyrenees within the horizon of the Peak at sunrise just before the solar rays have affected the almost continuous mass of clouds stretching from the Atlantic to the eastern part of the Pyrenees, filling all the valleys and enshrouding all but the highest summits of the range. Other photographs exhibit these clouds when acted on by the early rays of the luminary, and converted into the semblance of a storm-tossed sea. Images were also taken of the remarkable effects observed at sunset, and the satisfactory results of this first essay made it evident that photography may be applied with great advantage to the study of atmospheric phenomena.—Researches on the theory of the figure of the planets, by M. O. Callandreau. These researches, forming a second contribution to the study of the planets, are devoted more especially to the larger members of the solar system, in which the extent of flattening of the poles is an element which cannot be neglected in the general calculation.—On the compressibility of the solution of ethylamine in water, by M. F. Isambert. The experiments here described confirm the conclusions already arrived at by the author, showing that the aqueous solutions of the ammoniacal bases must be regarded as true chemical combinations, more or less dissociated, and dissolved in an excess of water.—On the geographical distribution of the Actiniæ inhabiting the French Mediterranean waters, by M. P. Fischer. A list is given of the thirty-three species already determined on the south coast of France. Of these sixteen are found also on the French Atlantic coast, which comprises twenty-four species not yet discovered in the Mediterranean. The northern limits of the French species have been clearly determined, but not so the southern, several forms occurring also in the Red Sea, and on the North and West African seaboards.

STOCKHOLM.

Royal Academy of Sciences, December 14.—Considerations on some theories of atmospheric electricity, by Prof. Edlund.—On the displacement of the shore line along the coasts of Sweden, by Dr. L. Holmström.—An account of the cosmological theory of Mr. Norman Lockyer, and of the views of Nordenskiöld on the subject, by Prof. Baron Nordenskiöld.—On post Glacial deposits containing *Ancylus fluviatilis*, by H. Munthe.—On *Rubus corylifolius*, Arrh., and *Rubus pruinosus*, Arrh., their nomenclature and value as species, by Dr. L. M. Neuman.—On the hyperborean fir, *Pinus silvestris*, L., *b. lapponica*, by Hr. Th. Örtenbladt.—On the development of the primary bundles of vessels of the Monocotyledons, by Miss S. Andersson.—On oyster-culture in Bohus, by Count Ehrensvörd.—On mutations in the coefficient of elasticity of the metals in consequence of the transmission of a galvanic current, by Dr. Mebius.—On conduction of electricity between flames and points, by K. Asperén.—On the influence of temperature on the exponent of refraction and the density of rock-salt, by Miss N. Lagerborg.—On a recent form of the Echinoconidæ, by Prof. S. Lovén.—On the forms of the fruit of *Trapa natans*, L., which formerly existed in Sweden, by Prof. Nathorst.—On the morphology and development of the Pantopoda, by Dr. G. Adlerz.—Contribution to the knowledge of the carbon-hydrates, by Drs. C. Johansson and Ekstrand.—On the action of fuming sulphuric acid on α-naphthalin combined with carbon-hydratic acid, by R. Manzelius.—On the action of sulphuric acid on α-nitro-naphthalin, by W. Palmær.—Analytical researches on the air near the fortress of Waxholm, in October 1885, by Dr. A. E. Selander.—On systems of coincidence of common algebraic differential equations, by Dr. J. Möller.—On some algebraic analogies conducting to elliptic integrals, by Dr. A. M. Johansson.—The conditions for an algebraic analogy,

$$y^n = (x-a)^{m^1} \ldots . (x-a)^{m^r},$$

to lead to elliptic integrals, by the same.—On the wave-length of algebraic curves, by G. Kobb.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Gospel Ethnology: S. R. Pattison (Religious Tract Society).—Tenants of an Old Farm: Dr. H. C. McCook (Hodder and Stoughton).—The Gospel in Nature: Dr. H. C. McCook (Hodder and Stoughton).—British Dogs, Nos. 13 and 14: H. Dalziel (U. Gill).—Proceedings of the Birmingham Philosophical Society, vol. v. part 2 (Cornish, Birmingham).—A Critique of Kant: K. Fischer; translated by W. S. Hough (Sonnenschein).—Circulars of Information of the Bureau of Education, No. 1, 1887 (Washington).—The Study of History in American Colleges and Universities: H. B. Adams (Washington).—Des Mortiers Hydrauliques: M. H. Le Chatelier (Dunod, Paris.

CONTENTS.

THURSDAY, DECEMBER 29, 1887.

THE ROSICRUCIANS.

The Real History of the Rosicrucians. By Arthur Edward Waite. (London: George Redway, 1887.)

WE have since the receipt of this work for review endeavoured to ascertain what notions existed in the brains of our acquaintances on the subject of the Rosicrucians, and have posed the question "Who and what were they?" to many sorts and conditions of men. The minds of many were absolute blanks on this subject: some thought it was the name of a benefit society—some that it was a kind of freemasonry. One gentleman knew Rosicrucian as "the winner of the Alexandra Plate at Ascot in 1871," and but few had any intelligible notion on the matter. We do not estimate our neighbours at a much lower rate than the average; and it may therefore be granted that there is a large section of the British public to whom the Rosicrucians and their doings are unbroken ground, and that there is ample justification for the appearance of a book which is calculated to dispel the prevailing ignorance.

Mr. Waite has already made "the mysteries of magic" his theme, and consequently comes before us as no uncertain guide in the mazes of the occult. In his present work he furnishes a sketch of the state of mystical philosophy in Germany at the close of the sixteenth century, when the Reformation had removed their fetters from the inquirers. Of these there were many, for men's minds seethed with an infinity of speculations, as well philosophical as religious. The Neo-Platonic philosophy, which had lingered throughout the Middle Ages, once again came into more extended repute, and was professed by various disciples, until German mysticism culminated with Paracelsus. It was at such a period of complex opinions and of mystical ways of thought that the existence of the Rosicrucian fraternity was first revealed to the world. The manifestoes put forth by the brotherhood consisted of the "Fama Fraternitatis; or a Discovery of the Fraternity of the most Laudable Order of the Rosy Cross," and of the "Confessio Fraternitatis R.C. ad Eruditos Europæ." In the latter work are incorporated "thirty-seven reasons of their purpose and intention": these condemn the Pope and Mahomet; offer vast treasure to the head of the Roman Empire; disparage the moribund philosophy of the day, offering in its place the meditations of the brethren, who arrogate to themselves an acquaintance with what is transacted in the farthest regions of the earth. Great promises of a general reformation are made; the knowledge of Nature is eulogized beyond the transmutation of metals or the possession of the supreme elixir. The Society professes to accept the Bible as its oracle, whilst it sagely condemns the innumerable expounders who "make a sport of Scripture as if it were a tablet of wax." The brethren were apparently unsuccessful as linguists, for they are careful to explain that having the use of a magic writing and language they are not so eloquent in other tongues, "least of all in this Latin, which we know to be by no means in agreement with that of Adam and of Enoch."

In the "Fama Fraternitatis" we have an exposition of their religious views, a condemnation of "ungodly and accursed gold-making," an offer of communion to such as shall seek them in sincerity, and an account of their origin. They claim that their founder was brother C. R. C. (subsequently identified with Christian Rosencreutz), a noble German born in 1378. At five years of age he was placed in a cloister, "where he learned indifferently the Greek and Latin tongues," and started with one of the monks for Jerusalem. The monk died, and brother C. R. C. never reached his destination; but his skill in physic obtained for him the favour of the Turks, and becoming acquainted with the wise men of "Damcar," in Arabia, he came thither at the age of sixteen. The unfortunate fact that "Damcar" is unknown to chorographers prevents our gratifying our readers by identifying its locality. Here he was received by the learned as one long expected, and was initiated into their arcane wisdom. Thus primed, he came, three years later, to Egypt, and thence to Fez, where he acquired cognizance of the elementary inhabitants, who revealed unto him many of their secrets. After two years he set sail for Spain to confer with the learned, generously offering to correct their errors in moral philosophy and in the arts, as well as the abuses obtaining in matters ecclesiastical. His proposals were, for some unaccountable reason, slighted by the Spanish *savants*, and the misprized brother returned to Germany, gathered round him a few disciples, founded the fraternity of the Rosy Cross, and died at the ripe age of one hundred and six. His tomb was after the lapse of one hundred and twenty years discovered, together with many mystical adjuncts, in a concealed vault. His fair and worthy body was found whole and unconsumed, and resting in proximity to the *Vocabularium*, *Itinerarium*, and Life of Paracelsus. From the "Fama" we learn that the brotherhood acknowledged the divinity of Jesus, the resurrection, a personal devil, two sacraments, the Bible as "the whole sum" of their laws, and the Pope as Antichrist. Such were their religious beliefs. In philosophy they sought a universal synthesis; they aimed at the substance at the base of all the vulgar metals; they held, although they did not originate, the doctrine that self-propagating elemental beings people earth, air, fire, and water, and believed in the *signatura rerum*, a "certain organic vital activity," which is frequently expressed in the exterior form of things, indicating their interior qualities. They seem to have used some form of practical magic, and accepted as fact the transmutation of metals and the existence of "the supreme medicine of the world." With such a nostrum in their possession the least they could do was to heal the sick, and they were accordingly charged to do so gratuitously. The whole manifesto concludes with a declaration that, although making no mention either of their names or meetings, everyone's opinion should come to their hands, in what language soever it be, and that none giving their names should fail to receive a personal visit or a written communication.

Unfortunately, the assertions contained in the publications of the brotherhood are, as Mr. Waite shows, confuted by a critical examination. We are asked to accept the fabulous oriental city, the youth of brother R. C., in spite of his precocious skill in physic, and his erection of a House of the Holy Spirit, where an "unspeakable concourse of

the sick" thronged for cure notwithstanding the fact that the Society remained unknown to Europe till the beginning of the seventeenth century. Thus much we might even be induced to swallow on the *credo quia impossibile est* principle; but the finding, in 1494, of the works of Paracelsus, who had been born in the previous year at Einsiedeln, staggers our faith. Courtesy forbids the lie direct, but, to use a phrase of Mr. Newell's, we incline to think that if the author of the "Fama" ever wrote a work of fiction it would sell.

We have, then, to seek elsewhere for an explanation of the Society's inception, and must do so in post-Lutheran times; the violence of its anti-Papal prejudices, and its ultra-Protestant principles, prohibiting the attribution of its origin to a more remote period. It will render the comprehension of the case more easy if, before theorizing as to the foundation of the Rosicrucians, we note the varying opinions which have obtained as to the signification of the letters F.R.C., which formed the title of the brotherhood, and as to the badge which they employed.

Michael Maier conceived that R. signified Pegasus, and C. *lilium;* others that R. was *ros*, dew, and C. *crux*, cross, dew being deemed the most powerful dissolvent of gold, and the cross being in chemical language equivalent to light—the menstruum of the red dragon, the producer of gold—since the letters L V X are all formed by the limbs of the cross. Again, it has been imagined that F.R.C. stood for Fratres Roris Cocti, or dew digested for the work of transmutation. The Society's published documents, however, sanction the generally received opinion that R. is for *rosa*, rose, and C. for *crux*, cross, and that the letters F.R.C. are the initials of Fratres Rosatæ Crucis.

The device of the Society is a red rose on a red or golden cross, this being usually placed on a calvary. Mr. Waite has some interesting memoranda upon the occult significance of the rose as the feminine emblem contrasting with the masculine cross; of the Brahmanic rose, the residence of the Deity, recurring with similar significance in Dante's Paradise; of Buddha and Indra crucified for stealing the blossom; of the identification of Jesus with the crucified flower; of the rose of Bacchus which enabled Midas to turn all things to gold; of that of Harpocrates consecrated to silence; and so forth. He cites the author of the "Summum Bonum," who sees in the symbol "the cross sprinkled with the rosy blood of Christ"; and the Abbé Constant, who has identified the rose with scientific initiation and the cross with religion, and beholds in their conjunction that happy union the antithesis of which has been chronicled by Mr. Draper. He is nevertheless fain to confess that the whole question of the significance of the crucified rose in its connection with the Society is one of pure conjecture, and that no presumption is offered by the fact of its adoption for its connection with universal symbolism.

Mr. Waite divides the Rosicrucian theorists into three categories. Firstly, such as accept the history of Christian Rosencreutz as that of an actual personage and the "Fama Fraternitatis" as a true history. These he regards as impervious to argument. Accepting the dictum of De Quincey, that the "Fama" is "monstrous and betrays itself in every circumstance," he decides that the legend of Christian Rosencreutz is not historically true, and that the Society did not originate as described. In the second section he places those who regard both personage and relation as mythical; and, in the third, believers in the existence of the secret Society, but who reject the "Fama" as a fiction.

The theorists of these latter categories have mostly sought the author of the Rosicrucian manifestoes amongst the *literati* of the period, whether they regarded him as a hoaxer or a satirist or as the spokesman of a hidden brotherhood. By them the publications in question have been variously attributed to Taulerus, Luther, Wiegel, Joachim Junge, Ægidius Gutmann, or Johann Valentin Andreas. Mr. Waite considers that it is only in the case of the last named that there is any sufficient evidence to support the plea of authorship. The grounds upon which that plea rests are, amongst others, that the writings of Andreas show him to have uniformly favoured secret Societies as a means for the reformation of his age and country. He is the acknowledged author of a work entitled the "Chymical Marriage of Christian Rosencreutz"—a species of alchymical "Pilgrim's Progress," which, after remaining several years in manuscript, was printed at Strasburg in 1616, and a translation of which occupies 97 pages of the present volume. The first manifestoes of the Society had only borne the initials C. R. C.; but the issue of 1615 calls it the Bruderschafft des Rosen-Creutzes, and it is hence argued that the manifestoes and the "Chymical Marriage" had a common author. The hero of the latter work binds a blood-red ribbon cross-wise over his white linen coat, and sticks four roses in his hat—a noteworthy coincidence, the arms of the Andreas family being a saltier between four roses. The connection of this escutcheon with the device of the crucified rose has been urged, as also the identity of the acknowledged principles of Johann Valentin with those set forth in the manifestoes, in favour of his authorship. This opinion has gained support from certain utterances of Prof. Besoldt, himself an intimate friend of Andreas. Against this view it must be remembered that Andreas describes the "Chymical Marriage" as a *ludibrium* of his youth, though he must have been aware that its alchymical contents would certainly be accepted seriously when published in his maturer years; and it is submitted that he, a man of known intellectual nobility, could scarcely have perpetrated a hoax the reprehensible nature of which he had himself stigmatized when dealing with the Rosicrucian manifestoes. Again, the accepted symbol of the fraternity was never a saltier between four roses, but either a Latin cross with a rose at the point of intersection or a cross rising out of a rose. The identification of the arms of Andreas with a badge of the brotherhood, which forms one of the strongest arguments in favour of his authorship, thus falls to the ground. These and other arguments elaborated by Mr. Waite suffice to render it very uncertain that the Rosicrucian publications emanated from Andreas. Mr. Waite suggests that Andreas may have been associated with the previously existing Militia Crucifera Evangelica, and, when disgusted with its assumption of occultism, have attempted to replace it by a practical Christian association free from mysticism and its symbols, from pretension to arcane endowments or transcendent powers. But he admits that undoubted difficulties beset this theory, and adds: "To my own mind it is far from satisfactory, and, from a careful consideration of all available materials,

I consider that no definite conclusion can be arrived at." He further declares that the esoteric form of the Society's symbol was a rose in the centre of which is figured a Latin cross: he calls attention to the seal of Luther, on which a heart, surmounted by a cross, is inclosed by the outline of a rose, and hence gathers that the unknown founders of the Society chose this emblem, not from any recondite associations, but simply because the reforming monk was their idol.

With the case of Johann Valentin Andreas the interest of the work culminates. When we have learnt that nothing can be determined, and that there is every reason to believe that could we probe the heart of the mystery we should find little to reward our search, we care little for a record of the progress of Rosicrucianism in France and Germany, or for the writings and biographies of Rosicrucian apologists such as Michael Maier, Robert Fludd, Thomas Vaughan, and John Heydon. Artistically, this continuation is an anti-climax, and the chapters which compose it might have been fittingly relegated to the appendix, together with those remaining sections which are devoted to a refutation of the claims of the Freemasons and of modern Rosicrucian Societies to connection with the original fraternity of the Rosy Cross.

The claim which Mr. Waite puts forward to be considered an impartial historian we readily admit, for we have rarely seen a work of this description that was so free from all attempts at the distortion of facts to dovetail with a preconceived theory. His style is perspicuous, and contrasts most favourably with that of his Rosicrucian rival, Mr. Hargrave Jennings, against whom he tilts with much vigour throughout his pages.

The most interesting portions of the book are those where the author is willing to speak himself; for the lucubrations of the *illuminati*, which fill some 250 out of the 446 pages composing the work, are for the most part insipid and fatuous to the lay mind. It was doubtless necessary to include transcripts of the "Fama" and the "Confessio," these being the authoritative expositions of the Society's views, but we could have spared much of the "Chymical Marriage," and all of the "Universal Reformation of the Whole World by order of god Apollo," which Mr. Waite describes as a fairly literal translation of advertisement 77 of Boccalini's "Ragguagli di Parnasso, Centuria Prima," and which, he adds, "throws no light upon the history or claims of the Rosicrucians." Neither is much learnt from the speculations of the apologists, whose philosophy, although mysterious, is not to be readily identified with that of the fraternity as officially set forth. In wading through such documents, one is reminded of Mr. Shandy's exclamation when Rubenius has furnished him with information on every conceivable point except upon the one on which he sought for it. The work on the whole is well done and satisfactorily produced, but it lacks an index. Had the author furnished as good an index to his volume as the enterprising publisher, Mr. Redway, has added to his advertisements, he would have enhanced its value as a book of reference. To those students of occultism whose palates, undebauched by the intellectual *hashish* of the rhapsodies of mysticism and the jargon of the Kabala, can still appreciate a plain historical statement of facts we gladly commend the book.

THE MECHANICS OF MACHINERY.

The Mechanics of Machinery. By Alex. B. W. Kennedy, Professor of Engineering and Mechanical Technology in University College, London. (London: Macmillan and Co., 1886.)

ALTHOUGH the author explains in his preface that this work is destined to meet the requirements of young students of engineering, still the mathematical student of mechanics would reap immense benefit from a careful study of the novel treatment presented here, and would recognize the shortcomings and unsatisfactoriness of the treatises usually put into his hands.

Here we have a treatise on *real* mechanics, with diagrams, drawn accurately to scale, of *real* machines, and illustrative examples drawn from *real* life, while the ordinary mathematical treatise put into the student's hands is generally a great contrast, by reason of its abstract method of treatment, the unpractical nature of the problems discussed, and its diagrams resembling nothing that ever existed, purposely drawn badly, for the reason, it is urged, that a bad draughtsman can copy them more easily.

Prof. Kennedy, in his preface, explains how he has been driven to the vernacular use of the word "pound" as a name for a unit both of weight and of force, as "the adoption of any other plan would have made the book practically useless to almost all engineers so long as the thousand-and-one problems of their every-day work come to them in their present form." This plan is so perfectly clear and intelligible to ordinary practical men to whom dynamical problems on a large scale are a reality and not a mere theoretical abstraction that it is a pity that Prof. Kennedy has gone back on his principles in inserting in § 30, on force, mass, and weight, an attempt at explanation of the confusion of ideas in books on mechanics written by mathematicians, due to the introduction of the word "mass," a word which the engineer never requires.

The explanation of the relation between force, weight, and acceleration is so simple that it may very well be given here. Taking the gravitation unit of force, universally employed by our engineers, as the attraction of the earth on a weight of one pound, and calling this the *force* of one pound, then a force of f pounds acting on a weight of w pounds will produce acceleration a, such that $\frac{f}{w} = \frac{a}{g}$, by Newton's Second Law of Motion; or, $f = \frac{wa}{g}$.

But, if v is the velocity acquired in feet per second, and s the number of feet described in t seconds from rest, then it is shown in Chap. VII. of the present treatise that $v = at$, $\frac{1}{2}v^2 = as$; so that $ft = \frac{wv}{g}$, and $fs = \frac{wv^2}{2g}$.

Here fs represents the work done on the body in foot-pounds of work, and the dynamical equivalent is $\frac{wv^2}{2g}$ foot-pounds of kinetic energy.

So also the product ft is called the *impulse*, in *second-pounds*, of the force f acting for the time t, and its dynamical equivalent is $\frac{wv}{g}$ units of momentum, the momentum of w pounds moving with velocity v being defined by the product wv.

Now the mathematician noticed that in these equations the quantity w occurs divided by g, so he said, Let us call the quotient $\frac{w}{g}$ the "mass" of the body, and denote it by the letter m, so that $w = mg$, equivalent to taking g pounds as the unit of mass.

Unfortunately, in this way the "mass" of a body, which is the measure of an unalterable quantity, is now measured by a variable unit, while the "weight" of a body, which is now defined by the mathematician as the force with which the earth attracts the body, depending on the local value of g, is, although a variable quantity, always represented by the same number—namely, the number of pounds in the body.

This confusion is entirely obviated if, following the engineers, we discard the word "mass" altogether; if we measure, as is customary in ordinary life, weight in pounds, and if we change the unit of force to the absolute unit, called by Prof. James Thomson the "poundal." Now, if a force of p poundals acts on a weight of w pounds, it will produce acceleration a, such that $p = wa$, and then $pt = wv$, and $ps = \frac{1}{2}wv^2$, so that ps, the work done in foot-poundals, has the dynamical equivalent $\frac{1}{2}wv^2$ foot-poundals of energy; not, as the footnote to p. 248 would imply, that $\frac{1}{2}mv^2 = \frac{wv^2}{2g}$, because $m = \frac{w}{g}$, but because $\frac{1}{2}mv^2$ is the kinetic energy in foot-poundals of m pounds moving with velocity v; while the impulse pt second-poundals, has the dynamical equivalent of wv units of momentum.

The unit of momentum has not yet received a name, but the Committee on Dynamics of the Association for the Improvement of Geometrical Teaching is preparing to suggest a distinctive name.

Supposing then that w represents the weight of a body in pounds, how is it possible, as asserted on pp. 219, 220, that $\frac{w}{g}$ remains constant when the body is moved about to parts of the earth where g has different values? and where is the practical value of estimating the effective inertia in terms of the variable unit of mass, as in § 48, when the constant unit of weight would be simpler and practically more intelligible?

These theoretical questions of units of force and weight have been discussed here at some length, as it is important that Prof. Kennedy in his next edition should carefully revise this part of the subject, which will best be done if he disregards the discussions on "mass" of the ordinary text-books, and if he writes always in the ordinary vernacular language used by engineers.

In Chapter XII., on "Friction in Machines," the true laws of friction are given for the first time in any treatise in this country, Morin's illusory laws as usually taught being entirely discarded. With proper lubrication of machinery the question of friction is properly a question of viscous liquid motion. Some interesting applications, with graphical solutions to such problems as friction-brakes and pulley-tackle, are appended, which ought immediately to be incorporated into academical text-books. Of the same nature are the problems on train-resistance in Chapter IX.: a slip on p. 328 of introducing an extraneous factor, π, need only be mentioned here, as the author himself has already corrected it.

We have discussed the dynamical part of the book first, but it is the kinematical part, which treats of mechanism, which forms the greater half of the book. Here the author has analyzed the classification of machines and their elementary parts with great skill and clearness, and illustrated the theory with excellent diagrams. The idea of the "centrode" is largely used in the book, the invention of which is originally due to Belanger. While analyzing fully the centrodes of valve mechanism, the author has mysteriously stopped short of the discussion of valve diagrams, which, in the steam-engine, is the most important practical application of kinematics. Peaucellier's parallel motion is fully described, with Kempe's amplifications: it would be instructive to see a diagram of Peaucellier's motion as applied to an actual steam-engine. Pröll's velocity and acceleration diagrams are carefully explained, with extensions due to the author: this subject has received considerable development of late from German writers, and is capable of solving very elegantly such difficult and important practical problems as, for example, the determination of the bending moment at any point of a connecting rod.

A very useful table of moments of inertia concludes the volume, but here we should prefer to see k the radius of gyration, called in this book the radius of inertia, or rather k^2, the square of the radius, tabulated, side by side of the corresponding area A or volume V.

In conclusion, Prof. Kennedy's students are to be congratulated on the possession of such an admirable textbook, and it is to be hoped that the style and influence of its teaching will make itself widely felt outside of professional circles.

A. G. GREENHILL.

THE SOLOMON ISLANDS.

The Solomon Islands and their Natives. By H. B. Guppy, M.B., F.G.S., late Surgeon R.N. (London: Swan Sonnenschein, Lowrey, and Co., 1887.)

THE Solomon Islands, whether we consider the romantic narratives of their discovery and rediscovery, the comparatively unsophisticated character of their inhabitants, their faunistic and floral relationships, or their remarkable geological structure, are of more than common interest to the scientific world, and it is a matter for congratulation that their description has been undertaken by a traveller and historian so eminently qualified for the task as is Dr. Guppy. The book which he has produced is a rich storehouse of interesting and important observations, and will henceforth be an indispensable work of reference to every student of the races inhabiting the Pacific islands. It is worth while to lay stress upon this fact for the sake of encouraging future travellers to give their observations to the world, because Dr. Guppy did not at first intend to make any special investigation of the habits and manners of the inhabitants, but was led to do so by the want of interest displayed by those who seemed to have so much better opportunities.

The Solomon Islanders seem to be of various types in different parts of the group, but their prevailing characters are distinctly Melanesian or Papuan. A circumstance is pointed out which seems to indicate the Indian Archipelago as having been the route by which the Eastern Polynesians reached the Pacific. This circumstance

consists in the possibility of tracing the native names of certain trees across the Central Pacific from the Indian Archipelago to the Austral and Society Islands. For instance, in the former locality the *Barringtonia speciosa* goes by the names of *Boewa boeton* and *Poetoen;* in the islands of the Bougainville Straits in the Solomon Group it is called *Puputu;* in Fiji, *Vutu;* in Tonga, *Futu;* and in the Hervey and Society Islands, *E-Hoodu* or *Utu*. The name thus appears to have undergone a kind of progressive modification as the tree has receded from its original home. The large amount of information which Dr. Guppy has been able to collect is mainly due to his remarkable tact in dealing with the natives: he seems to have at once succeeded in establishing friendly relations with all those with whom he came in contact, and though he was continually in their power, going long journeys with no other escort than a body of them, he met with nothing but kindness at their hands. He modestly ascribes this satisfactory result mainly to the soothing influences of tobacco, without which, he says, the white traveller in these islands "is worse off than a man without any money in his purse in London," but something must undoubtedly be attributed to the kindly and conciliatory personal influence of the writer himself.

Where so much excellent matter is given it seems ungrateful to ask for more, but it is impossible to repress the desire for knowledge regarding the dwellers in the interior of these islands, who seem to be always at war with the coast tribes, and are regarded by them with so much contempt that "man-bush" is with the latter a common term of reproach. Very interesting, too, are the worked flints, not unfrequently found in the soil either during agricultural operations or after heavy rains. They may probably have been the work of the primitive Negrito race which was at one time widely spread over this region of the globe. It is worthy of notice that in none of the islands visited by the author was any chalk found which contained flints, but there are records of its existence in Ulaua, another member of the group.

Two chapters, certainly not inferior to the rest of the work in interest, are occupied by a history of our knowledge of this group of islands. It does not often happen that one who has distinguished himself as an explorer is willing to undertake a piece of literary work, calling for the patient and critical examination of an old manuscript, but it is a peculiarly happy chance that has thrown the translation of Gallego's journal into the hands of one whose exceptionally accurate knowledge of the locality has no doubt enabled him to avoid errors into which the best of scholars without such information must have fallen. Hernando Gallego was chief pilot to an expedition which was despatched from Peru under the command of Alvaro de Mendana for the ostensible purpose of spreading the Christian faith among the islanders of the Pacific. In the year 1567 they reached the Solomon Islands and gave names to most of them, but lest the English should attempt to possess themselves of the newfound territory no account of the discovery was published; and hence, after one or two futile attempts on the part of the Spaniards to refind and colonize them, knowledge of their whereabouts gradually became a vague tradition, and at length even their very existence was doubted. Two hundred years elapsed before Carteret sighted and anchored off the group, but he did not land. Then in rapid succession came the discoveries of Bougainville, Surville, Maurelle, and Shortland, but none of these identified their discoveries with the previous work of the Spaniards, and it was reserved for the genius of Buache to point out "that, between the extreme point of New Guinea as fixed by Bougainville and the position of Santa Cruz as determined by Carteret, there was a space of 12½° longitude, in which the Islands of Solomon ought to be found." His conclusion, that the islands seen by Carteret and others were the same as those previously discovered by the Spaniards, though long disputed, is now generally admitted, and justice has been rendered both to the gallant explorers and to the laborious and gifted investigator.

In reading this journal it is impossible not to wish that the chart accompanying the volume were on a somewhat larger scale, that more names had been inserted, and that the author, even if he did not feel at liberty to restore those given by the original discoverers, had at all events inserted them within parentheses.

Of Dr. Guppy's work in natural history it would be difficult to speak too highly. It embraces, in addition to a mass of anthropological material, to which reference has been made above, a general account of the chief divisions both of the animal and vegetable kingdoms. Special attention may be called to the observations upon floating seeds and seed-vessels, which have been utilized by Mr. Botting Hemsley in his work on the oceanic dispersal of plants, and to a remarkable fungous growth (*Pachyma?*) found lying loose upon the soil. Of reptiles, batrachians, and mollusca many new species were obtained; an interesting discussion is given regarding the origin of the edible birds'-nests, and an account of attempts to ascertain by direct evidence whether the *Birgus latro* is really able to husk and break cocoa-nuts for itself, as well as details of experiments on the power of various animals to resist submersion in sea-water. The Solomon Islands stand in a remarkable zoo-geographical position, on the boundary between the Polynesian and Indo-Malayan regions, hence a special interest attaches to these lists of species and biological data. The size of the author's collections is little short of marvellous when it is remembered that for two out of the three years spent there his own cabin was the only place where he could store them. He seems, indeed, to have met with but scanty encouragement from those quarters whence he might reasonably have expected it, and every Briton should blush when he reads and reflects upon the truth of the closing words of Dr. Guppy's Introduction :—

"Stifling my own patriotic regrets, I cannot but think that the presence of Germany in these regions will be fraught with great advantage to the world of science. When we recall our spasmodic efforts to explore New Guinea and the comparatively small results obtained, when we remember to how great an extent such attempts have been supported by private enterprise and how little they have been due to government or even to semi-official aid, we have reason to be glad that the exploration of these regions will be conducted with that thoroughness which can only be obtained when, as in the case of Germany, geographical enterprises become the business of the State."

CROWN FORESTS AT THE CAPE OF GOOD HOPE.

Management of Crown Forests at the Cape of Good Hope under the Old Régime and under the New. By John Croumbie Brown, LL.D. (Edinburgh: Oliver and Boyd; London: Simpkin Marshall and Co., 1887.)

IN June last we noticed a work by Dr. Brown dealing with the schools of forestry in Germany, which, it appears, was the author's fifteenth volume on a variety of forest subjects. He has now presented the public with a new volume, out of a store of thirty said to be ready for publication. This plethora of forest literature showered upon us by Dr. Brown is becoming alarming. We pointed out on the previous occasion that the English reader has, in reality, very little interest to spare for forest questions, and what little does exist will certainly not be augmented by literature of the class under review. Here we have a goodly volume, comprising 352 pages of print, made up of a motley collection of old and new official reports, proceedings of an endless succession of Committees, &c., which, even if it were an official Blue-book, would have to be pronounced badly arranged and filled with quantities of irrelevant matter. We do not mean to say that there is not a silver thread running through the whole; what we desire to point out is that the information to be conveyed and the lesson to be learnt could with the greatest ease have been arranged in a pamphlet of thirty or forty pages. To scatter a few grains amongst a huge quantity of chaff is highly objectionable, and it is our duty to protest in the strongest terms against this class of book manufacture. The author had a really interesting story to tell, which, if placed before the public in a small pamphlet or an article in a periodical, would have been sure to attract attention, and might have done some good.

The story to which we refer is peculiarly English. It has been said that whenever we engage in war we generally begin by incurring some reverses: we then gather up our strength, and meet the enemy in such force that the strife is certain to end in success. If this holds good as regards our frequently occurring little wars, it seems to be no less applicable to our Civil administration. Looking, for instance, at our forest policy at the Cape, which Dr. Brown has brought before us in the present volume, it will be seen that after prolonged playing with the question, and after the forests had been well-nigh ruined, vigorous steps were taken to redeem the past.

As in most countries, the forests at the Cape were originally made use of by the population without let or hindrance. Then, with the arrival of European administration, came the colonist, who also betook himself to the woods, partly to clear the land for cultivation, partly to supply himself with material for his domestic requirements, and partly to cut and sell timber for the purpose of making a livelihood. The woodlands, which appeared sufficient to supply the wants of the native population, were soon found to be incapable of bearing the additional strain caused by a European Government and the inroads made by the accompanying colonist. Some enlightened person perceived that the forests could not last at the new rate of consumption of its produce, and raised the alarm. Inquiries were set on foot, officers reported, and Committees deliberated. It was found that the denudation of extensive areas had become an accomplished fact, and that more were rapidly following in the same path. The principal causes were, as in all similar cases, the following:—

(1) Reckless working of the forests by natives and colonists.

(2) Extensive and frequent fires overrunning the forests, destroying all humus, seedlings, and young trees, and damaging more or less the trees of more advanced age.

(3) Uncontrolled clearing of land for cultivation.

So much having been ascertained, the Government should at once have proceeded to take steps to counteract the evil; but only half-measures were adopted. The Government attempted to bring the forests under control by prohibiting certain acts, without providing an efficient agency to see the restrictions enforced. If in any instance they were enforced, it was found that they interfered with previously prevailing practices, complaints were made, and the strife swayed to and fro. Then the Government of the time tried various means to satisfy all parties. Once it resolved to throw the forests open to private enterprise by offering them for sale. In this manner a certain area passed into the hands of private parties, but fortunately only a limited number of lots were sold. Next, the forests were closed, but this also would not meet the case, and they were opened again, so-called licenses for the removal of fixed quantities of material being issued against small payments. There being no proper staff to control the operations, matters grew from bad to worse. About this time Dr. Brown appeared upon the scene, having accepted the appointment of Government Botanist of the Cape Colony in the year 1863. He soon perceived the unsatisfactory condition of the Cape woodlands, and he strongly urged the introduction of a more systematic treatment. Fresh inquiries were set on foot, new Committees sat and deliberated, but it was not until the year 1881 that really efficient measures were adopted. By that time the mischief had been done, and the yield of the forests was so low that, out of a total consumption of two and a quarter million cubic feet of timber, only a quarter of a million cubic feet came from the colonial forests, while a little over two million cubic feet were imported.

In the year 1881 the services of a French forest officer, Count de Vasselot de Regne, who had previously done excellent service in the fixing of the dunes and creation of extensive new forests at Royan, near Bordeaux, were secured as Superintendent of the Cape forests, and with his advent a new *régime* commenced. The selection of this gentleman, due, we believe, to Colonel Pearson, lately in charge of the English forest students at Nancy, was most fortunate. Although we are not acquainted with the Cape from personal experience, we have no hesitation in saying that the reports issued during the last six years prove the administration of the Cape forests to rest in very able hands, and that substantial progress has been made during that short period towards placing the management on a sound and solid basis. A fairly adequate and competent staff has been brought together, the forests are being demarcated, waste is being put down, fir conservancy has been begun, blank areas are being planted, and there is altogether a fair prospect that, after

some time, the colony will once more be in a position to supply the necessary forest produce from its own woodlands. At the same time the financial aspect of the business has not been overlooked, and there are indications that the woodlands will before long prove to be a source of substantial income to the colonial Exchequer.

The forests of the Cape deserve to be carefully preserved, not only for the purpose of their direct utility in providing timber and other produce, but also for their usefulness in other respects. Whether their existence will increase the rainfall to any appreciable extent may be a matter of doubt, but they certainly moderate the temperature and reduce evaporation; in other words, they husband the water which falls on the soil. This effect is all the more important, because Cape Colony is situated, approximately, between the 28th and 35th degrees of south latitude, and the rainfall over about half the area amounts to less than 10 inches a year, while only a comparatively small portion enjoys a rainfall of over 20 inches.

Considering these matters, we trust that the colonial authorities will now persevere in making up for past remissness by maintaining steadily a policy of efficient forest conservancy. It needed many warnings before the proper steps were taken, and in this respect no one deserves more praise than Dr. Brown. By raising his voice loudly during the years 1863-66 he has certainly deserved well of the Cape Colony. While it is a pleasure to record this, it is to be regretted that our author has not succeeded in placing the history of the case before the public in a more readable form than that adopted in the present volume. SW.

OUR BOOK SHELF.

Thomas A. Edison and Samuel F. B. Morse. By Van Buren Denslow, LL.D., and Jane Marsh Parker. (London: Cassell and Co., 1887.)

THIS book is an evident compilation, principally of newspaper cuttings from the other side of the Atlantic. The authors are Americans. Edison is posed as the inventor of the duplex and quadruplex systems of telegraphy, though each was invented in Europe when he was seven years old; while Morse is lauded as having sent the first telegram in 1844, when telegraphy was seven years old, and flourishing well in England. Edison's grandfather lived to be 102 years old, his father is now living at 83. It is to be hoped that he will live long enough to tire out these foolish defamers of his true merit, for merit, industry, and inventive skill he certainly has. Personally he is a charming man, and impresses one with his modesty and communicativeness. The phonograph, carbon transmitter, and glow lamp are quite sufficient to establish his fame without dragging in apparatus he simply altered or perhaps improved. We read in this silly book, "The very words 'electric light,' must stand for ever as closely associated with the name of Edison as is gravitation with Newton or the telescope with Galileo."

We read (p. 96):—"There have been four eras in the history of the magnetic telegraph. In each of these eras a citizen of the United States has been conspicuous. . . . The first era was that of Franklin and his kite. . . . The second era was that of invention—the era of Morse, Henry House (*sic*), and *Daniell* [so the authors reckon Daniell an American !]. Had the Daniell battery been known in 1827, one Harrison Gray Dyer, of New York, would have given to the world what Prof. Morse did not complete until some seventeen years after.

"The third era was that of the evolution of the telegraph —the multiplication of its effects. Of the many names conspicuous in this era none are more deserving of special mention than Hiram Sibley, and none take precedence of Thomas Alva Edison." [N.B.—Edison was born in 1847.] The fourth era was "an era of chaos in its beginning, when Morse lines, Bain lines, House lines, and O'Reilly lines, with their endless litigations over infringements of patents and broken contracts, local jealousies, disastrous competitions, unequal and capricious tariffs, made investing in telegraph stocks a sure method of throwing away money."

And this is history!

The following story is gravely told:—

"When the boy (Edison) was a little under six years old, he became greatly interested in the fidelity with which an old goose was brooding her nest of eggs. When the young family of golden-green goslings came out and took to the water, he was told that this astounding result was produced simply by the animal heat of the old bird sitting on them. The first lesson in organic chemistry was of a kind too remarkable to be let slip without testing it by experiment. Soon after the boy was missed. Messengers were sent after him everywhere, but he could not be found. 'By and by,' says the sister, 'don't you think father found him curled up in a nest he had made in the barn, sitting on goose eggs and hen eggs and trying to hatch them?'"

Sound, Light, and Heat. By Mark R. Wright. (London: Longmans, Green, and Co., 1887.)

WE gladly welcome the appearance of such an admirable text-book as the one before us. It embraces the work required for the various elementary examinations in sound, light, and heat, but it is in no sense a cram-book. The subjects are treated experimentally, and the arrangement is apparently that which practical experience in teaching has led the author to believe to be the best. The experiments described are thoroughly practical, but, at the same time, the apparatus required is comparatively simple. The author is of opinion—and we quite agree with him—that a beginner's time is best spent in making himself acquainted with the facts of science; he has accordingly given little space to theoretical considerations, but he has carefully avoided making statements that might lead the student to form notions at variance with the modern theories.

The drawings, and the descriptions of the apparatus they represent, leave nothing to be desired. The numerical examples, of which there is a great number, combined with the experimental treatment, entitle the book to rank as one of our best text-books of elementary science, and we can confidently recommend it.

Through the West Indies. By Mrs. Granville Layard. (London: Sampson Low, 1887.)

THE author of this little book spent several months in the West Indies, and heartily enjoyed her expedition. She has nothing very new to say about the various places she visited, but she writes pleasantly, and succeeds in conveying a vivid impression of many of the scenes by which she herself was strongly impressed. Occasionally she offers shrewd suggestions as to the industry and trade of the West Indian Islands, and she gives as an appendix a useful paper on "The Sugar Question." This paper contains the substance of notes and suggestions furnished by the Hon. W. H. Ioner, Member of the Legislative Council, Barbados.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

"The Conspiracy of Silence."

WILL you allow me a word on "The Great Lesson" by the Duke of Argyll? It is especially what is said about Darwin's coral-island theory in the following lines, to which I wish to refer:—"All the acclamations with which it was received were as the shouts of an ignorant mob. It is well to know that the *plébiscites* of science may be as dangerous and as hollow as those of politics. The overthrow of Darwin's speculation is only beginning to be known. . . . Reluctantly, almost sulkily, and with a grudging silence as far as public discussion is concerned, the ugly possibility has been contemplated as too disagreeable to be much talked about."

The terms "ignorant mob," "sulkily," and "grudging silence," as used above, cannot readily be forgotten if forgiven by men of science on this side of the Atlantic any more than by their brethren in England.

I am unable to see anything sulky or silent in the exposition of Mr. Murray's coral-island theory of over three pages in length, which was published and sent to all the scientific world in NATURE, vol. xxii. p. 351; nor in the many articles in the current literature and recent geological text-books that have since appeared. In this country no large text-book of geology has been issued since 1880; but Mr. John Murray's work has been fairly discussed, and, so far as I know, has always been recognized. Here at Williams College, for example, the views of Mr. Murray referred to have been expounded each year in the course in geology since 1880. One may, I take it, differ from the Duke of Argyll in accepting or rejecting, wholly or in part, any theory, without laying himself open to the charges quoted above. Of anything like sulkiness or grudging silence I have yet to see or hear the first evidence. There is indeed a "great lesson" in the article by the Duke of Argyll, but it is hardly the one he intended to give. SAMUEL F. CLARKE.

Williams College, Williamstown, Mass., December 5.

"DARWIN'S LIFE AND LETTERS" are now public property, and as reference to vol. iii. p. 242, shows—what nearly every scientific man knew—that the late Sir Wyville Thomson was distinctly anti-Darwinian in his views, it follows that the Duke of Argyll's inferences as to his reasons for urging Mr. Murray's withdrawal of the "new coral-reef theory" paper from the Royal Society of Edinburgh is illogical, not to say absurd. In justice to Sir Wyville's memory and in support of Mr. Bonney's surmise (NATURE, November 24, p. 77) I wish to state that, talking with Sir Wyville about "Murray's new theory," I asked what objection he had to its being brought before the public? The answer simply was: he considered that the grounds of the theory had not as yet been sufficiently investigated or sufficiently corroborated, and that therefore any immature, dogmatic publication of it would do less than little service either to science or to the author of the paper.

AN OLD PUPIL OF WYVILLE THOMSON'S.

December 17.

Greenland Glaciers.

I HAVE received a letter from Prof. Steenstrup, of Copenhagen, which gives further interesting information respecting the extraordinarily rapid advance of the Greenland glaciers, and corroborates the opinion I expressed in the paper I recently read before the Geological Society, that the rate of advance during the Glacial period may have been far more rapid than that generally assumed, and that that period should be much shortened. Prof. Steenstrup states:—

"Meantime the difference between the Alpine data and the Greenland data seems to have grown greater and greater. During the now returned biennial expedition to our northernmost boundary of the west coast of Greenland, the leader of the Expedition, the clever naval officer, Mr. Care Ryder, has measured a progress or a flow of the great glaciers = 99 feet per diem or in twenty-four hours during the summer, and = 30–35 feet in twenty-four hours during winter months."

This, no doubt, will interest many of your Alpine readers.

JOSEPH PRESTWICH.

Shoreham, Sevenoaks, December 17.

"The Mammoth and the Flood."

IN the notice which you have given of my book, which you are good enough to say is, apart from its theories, a valuable work of reference, I should have been more gratified if you had devoted a little space either to stating my arguments or to refuting them, instead of indulging in a rhetorical wail over my backsliding from the orthodox ways of uniformity.

The theories for which I am responsible have been accepted by so many men in the first rank in science in both hemispheres that I am naturally anxious to have them seriously and severely discussed, and I think your critic would allow that I have justified my hope that this will be the case by converging upon my inferences an unusual array of facts.

It was assuredly quite time that someone who disbelieves in "authority in science" should raise a strong protest against the extravagant position which the English school of geology has taken up on this question of uniformity, an extravagance of which students in other branches of science are hardly aware.

The head of the Geological Survey in this country, speaking not long ago with all the authority and responsibility which surround a President of the British Association, committed himself to the following statement:—"From the Laurentian epoch down to the present day, all the physical events in the history of the earth *have varied neither in kind nor in intensity from those of which we now have experience.*"

This was not the opinion of an irresponsible and eccentric student, but of the official mouthpiece of English geology, and with one notable exception—namely, Prof. Prestwich—it has remained, so far as I know, without protest or repudiation, while Prof. Prestwich himself has been treated as a heretic for the views he has so courageously and ably maintained.

My book is meant to challenge the doctrine of uniformity as generally held by English geologists, and which as held here is largely repudiated both in America and on the Continent.

In regard to its many arguments, I cannot defend them in a letter, but I can shortly examine the only one to which your critic directs attention, and which happens to be a very crucial one.

This is the explanation of the existence of a series of mammoths buried in the tundras of Siberia, throughout its entire length, with their soft parts intact. This fact, which has been known for a century, compelled Cuvier long ago to adopt a conclusion which I have simply accepted and enlarged. I state it shortly in the following extract from my work:—"The facts compel us to admit that when the mammoth was buried in Siberia the ground was soft and the climate genial, and that immediately afterwards the same ground became frozen, and the same climate became Arctic, and that they have remained so to this day, and this not gradually and in accordance with some slowly continuous astronomical or cosmical changes, but suddenly and *per saltum.*" I also argue that the only way I can explain the existence of a chain of such carcasses buried many feet deep in *continuous beds of gravel and clay* is by the operation of one cause only, and that a flood of water on a large scale.

Your critic, who I can hardly think has read the part of my book dealing with this issue, says that the carcasses are found in ice. The fact is, *they are never found in ice*, as the Russian explorers have so well shown. The reference to ice in the account of the discovery of the famous Adam's mammoth has been shown by Baer to have been altogether misunderstood, and nothing is more clear than that they are found buried deep in hard frozen gravel and clay.

Secondly, he urges a view which was generally held fifty years ago, but which has been completely dissipated by the elaborate researches of the Russian naturalists, especially the geologist Schmidt, and which I quote at length—namely, that the carcasses have in some way been floated down by the Siberian rivers and buried in their warp. As Schmidt shows, the

Siberian rivers make no deposit, either in winter or summer, which could cover in a mammoth. Nor are the mammoths chiefly found near rivers, but on high ground out of the reach of rivers. When they occur near the rivers, it is generally on the head streams, which could not float such carcasses.

Surely, in criticizing my view of a problem which has been the crux of almost every serious student since the days of Cuvier, your critic might have noticed these now elementary facts. It is not fair to me or to your readers to deal with this difficult question as if it could be settled by a casual reference to causes long ago discarded by such authorities as Brandt and Baer, Schmidt and Schrenck.

I am anxious beyond measure to meet with some criticism that I can reply to, and shall not shrink from the issue being tried by the severest tests.

What I complain of, and others more important than myself share my opinion, is that the only answer forthcoming from uniformitarians to test cases like the one above referred to is, ostrich-like, to put their heads in the sand and to cry out, "Since we are committed to Lyell's theory, it is useless to quote facts against us." This may have done in the fifteenth century, but it will not do now when so many critics are abroad.

May I presume to invite a discussion in your paper on this most interesting question? I cannot forget that it was in your pages I first raised it many years ago.

Bentcliffe, Eccles, December 10. HENRY H. HOWORTH.

In regard to the first part of Mr. Howorth's letter, I must remind him that it was admitted in my review that such a being as an irrational uniformitarian did exist, and was duly smitten in his book.

In regard to the occurrence of mammoth carcasses (not skeletons), I wrote of ice with some hesitation, knowing alleged cases to be open to question, but I mentioned it, because, in my opinion, it would be the most difficult to explain, and the strongest case in favour of Mr. Howorth. Where the carcass is preserved in clay or gravel the difficulty is less. All that seems needed is a flood of rather exceptional character, carrying the dead beast rather far north; then, if this happened at the right season of the year, the body might be buried by other floods before decomposition set in (the temperatures might be always low, though sometimes above 32° F.), and so the body might escape unrotted, until it was finally well entombed. My position was that, though this explanation of the escape of a carcass from destruction, under circumstances not very different from the present, was not easy, the explanation of such a series of catastrophes as Mr. Howorth demanded was much harder. The grounds of this opinion cannot of course be stated in the limits of a letter, nor can I discuss *seriatim* the cases which he cites. So far as my memory serves me (I am writing at a distance from any scientific library) they are not so universally favourable to his view as is stated in his letter.

The remainder of Mr. Howorth's letter is open to the charge which he brings against the review, of being merely rhetorical. *Quis tulerit Gracchos de seditione querentes?*

YOUR REVIEWER.

Centre of Water Pressure.

THE following extremely simple construction for the centre of pressure of a homogeneous liquid on a triangular area occupying any position whatever in the liquid has not (I learn from a high authority on hydrodynamics) been hitherto known, and it may be interesting to some of the readers of NATURE.

Let a particle be imagined to be placed at each vertex of the triangle, its mass being proportional to the depth of this vertex from the surface of the liquid; let G' be the centre of gravity of these particles, and let G be the "centre of gravity" of the triangular area. Then P, the centre of pressure, lies on the line GG' at a distance $\frac{1}{4}GG'$ from G.

There is another almost equally simple way of expressing this result; and of course it is known that there are other ways, more or less practically unmanageable, of representing the position of this point, P, by means of momental ellipses, &c.

GEORGE M. MINCHIN.

R.I.E. College, Cooper's Hill, December 15.

The Recent Earthquakes in Iceland.

ON October 28 last, at 20 minutes past 5 in the morning, two earthquakes occurred at Reykjavík, and reports were soon received as to earthquakes in other districts, especially at Cape Reykjanes. The whole peninsula of Reykjanes is covered with lava streams, and there are many craters and fissures. The extreme point of this peninsula seems in former times to have been the scene of many volcanic eruptions. Tradition tells that long ago the promontory stretched eight miles further to southwest than it does now, and that great earthquakes and volcanic eruptions in the years 1389–90 produced the subsidence of the ancient promontory. The land reached then to Eldey (the Fire Island), or, as the Danes call it, "Melsækken." In historic times ten volcanic eruptions are known to have taken place in the neighbourhood of these rocks.

During the night between October 27 and 28 more than forty shocks were felt at the lighthouse of Cape Reykjanes, nine of the lamps were broken, and the house where the lighthouse keeper lives and a warehouse were damaged. A fissure from south-west to north-east was formed in the rocks 2 yards from the lighthouse; the rocks beneath were cracked in several places, and these cracks go in the same direction as the old fissures associated with volcanic cones. At Eyrarbakki the earthquake was observed at 25 minutes past 5, and proceeded from north-north-west to south-south-east. To north-west the earthquake was felt in Borgar fjord, and as far to the south-east as to Eyjafjöll. This shock was therefore felt over an area of more than 4500 square miles.

A less violent earthquake was felt here in Reykjavík on November 13, at 35 minutes past 9 p.m.

In the year 1882 I published in an Icelandic review, *Andvari*, a list of questions concerning earthquakes, nearly the same as were published in 1880 by Prof. A. Heim for the Earthquake Commission in Switzerland. A similar list of questions has now been printed in the Icelandic newspapers. The questions will also be printed separately, and sent to Icelandic clergymen and others who probably take interest in this subject.

Reykjavik, November 30. TH. THORODDSEN.

The Canary Islands.

NOW that the Canary Islands are rapidly becoming better known as one of the most advantageous health-resorts within easy reach of England, it may be of some interest to mention a few facts concerning diseases in the Archipelago.

The one pre-eminent fact is that the climate seems to modify the virulence of the worst, the most dangerous diseases. Puerperal fever, though rather prevalent, is seldom, I may almost say never, fatal, though I know of cases where the patient has been neglected for several days before medical advice was obtained. Diphtheria is also very prevalent in the large towns, owing to the total absence of the most ordinary sanitary precautions, but it seems always to exist in a mild form. I know of certain families who apparently have it frequently, but this terrible disease seems to be only fatal where the most elementary knowledge of nursing is absent.

Fevers of all kinds are lighter in character. The treatment recommended there by the profession is different from that in vogue in England. For example, it starts by a thorough clearing out of the system by means of somewhat violent purgatives and emetics.

Equable as is the climate by day and night, the natives suffer most from chills, which often end fatally. This, I think, may be in a great measure accounted for by the absence of woollen or silken clothing. Those who visit the Canaries from colder northern latitudes where wool is worn next the skin, and who most wisely continue this habit, do not suffer in this way. It is advisable that every article of clothing worn in the islands be either made of wool or silk. Thus armed, one is almost impregnable to the attacks of any disease of a catarrhal nature. Malaria does not exist. Precautions as to hours of recreation, such as keeping in the house at sundown, are in these islands unnecessary, and one may be out on the hottest day at the hottest hour without fear of sunstroke.

The only disease which in any way can be said to be peculiar to, or prevalent in, the Canary Islands is elephantiasis, which, as your readers well know, does not affect well-nourished inhabitants, and is neither contagious nor infectious.

In Gran Canaria diseases of the stomach and intestines are

common among the peasants. Such are clearly traceable to the national food, *gofio*, which in this island is made of Indian corn.

For phthisis the Canary Islands have been proved of inestimable value, and therefore on this point nothing more need be said. The temperature throughout the year, by day and by night, varies exceedingly little. In my recently-published work on these islands I have gone so fully into this question that I need not recapitulate it here.

I should not have thus ventured to trouble you had I not been asked by some leading members of the medical profession to summarize the facts, bearing upon diseases, scattered through the pages of my book and to add thereto others which I had deemed unsuitable for the general reader. OLIVIA M. STONE.

11 Sheffield Gardens, Kensington, W., December 14.

The Ffynnon Beuno and Cae Gwyn Caves.

MR. SMITH has entirely failed to substantiate the statement made by him in his letter of December 1 (p. 105) concerning the drift over the entrance of the Cae Gwyn Cave, which is 20 feet in thickness and full of ice-scratched boulders, many of large size; therefore I need only say in reply that the Geological Surveyors who surveyed this district have examined the section and have had no hesitation whatever in classifying the deposits in the section with the Glacial beds of the area. In regard to the age of river-drift implements as compared with those found in the cavern, which are identical with the implements found in Kent's cavern and the French caves, I need only quote the remarks of M. Lartet ("Reliquiæ Aquitanicæ," p. 9):—"If some are inclined to attribute to the works of human industry found in the 'Diluvium' or 'Drift' a date more ancient than to those occurring in caves with a similar association of animal remains, we are obliged to remark that such a proposition, expressed as a systematic generalization, is not justifiable in any point of view." . . . "Caves were in truth the first shelter which primitive man would choose, whether driven by instinct or determined by reason."

When Mr. Smith calls the implements found in the gravels at Mildenhall, Neolithic, which others claim to be Palæolithic, and one most eminent authority to be pre-Glacial, I am perfectly justified in saying that the classification of such implements, as defined by Mr. Smith, has no chronological value, and therefore I do not think that anyone is likely to be convinced by his arguments when he is "content to resist the idea of the pre-Glacial age of these caves on purely archæological grounds."

HENRY HICKS.

Hendon, December 23.

Distorted Earth Shadows in Eclipses.

WITH reference to the peculiar appearance of the earth's shadow in the lunar eclipse of August 3 of this year, and noted by "H. H." and "M. C." (see NATURE, vol. xxxvi. pp. 367 and 413), it may be of interest to record a similar distortion observed by Capt. A. E. Barlow, on the s.s. *Nizam*, at Suez, on August 23, 1877. The following entry appears in his meteorological log:—

"The eclipse of August 23. The moon as seen at midnight at Suez. Weather fine starlight. A few cir.-c. (amount 3) travelling from northward."

The shadow was irregular and jagged as in "M.C.'s" description. HENRY TOYNBEE,
Marine Superintendent.

Meteorological Office, December 22.

DR. BALFOUR STEWART, F.R.S.

IN the genial Manchester Professor the scientific world has lost not only an excellent teacher of physics but one of its ablest and most original investigators. He was trained according to the best methods of the last generation of experimentalists, in which scrupulous accuracy was constantly associated with genuine scientific honesty. Men such as he was are never numerous; but they are the true leaders of scientific progress:—*directly*, by their own contributions; *indirectly*, though (with rare exceptions) even more substantially, by handing on to their students the choicest traditions of a past age, mellowed by time and enriched from the experience of the present. The name of Stewart will long be remembered for more than one striking addition to our knowledge, but his patient and reverent spirit will continue to impress for good the minds and the work of all who have come under its influence.

He was born in Edinburgh, on November 1, 1828, so that he had entered his sixtieth year. He studied for a short time in each of the Universities of St. Andrews and Edinburgh, and began practical life in a mercantile office. In the course of a business voyage to Australia his particular taste for physical science developed itself, and his first published papers:—"On the adaptation of the eye to different rays," and "On the influence of gravity on the physical condition of the Moon's surface":—appeared in the *Transactions of the Physical Society of Victoria* in 1855. On his return he gave up business for science, and resumed study under Kelland and Forbes, to the latter of whom he soon became Assistant. In this capacity he had much to do with the teaching of Natural Philosophy on occasions when Forbes was temporarily disabled by his broken health. During this period, in 1858, Stewart was led to his well-known extension of Prévost's *Law of Exchanges*, a most remarkable and important contribution to the theory of Radiation. He seems to have been the first even to suggest, from a scientific stand-point, that radiation is not a mere surface phenomenon. With the aid of Forbes' apparatus, then perhaps unequalled in any British University, he fully demonstrated the truth of the conclusions to which he had been led by theory; and the award of the Rumford Medal by the Royal Society, some years later, showed that his work had been estimated at its true value, at least in the scientific world. In fact his proof of the necessary equality between the radiating and the absorbing powers of every substance (when divested of some of the unnecessary excrescences which often mask the real merit of the earlier writings of a young author) remains to this day the simplest, and therefore the most convincing, that has yet been given.

Radiant Heat was, justly, one of Professor Forbes' pet subjects, and was therefore brought very prominently before his Assistant. Another was Meteorology, and to this Stewart devoted himself with such enthusiasm and success that in 1859 he was appointed Director of the Kew Observatory. How, for eleven years, he there maintained and improved upon the memorable labours of Ronalds and Welsh needs only to be mentioned here:—it will be found in detail in the *Reports of the British Association*. Every species of inquiry which had to be carried out at Kew:—whether it consisted in the testing of Thermometers, Sextants, Pendulums, Aneroids, or Dipping-Needles, the recording of Atmospheric Electricity, the determination of the Freezing-Point of Mercury or the Melting-Point of Paraffin, or the careful study of the peculiarities of the Air-Thermometer:—received the benefit of his valuable suggestions and was carried out with his scrupulous accuracy.

About twenty years ago Stewart met with a frightful railway accident, from the effects of which he did not fully recover. He was permanently lamed, and sustained severe injury to his constitution. From the vigorous activity of the prime of life he passed, in a few months, to grey-headed old age. But his characteristic patience was unruffled, and his intellect unimpaired.

His career as Professor of Physics in the Owens College has been, since his appointment in 1870, brilliantly successful. It has led to the production of an excellent treatise on *Practical Physics*, in which every necessary detail is given with masterly precision, and which contains (what is even more valuable, and could only have been secured to the world by such a publication) the matured convictions of a thorough experimenter as to

the choice of methods for the attack of each special Problem.

His *Elementary Physics*, and his *Conservation of Energy*, are popular works on physics rather than scientific treatises:—but his *Treatise on Heat* is one of the best in any language, a thoroughly scientific work, specially characteristic of the bent of mind of its Author.

Stewart published, in addition to his *Kew Reports*, a very large number of scientific memoirs and short papers. Many of these (notably the article in the *Encyc. Brit.*, 9th edn.) deal with Terrestrial Magnetism, in itself as well as in its relations to the Aurora and to solar disturbances. A valuable series of papers, partly his own partly written in conjunction with De la Rue and Lœwy, deals with Solar Physics. His paper on the *Occurrence of Flint Implements in the Drift* (*Phil. Mag.* 1862, I.) seems to have been ignored by the "advanced" geologists, one of whose pet theories it tends to dethrone; and to have been noticed only by physicists, especially Sir W. Thomson, whose beautiful experiments have done so much to confirm it. His paper on *Internal Radiation in Uniaxal Crystals*, to which Stokes alone seems to have paid any attention, shows what Stewart might have done in Mathematical Physics, had he further developed the genuine mathematical power which he exhibited while a student of Kelland's.

I made Stewart's acquaintance in 1861, when he was the first-appointed Additional Examiner in Mathematics in the University of Edinburgh, a post which he filled with great distinction for five years. A number of tentative investigations ultimately based upon our ideas as to possible viscosity of the luminiferous medium, effect of gravitation-potential on the physical properties of matter, &c., led to the publication of papers on *Rotation of a disc in vacuo*, *Observations with a rigid spectroscope*, *Solar spots and planetary configurations*, &c. These, as well as our joint work called *The Unseen Universe*, have been very differently estimated by different classes of critics. Of course I cannot myself discuss their value. There is, however, one of these speculations, so closely connected with Stewart's Radiation work as to require particular mention, especially as it seems not yet to have received proper consideration, viz. *Equilibrium of Temperature in an enclosure containing matter in visible motion.* (NATURE, 1871; iv. 331.) The speculations are all of a somewhat transcendental character, and therefore very hard to reduce to forms in which they can be experimentally tested; but there can be no doubt that Stewart had the full conviction that there is in them all an underlying reality, the discovery of whose exact nature would at once largely increase our knowledge.

Of the man himself I cannot trust myself to speak. What I *could* say will easily be divined by those who knew him intimately; and to those who did not know him I am unwilling to speak in terms which, to them, would certainly appear exaggerated.

P. G. TAIT.

CHRISTMAS ISLAND.

PROFESSOR NEWTON sends us the following extracts from a letter received by him from Mr. J. J. Lister, M.A., St. John's College, Cambridge, the naturalist on board H.M.S. *Egeria*, Commander Aldrich, R.N., describing the recent visit to that little-known island:—

"We left Batavia on Tuesday, September 27, about 5 a.m., and were in the Straits of Sunda by the afternoon. We saw the hills on the Java side clearly, scored by many steep-sided valleys, and the green of the fields contrasting brightly with the red volcanic earth. Behind these nearer hills one of the great conical mountains loomed out every now and then from his covering of clouds. To the westward, and more distant, a high volcanic peak on the main island of Sumatra rose above nearer islands, and later in the afternoon we saw the simple conical mass of Krakatão. Next day we were bouncing about in deep blue water, as we steamed south against a head-wind—a change after the quiet sailing over the pale green shallow seas in which we had been since we entered the Straits of Malacca. On Friday, September 30, we reached Christmas Island. The first we saw of it was a long line against the south-east horizon, with a shallow saddle in the middle and a gradual rise at either end—that to the west being the higher. On nearer approach the island was seen to be uniformly covered with trees, with a low cliff, much undermined at the water's edge; above this a gradual slope leads to another steep ascent, which in some places, especially at the projecting headlands, is a bare cliff, in others covered with trees. From this there is a gradual rise to the top. We found that there is a cap of coral limestone over the whole island. The top is formed of gray pinnacled masses with steep fissures between them, and the surface of the rock is worn into a rough honey-comb with sharp points and ridges which break under foot and show the glistening white rock. On the slope of the island this rock forms horizontal terraces, with a rough slope of pinnacled masses or a sheer cliff leading down from them, and these seemed to be in a general way continuous at the same level along the side of the island. I suppose they mark the pauses in its gradual elevation during which a fringing reef has formed. Some pieces of rock, apparently volcanic, were picked up at Flying-fish Cove, but it was not found where they had fallen from.

"No stream or standing water was found. Apparently all the rain that falls soaks into the porous rock at once. The vegetation, however, looked fresh and green, and the under parts of fallen logs were sodden with moisture. On two of the nights during the ten days we were there, there was heavy rain; otherwise we had fine weather. Many of the trees are tall, reaching 150 to 170 feet or more, and some of them have vertical buttresses at the base, which wind about horizontally and give off secondary buttresses. They are often laden with great clumps of birds'-nest ferns, as well as with other ferns, orchids, and parasitical trees, and their trunks are festooned with long straight lianas. I only found two orchids with flowers out, but these were small and inconspicuous. Along the shore there are tangled thickets of screw pines, and another kind grows on the higher part. A large proportion of the trees bear edible fruits. Altogether I am sending home some fifty kinds of flowering plants and fifteen of ferns.

"The rat (*Mus macleari*) swarms on the island. They come out at dusk, and run about, in and out of the tents that were pitched by the shore, through the night. There is another kind of rat which is larger and black, except where the scanty fur on the feet allows the pale skin to show. There is also a shrew mouse, whose short shrill squeak may often be heard in the woods. I caught three of them one night in a pitfall. Several specimens of the fruit-eating bat (*Pteropus natalis*) were obtained, including males, which have no pale-coloured tippet, as Mr. Thomas [P.Z.S., 1887, p. 512] thought might possibly be the case. There is a small insectivorous bat in the island, but I did not succeed in getting one.

"The large fruit-eating pigeon (*Carpophaga whartoni*) is very common. They congregate in the fruit-bearing trees, and may then be shot by the dozen. They are excellent eating, and supplied fresh meat for the ship.

"There is a small dove—brown, with a rich bronzy-green on the back and wings—which is very common. Their habits are remarkably in keeping with their colouring. On trees they are restless and seldom seen, but on the ground, among fallen brown and green leaves, where their colour makes them very inconspicuous, they seem to have no fear. I shot seven one morning close to our place: they were feeding in pairs on fallen berries, and

when one of a pair was shot, the other went on feeding as though nothing had happened.

"The thrush (*Turdus erythropleurus*) is very abundant, and as tame as possible. None of my specimens show any mottling, but Capt. Aldrich told me that he saw one with the breast mottled. The bill and feet are as yellow as a cock blackbird's. I heard no *song*, but they often give a 'chick——chick——chick—chick-chick-chick,' quickening time at the finish.

"Parties of twelve to twenty of a species of *Zosterops* were very common. They had just-fledged young ones among them.

"The other birds we obtained were two hawks, an owl, a swift, a heron, a plover, and a sandpiper. Besides these, frigate-birds, gannets, boobies, and boatswain-birds of two kinds were everywhere abundant.

"We obtained three kinds of lizards, and the *Typhlops* which was found before, but no tortoises. We saw a turtle making off down the beach early one morning, but it got into the sea before it could be turned over.

"We saw no frogs, and heard none.

"We found five kinds of land-shells, four of butterflies, a few moths, and some eighteen species of beetles, besides spiders, centipedes, &c. I have one of the hawks alive, which I hope to be able to bring home to England. . . . . "J. J. LISTER."

Accounts have been received from Captain Aldrich, R.N., of H.M. surveying-vessel *Egeria*, of a recent visit to Christmas Island in the Indian Ocean, made in consequence of the interest attaching to the small collection recently brought thence by Captain Maclear, R.N., (see NATURE, vol. xxxvi. p. 12). Mr. J. J. Lister kindly volunteered to act as naturalist, and proceeded from England to Colombo, whence he took a passage in the *Egeria* for the purpose of collecting.

Captain Aldrich states that the highest point of the island was reached at the expense of considerable labour, but without as much difficulty as was anticipated. This point is 1200 feet high, and not, as was before incorrectly stated, 1580 feet.

The island is coral-clad to the very top, the actual summit being a block of coralline limestone, worn and undermined. No rock other than of a calcareous nature was met with in the island, though a diligent search was made, and holes dug where the soil appeared thickest.

Three tiers of cliffs, probably marking sea-levels, intervene between the top of the existing sea cliffs and the summit. Breaches in these cliffs afforded means of scaling them, aided by the numerous aërial roots of the trees with which the island is densely covered.

Between the cliffs the ground rises irregularly, being covered in some places with soil apparently deep, intermixed with fragments of coral. Tangled jungle and high forest grow everywhere. The vertical rise to the summit where ascended takes place in the following manner, as described by Captain Aldrich :—

Coast cliff	30	feet vertical.
Moderate slope	90	,,
First inland cliff	85	,,
Moderate slope	250	,,
Second inland cliff, Slope, Third inland cliff	95	,,
Steep slope of rough ground	650	,,

The total horizontal distance is about 5000 feet.

Christmas Island therefore appears to be a remarkable instance of the complete casing with coral of an island which, from the time that its nucleus first came within the reef-building zone, has been steadily subjected to a movement of upheaval, varied by pauses, during which the cliffs were eroded by the sea. So far as I am aware, no case of similar magnitude has yet been recorded.

The collections now on their way to England are, it is feared, not so varied as was anticipated from the samples of life brought home by the *Flying Fish*.

A considerable number of interesting photographs were obtained by the officers, and accompany Captain Aldrich's report, which will be published.

The *Egeria* has obtained a line of soundings across the hitherto unfathomed area of the southern Indian Ocean, between the Strait of Sunda and Mauritius, but no details have as yet come to hand.

December 17. W. J. L. WHARTON.

TIMBER, AND SOME OF ITS DISEASES.[1]

II.

THE enormous variety presented by the hundreds of different kinds of woods known or used in different countries depends for the most part on such peculiarities as I have referred to above, together with some others which have not as yet been touched upon. Everybody knows something of the multitudinous uses to which timber is put, and a little reflection will show that these uses are dependent upon certain general properties of the timber. Speaking broadly, the chief properties are its weight, hardness, elasticity, cohesion, and power of resisting strains, &c., in various directions, its durability in air and in water, and so forth; moreover, special uses demand special properties of other kinds also, and the colour, closeness of texture, capacity for receiving polish, &c., come into consideration.

Now, there is no doubt that the structure of the wood as formed by the cambium is the chief factor in deciding these technological characters: it is not the only factor, but it is the most important one. Consequently no surprise can be felt that those who are interested in timber have of late years turned their attention to this subject with a view to ascertain as much as possible about this structure, and to see whether it can be controlled or modified, what dangers it is subject to, and how far a classification of timbers can be arrived at. The more the subject is studied, the more interesting and practically important the matter becomes. The results already obtained (though the study is as yet only in its infancy), have thrown brilliant light on several burning questions of physiology—as witness the researches of Sachs, Hartig, Elfving, and Godlewski, on that old puzzle, to account for the ascent of water in tall trees. The study is, moreover, of first importance for the comprehension of the destruction of timber, due to "dry-rot" and the parasites which cause diseases in standing trees, as is shown by the brilliant researches of Prof. R. Hartig on the destruction of timber by Hymenomycetes; and again as yielding trustworthy information as to the value of different kinds of timber in the arts, and enabling us to recognize foreign or new woods of value. In support of this statement it is only necessary to call attention to the "Manual of Indian Timbers," prepared for the Indian Government by Mr. Gamble; or to refer to the beautiful series of wood-sections prepared by Nördlinger.

It is, of course, impossible in an article like the present to do more than touch upon a few of the more interesting points in this connection; but I may shortly summarize one or two of the more striking of these peculiarities of timbers, if only to show how well worth further investigation the matter is.

Many timbers, from both tropical and temperate climates, exhibit the so-called "annual rings" on the transverse section; but this is not the case with all. Most European timbers, for instance, are clearly composed of such layers; but in some cases the layers ("rings" on the transverse section) are so narrow and

[1] Continued from p. 186.

numerous that the unaided eye can scarcely distinguish them, or the differences between the spring and autumn wood are so indistinctly marked that they may appear to be absent, or are at least obscure, as in the Olive, Holly, and Orange, for instance. It is in the tropics, however, that timber without annual rings is most common, chiefly because the seasons of growth are not sufficiently separated by periods of rest to cause the

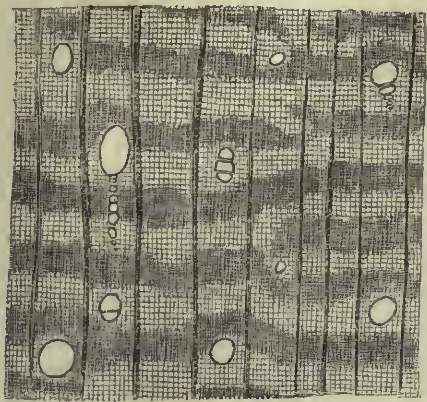


FIG. 7.—Transverse section of the wood of *Pongamia glabra*, Vent., selected to show a type of timber not uncommon in India. No distinct annual rings appear, but the wood is traversed by wavy bands of tissue, which may run into one another or not. The vessels ("pores") are few and scattered, and differ in size; the medullary rays well marked, but not large. To this type—differing in other details—belong many species of figs, acacias, and other Asiatic Leguminoseæ, &c.

formation of sharply-marked zones, corresponding to spring and autumn wood, *e.g.* some Indian Leguminosæ, &c. Zones of tissue of other kinds often occur in such timbers, and have to be understood, since they affect the properties of the wood very differently, *e.g.* some of the Figs.

None of the conifers or dicotyledonous trees, however, are devoid of medullary rays, and distinctive characters

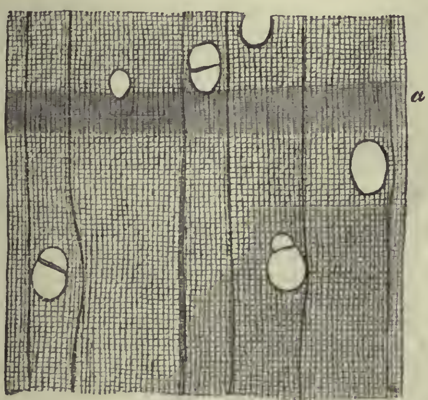


FIG. 8.—Transverse section of wood of *Tamarindus indica*, Linn., selected to show a not uncommon type of Asiatic timber. The annual rings are indistinct, but occasionally indicated by denser tissue (*a*). The vessels are fairly large and few, and scattered much as in Fig. 7, but there are no such broad bands of cells as there.

are based on the breadth and numbers of these: as examples for contrast may be cited the fine rays of the Pines and Firs, and the coarse obvious ones of the Oaks.

Again, the prominence or minuteness, or even (Coniferæ) absence, of vessels in the secondary wood afford characters for classification. The contrast between the extremely small vessels of the Box and the very large ones of some Oaks and the Chestnut, for instance, is too striking to be overlooked. Then, again, in some timbers the vessels are distributed more or less equably throughout the "annual ring," as in the Alder, some Willows and Poplars, &c.; whereas in the Chestnut and others they are especially grouped at the inner side of the annual zone (*i.e.* in the spring wood), and in some cases these

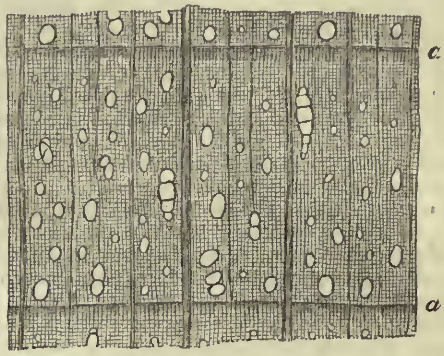


FIG. 9.—Transverse section of the wood of *Acer pseudo-platanus*, selected to show a type of timber common in Europe. The annual rings (*a*) are well-marked and regular. The vessels are small and numerous, and scattered somewhat equally over the whole breadth of the ring. The medullary rays are numerous, some broad, some fine. Many European timbers (beech, hornbeam, lime, &c.) agree with this type, except in details.

groupings are such as to form characteristic figures on the transverse section, as in some Oaks, *Rhamnus*, &c. In the woodcuts (Figs. 7–10) I have given four examples illustrating a few of the chief points here adverted to.

Passing over peculiar appearances due to the distribution of the wood-parenchyma between the vessels, as

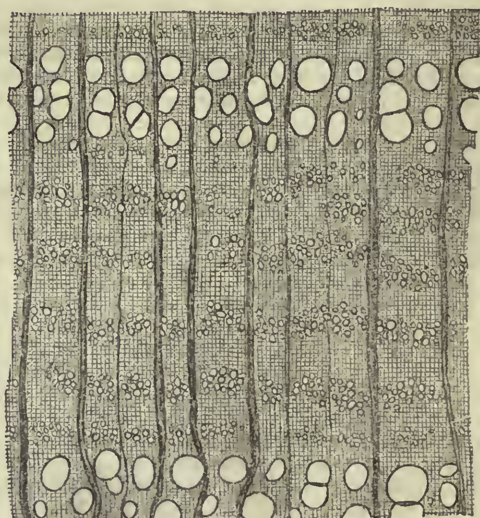


FIG. 10.—Transverse section of wood of the common elm (*Ulmus campestris*), selected as a common type of European timber. The annual rings are very distinct, owing to the large vessels in the spring wood; the vessels formed during the summer and autumn are grouped in bands or zones. The medullary rays are numerous, but not very broad. The oak, ash, chestnut, and others agree in the main with this type, differing chiefly in the *mode of grouping* of the smaller vessels, and in the *breadth* of the medullary rays.

exemplified by the Figs and the Maples, as well as minor but conspicuous features which enable experts to recognize the timber of certain trees almost at a glance, I may now proceed to indicate a few other peculiarities which distinguish different timbers.

The weight of equal volumes of different woods differs

more than is commonly supposed, and there are certain details to be considered in employing weight as a criterion which have not always been sufficiently kept in mind.

A cubic foot of "seasoned" timber of the Indian tree *Hardwickia binata* weighs about 80 lbs. to 84 lbs., while a cubic foot of *Bombax malabaricum* may weigh less than 20 lbs., and all gradations are possible with various timbers between these or even greater extremes. If we keep in mind the structure of wood, it is evident that the weights of equal volumes of merely seasoned timber will yield only approximate results. For even if the seasoning, weighing, &c., are effected in a constant atmosphere, woods which differ in "porosity" and other properties will differ in the extent to which they absorb moisture from damp air or give it up to dry air.

In our climate, timber which is felled in April or May, generally speaking, contains much more water than if felled in July and August: it is, in fact, no uncommon event to find that about half the weight, or even more, of a piece of recently felled timber is due to the water it contains. If this water is driven off by heat, and the piece of wood thoroughly dried, the latter will be found to weigh so much less, but it will increase in weight gradually as it imbibes moisture again.

Now it happens that the weight of a piece of timber, compared with that of an equal volume of some standard substance—in other words, the specific weight—is of very great importance, because several other properties of wood stand in relation with it, *e.g.* the hardness, durability, value as fuel, tendency to shrink, &c. Fresh-cut timber in very many cases contains on an average about 45 to 50 per cent. of its weight of water, and if "seasoned" in the ordinary way this is reduced to about 15 to 20 per cent.; but the fresh timber also contains air, as may easily be shown by warming one end at the fire or in hot water and watching the bubbles driven out, and the seasoned timber contains less water and more air in proportion, so that we see how many sources of error are possible in the usual weighings of timber. At the same time, many comparative weighings of equal volumes of well-seasoned timber do yield results which are of rough practical use.

The fact is that the so-called "specific weight" of timber, as usually given, is not the specific gravity of the wood-substance, but of that *plus* entangled air and water. It is interesting to note that, although we associate the property of floating with wood, timber deprived of its air will sink rapidly, being about half as heavy again as water, volume for volume.

The point just now, however, is not to discuss these matters in detail, but rather to indicate that, other things equal, the density of a piece of timber will be greater, the more of that closely-packed, thick-walled autumn wood it contains; while the timber will be specifically lighter and contain more air when dry, the greater the proportion of the looser, thin-walled spring wood in its "annual rings." In other words, if we could induce the cambium to form more autumn wood and less spring wood in each annual ring, we could improve the quality of the timber; and, in view of the statement which has been made, to the effect that large quantities of timber of poor quality reach the Continental wood-yards every year, this is obviously an important question, or at any rate may become one. The remainder of this article must be devoted to this question alone, though it should be mentioned that several other questions of scientific and practical importance are connected with it.

The first point to notice is that the cambium-cells, like all other living cells which grow and divide, are sensitive to the action of the environment. If the temperature is too high or too low, their activity is affected and may even be brought to an end; if the supply of oxygen is too small, their life must cease, since they need oxygen for respiration just as do other living cells; if they are deprived of water, they cannot grow—and if they cease to grow they cannot divide, and any shortcomings in the matter of water-supply will have for effect a diminution of activity on the part of the cambium. The same is true of the supply of food-substances: certain mineral salts brought up from the soil through the roots, and certain organic substances (especially proteids and carbo-hydrates) prepared in the leaves, are as necessary to the life of a cambium-cell as they are to the life of other cells in the plant. Now, since the manufacture of these organic substances depends on the exposure of the green leaves to the light, in an atmosphere containing small quantities of carbon-dioxide, and since the quantities manufactured are in direct relation to the area of the leaf-surface—the size and numbers of the leaves—it is obvious that the proper nourishment of the cambium is directly dependent on the development of the crown of foliage in a tree. Again, since the amount of water (and mineral salts dissolved in it) will vary with the larger or smaller area of the rootlets and absorbing root-hairs (other things equal), this also becomes a factor directly affecting our problem. Of the interdependencies of other kinds between these various factors we cannot here speak, since they would carry the argument too far for the space at command; some of them are obvious, but there are correlations of a subtle and complex nature also.

First as to temperature. The dormant condition of the cambium in our European winter is directly dependent on the low temperature: as the sun's rays warm the environment, the cambial cells begin to grow and divide again. The solar heat acts in two ways: it warms the soil and air, and it warms the plant. Wood, however, is a bad conductor of heat, and the trunk of a tree is covered by the thick corky bark, also an extremely bad conductor, and it would probably need the greater part of the early summer to raise the temperature of the cambium sufficiently for activity in the lower parts of a tree by direct solar heat: the small twigs, on the contrary, which are covered by a thin layer of cortex, and epidermis, are no doubt thus warmed fairly rapidly, and their early awakening is to be referred to this cause. The cambium in the trunk, however, is not raised to the requisite temperature until the water passing up through the wood from the roots is sufficiently warm to transmit some of the heat brought with it from the soil to the cells of the cambium. This also is a somewhat slow process, for it takes some time for the sun's rays to raise the temperature of the soil while the days are short and the nights cold. Hartig has shown that the cambium in the lower part of the trunk of a tree may be still dormant three weeks or a month after it has begun to act in the twigs and small branches; and it has also been pointed out that trees standing in open sunny situations begin to renew their growth earlier than trees of the same species growing in shady or crowded plantations, where the moss and leaf-mould, &c., prevent the sun from warming the soil and roots so quickly. These observations have also a direct bearing on the later renewal of cambial activity in trees growing on mountains or in high latitudes. Moreover, though I cannot here open up this interesting subject in detail, these facts have their connection with the dying off of temperate trees in the tropics, as well as with the killing of trees by frost in climates like our own. One important practical point in this connection may be adverted to. Growers of conifers are well aware that certain species cannot be safely grown in this country (or only in favoured spots) because the sun's rays rouse them to activity at a time when spring frosts are still common at night, and their young tissues are destroyed by the frosts. Prof. R. Hartig has pointed out a very instructive case. The larch is an Alpine plant, growing naturally at elevations where the temperature of the soil is not high enough to communicate the necessary stimulus to the cambium until the end of May or June.

Larches growing in the lowlands, however, are apt to begin their renewed growth in April, and frosted stems are a common result, a point which (as the renowned botanist just referred to also showed) has an important bearing on that vexed question—the "larch-disease."

The supply of oxygen to the cambium is chiefly dependent on the supply of water from the roots, and the aëration of the stem generally. The water begins to ascend only when the soil is warm enough to enable the root-hairs to act, and new ones to be developed, and the supply of mineral salts goes hand in hand with that of water.

Now comes in the question of the sources of the organic substances. There is no doubt that the cambium at first takes its supply of food-materials from the stores which have been laid by, in the medullary rays, &c., at the conclusion of the preceding year; and it is known that special arrangements exist in the wood and cortex to provide for this when the water and oxygen arrive at the seat of activity.

Assuming that all the conditions referred to are favourable, the cambium-cells become filled with water in which the necessary substances are dissolved, and distended (become turgid, or turgescent, as it is technically called) sufficiently for growth. Speaking generally, and with reference chiefly to the trunk of the tree, which yields the timber, the distension of the cells is followed by growth in the direction of a radius of the stem, and division follows in the vertical plane, tangential to the stem. Then the processes already described with reference to Fig. 5 repeat themselves, and the trunk of the tree grows in thickness.

Now it is obvious that the thickening of the mass of timber inside the cylinder of cambium must exert pressure on the cortex and bark—must distend them elastically, in fact—and some ingenious experiments have been made by De Vries and others to show that this pressure has an effect in modifying the radial diameter of the cells and vessels formed by the cambium. Several observers have promulgated or accepted the view that the differences between so-called spring and autumn wood are due to the variations in pressure of the cortex on the cambium, but the view has lately gained ground, based on experimental evidence, that these differences are matters of nutrition, and a recent investigator has declared that the thick-walled elements and small sparse vessels characteristic of autumn wood can be produced, so to speak, at will, by altering the conditions of nutrition.

It is authoritatively stated that the pines of the cold northern countries are preferred for ships' masts in Europe, and that the wood-cutters and turners of Germany prize especially the timber of firs grown at high elevations in the Bavarian Alps. Now the most striking peculiarity of the timbers referred to is the even quality of the wood throughout: the annual rings are close and show less of the sharp contrasts between thin-walled spring wood and thick-walled autumn wood, and Hartig suggested that this is due to the conditions of their nutrition, and in the following way. The trees at high elevations have their cambium lying dormant for a longer period, and the thickening process does not begin in the lower parts of the trunk until the days are rapidly lengthening and the sun's rays gaining more and more power: the consequence is that the spring is already drawing to a close when the cambium-cells begin to grow and divide, and hence they perform their functions vigorously from the first.

One of the most interesting experiments in this connection came under my observation this summer, owing to the kindness of Prof. Hartig. There is a plantation of larches at Freising near Münich, with young beeches growing under the shade of the larches. The latter are seventy years old, and are excellent trees in every way. About twenty years ago these larches were deteriorating seriously, and were subsequently "under-planted" with beech, as foresters say—*i.e.* beech-plants were introduced under the shade of the larches. The recovery of the latter is remarkable, and dates from the period when the under-planting was made.

The explanation is based on the observation that the fallen beech-leaves keep the soil covered, and protect it from being warmed too early in the spring by the heat of the sun's rays. This delays the spring growth of the larches: their cambium is not awakened into renewed activity until three weeks or a month later than was previously the case, and hence they are not severely tried by the spring frosts, and the cambium is vigorously and continuously active from the first.

But this is not all. The timber is much improved: the annual rings contain a smaller proportion of soft, light spring wood, and more of the desirable summer and autumn wood consisting of closely-packed, thick-walled elements. The explanation of this is that the spring growth is delayed until the weather and soil are warmer, and the young leaves in full activity; whence the cambium is better nourished from the first, and forms better tracheides throughout its whole active period. Such a result in itself is sufficient to repay the investigations of the botanist into the conditions which rule the formation of timber, but this is by no means the only outcome of researches such as those carried on so assiduously by Prof. Hartig in Münich, and by other vegetable physiologists.

It is easy to understand that the toughness, elasticity, and such like qualities of a piece of timber, depend on the character of the tracheides, fibres, &c., of which it is chiefly composed. Investigations are showing that the length of such fibres differs in different parts of the tree. Sanio has already demonstrated that in the Scotch pine, for instance, the tracheides differ in length at different heights in the same trunk, becoming longer as we ascend, and also are longer in the outer annual rings than in the inner ones as the tree grows older, up to a certain period; and this is in accordance with other statements to the general effect that for many years the wood improves, and that better wood is found at the base of the trunk.

However, it is impossible to pursue these subjects in all their details: my object is served by showing how well worthy of the necessary scientific study is timber even to those who are only concerned with it in its usual conditions, and within those limits of variation in structure and function which constitute health. The importance of the subject in connection with the modern development of biology along the grand road of comparative physiology, does not need insisting upon here. It will be the object of further articles to show how it is, if possible, still more important and interesting to know the structure and functions of healthy timber, before the practical man can understand the diseases to which timber is subject. At the same time it must be clearly borne in mind that these are but sketches of the subject; for it is as true of trees and their diseases as it is of men and human diseases, if you would be trainers and doctors you must know thoroughly the structures and peculiarities of the beings which are to be under your care.

H. MARSHALL WARD.

(*To be continued.*)

NOTES.

THE collections of natural history lately forwarded to the British Museum by Dr. Emin Pasha, from Central Africa, will be described at the meeting of the Zoological Society on January 17. The specimens have been determined by various experts in the different branches of natural history to which they belong. Mr. Oldfield Thomas has prepared a paper on the mammals, amongst which are examples of a remarkable

new species of *Hyrax*. Captain Shelley will contribute a paper on the series of birds, which also embraces several new forms. The Lepidoptera have been worked out by Mr. A. G. Butler, and contain specimens of thirteen butterflies new to science, Mr. Edgar A. Smith has examined the fresh-water shells which Dr. Emin Pasha obtained on the Lake Albert Nyanza. They are referable to five species only, but three of these, as might have been expected from the novel locality in which they were obtained, appear to be new to science.

THE Physical Society, of which Dr. Balfour Stewart was President, was represented at his funeral by three of its members: Mr. J. Johnstone Stoney, Prof. G. F. Fitzgerald, and Prof. W. F. Barrett.

IN a lecture lately delivered by Sir Douglas Galton at the Parkes Museum, he drew attention to the increase every year of fog and smoke in London, and to the possibility of their abatement. Dr. Russell's experiments, carried out at St. Bartholomew's Hospital, for the Meteorological Council, showed that the City rain contained twice as much impurity as that collected in the suburbs. That is to say, if the City rain were diluted with nearly an equal bulk of water, we should have the rain of the suburbs. He referred to the experiments of Prof. Lodge with a bell-jar filled with smoke, which is quickly deposited by a discharge of electricity, and argued that by disturbing the electrical condition of the air by kites or balloons, rain may be caused, and by this means the fog dislodged. Failing this, nothing remains but to use gas instead of open stoves, but this method at present costs about four times as much as coal.

WE learn from the *Annales Industrielles* that a mine-shaft is being successfully sunk by M. Alexandre, of the Houssu Company, in Belgium, through a stratum of moist sand 12 metres thick, met with at 70 metres depth, by the Poetsch method, which consists in freezing the sand, then excavating it like rock. In the present case ten iron tubes (with cutting crown) are inserted in the sand at about 1 metre interval, penetrating the coal below. Into these are put other tubes, through which is passed a very cold liquid to return by the larger tubes (generally chloride of magnesium cooled by expansion of ammonia). The sand is frozen more than 3 metres round the tubes. It has the appearance of a rock harder than the compact chalk of the English Channel tunnel; it is sparkling, and speckled with particles of coal. The chloride of magnesium, injected at $-14°$ C., returns at $-12°$. A thermometer inserted 10 centimetres in the stratum read $-8°$. M. Poetsch's method was lately applied to making a tunnel at a small depth under part of the city of Stockholm.

WATERSPOUTS are sometimes seen on the Lake of Geneva, and M. Dufour has made a study of one which occurred on August 19, about 7.30 a.m. (*Arch. des Sciences*). It seems to have arisen on the lake at the meeting of two winds, one from the south, in the eastern part of the lake, and the other from the west, in the western; and its path was along the line of demarkation, changing direction somewhat as it neared the northern shore. Some testified to a rising of the water, which was in violent rotation (in the direction of the hands of a watch). The base of the column was like whirling opaque smoke, which rose in widening spiral, lost above in the cloud. The column was considerably inclined, the upper part advancing more quickly than the lower. In the rear was heavy rain. It is estimated that the *trombe* was about 2 to 3 metres in diameter at foot, and about 106 metres high, and its rate of progress about 760 metres per minute (the speed of an express train). On reaching the shore it disappeared, doing no harm either to vineyard or railway, and had the look of a serpent drawing in its tail. The weather was very variable that day, from hour to hour, and from one part of the lake to another. There was no thunder nor lightning.

THE United States Monthly Weathly Review for September last shows nine depressions in the Atlantic Ocean, of which five were of tropical or sub-tropical origin. Three advanced eastwards from the American Continent north of 45° N., and one appeared over the British Isles. Three of the depressions moved across the Atlantic to Western Europe. As compared with September 1886, there was a slight decrease in the quantity of ice reported; this year the northern limit was lat. 45° 37′ N., and the eastern limit long. 40° 50′ W.

THE Port Officer of Madras has given notice, dated September 22, 1887, that the following storm-signals have been adopted at the ports of the Madras Presidency, instead of the flags hitherto used:—Day-signals: a ball indicates the probable approach of dangerous weather; a drum indicates that a cyclone is likely to approach the port; a cone, apex upwards, at the flag-staff of the port, indicates that it is decided the shipping shall be ordered to sea. Night-signals: three lights, hoisted vertically one above the other, indicate the probable approach of dangerous weather; two lights, hoisted vertically one above the other, indicate that a cyclone is likely to approach the port; three bright lights, hoisted triangularly, one at the mast-head and one at each yard-arm of the flag-staff of the port, indicate that it is decided the shipping shall be ordered to sea.

AT the conclusion of the Colonial and Indian Exhibition some specimens of the American lake-trout (*S. namaycush*), which had been hatched and reared in the Canadian Section, were put in a tank, where they prospered. One fish especially prospered, surpassing the others in size by an inch. In the course of this year they have all disappeared, with the exception of the one referred to, whose colossal form accounts for its missing congeners, which evidently became its prey. Mr. W. August Carter, of the National Fish-Culture Association, states that about 50,000 of these fish were hatched at South Kensington two years ago, when he observed them attack one another soon after leaving their sac. There is a great diversity of growth among them—greater than that which exists among British trout.

A CURIOUS incident is reported by Mr. William Burgess, proprietor of the Midland Counties Fish-Culture Establishment. He states that a pond constructed by him last March, measuring 50 feet by 30 feet, which is entirely isolated from other similar ponds, was shortly after its formation found to be populated with trout fry in their alevin stage. No fish of any kind had been placed in the pond, and none could have entered it, the inlet and outlet being blocked with perforated zinc of a very fine mesh. The soil of the pond in question was excavated from a brook where trout must have previously spawned, and the ova, although buried in mud and flung heedlessly about, survived, and the fry came to life when water had been let into the pond. This is another proof of the enduring capacity of *Salmonidæ* ova.

AT a recent meeting of the Paris Biological Society, M. L. Vaillant offered some remarks concerning the way in which *Antennarius marmoratus*, a curious fish already studied by Agassiz, builds its nest. Each nest is made of one sea-weed (of the Sargasso Sea) the different twigs being brought together and made fast to each other by the fish by means of a pasty sort of substance provided by the animal itself. Agassiz thought that separate bits of sea-weed were used, but it is shown that it uses the whole of the twigs and branches of a single plant; which, of course, allows of much easier work.

THAT the weasel (*Mustela vulgaris*) destroys frogs is proved by the following incident. While standing near a pond on his estate, a gentleman at Worcester observed a weasel give chase to a frog, which it followed to the water and succeeded in capturing. Holding it firmly by the head, the weasel emerged from the water and brought its victim to the bank, but on finding

itself disturbed let go the frog and disappeared. Happening to visit the spot on the succeeding day the gentleman found the frog alive in exactly the same place where it had been left by the weasel, although it had been bitten through to the skull.

AT a recent meeting of the Jena Naturalists' Society, Herr Stahl read a paper on the significance of those excreta of plants known as *raphides*, *i.e.* crystalline needles often met with in the cells in large quantity. From experiments he inferred that they were a protection to plants against being eaten by animals. Many animals avoid plants with raphides, or eat them reluctantly; and some animals, *e.g.* snail species, in eating plants that have raphides select those parts that are without the crystals. Many plants held for poisonous, *e.g. Arum maculatum*, owe their burning taste simply to the very numerous raphides, which, forced out of their cells, enter the tongue and palate. The juice obtained by filtration has quite a mild taste.

WE have received the Proceedings of the Academy of Natural Sciences of Philadelphia, Part 2, April to August 1887. The volume contains a valuable paper, by Mr. Edward Potts, presenting "Contributions towards a Synopsis of the American Forms of Fresh-water Sponges, with descriptions of those named by other authors, and from all parts of the world." In closing this monograph, Mr. Potts says he knows of no more hopeful field of labour for a young American naturalist, seeking for new worlds to conquer, than that provided by the fresh-water sponges. Active workers in this field in North America have, thus far, but glanced at a few streams and lakes, mostly in the neighbourhoods of Buffalo, Chicago, and Philadelphia, and in parts of Florida, Nova Scotia, and Newfoundland. Mr. Potts has little doubt that the rest of the American continent holds many rare prizes in trust for younger and better-equipped explorers.

ANOTHER important paper by Dr. B. Franke, of Leipzig, upon the preparation and constitution of the hydrates of manganic oxide and peroxide is contributed to the current number of the *Journal für Praktische Chemie*. By the action of 100 c.c. concentrated sulphuric acid upon 8 grammes potassium permanganate, a beautiful dark reddish-brown crystalline salt was obtained of the composition $Mn_2(SO_4)_3 . H_2SO_4 . 4H_2O$. When the crystals of this salt were placed in a solution of soda, they were decomposed with deposition of a crystalline powder. These precipitated minute crystals were found to consist of the hydrate of manganic oxide, $Mn_2O_3 . H_2O$, and were shown to possess the constitution $Mn\langle{}^{O}_{O}\rangle Mn\langle{}^{OH}_{OH}$; they were of a steel-gray colour, and possessed metallic lustre. On heating to a temperature exceeding 120°, water was evolved, and black Mn_2O_3 remained. When, however, the reddish-brown salt was dissolved in dilute sulphuric acid, a precipitate afterwards separated out, consisting of pure manganous acid, $MnO_2 . H_2O$, of constitution ${}^{HO}_{HO}\rangle MnO$. On rapidly filtering and washing with water, alcohol, and ether, the acid was obtained as a brown powder, which on ignition became transformed into black Mn_2O_3. One of the most interesting results of similar experiments with hydrochloric acid, which behaves in a precisely analogous manner, is that they throw considerable light upon the mode of action of hydrochloric acid upon manganese dioxide, the reaction so commonly employed for the preparation of chlorine. The first action is shown to consist in the formation of a chlorine substitution-product of manganous acid, thus: $MnO_2 + 6HCl = H_2MnCl_6 + 2H_2O$. This substance is, however, rapidly broken up into manganous chloride, hydrochloric acid, and free chlorine: $H_2MnCl_6 = Cl_2 + MnCl_2 + 2HCl$. A secondary action then commences, the manganous chloride thus formed combines with further quantities of the chlorine substitution-product to form manganic chloride: $MnCl_2 + H_2MnCl_6 = Mn_2Cl_6 + 2HCl$. The manganic chloride, as is well known, does not remain as such, and Dr. Franke shows that, like the sulphate, it is at once decomposed by water as follows: $Mn_2Cl_6 + 4H_2O = Mn_2O_3 . H_2O + 6HCl$. Finally, the free hydrochloric acid decomposes the crystalline hydrate with formation of stable manganous chloride. It is especially important to have this reaction thus thoroughly cleared up, as it is one of the earliest brought to the attention of students.

A NEW method of determining the amount of fusel oil in spirituous liquors, by counting drops from an instrument named a *stalagmometer*, has been lately brought before the Berlin Chemical Society by Herr Traube, who had previously worked out such determinations with a capillarimeter, but finds the new plan preferable on some accounts. The instrument is a short, bent glass tube bulged at one part, into which the liquor is sucked up, being previously diluted to about 20 vols. per cent. The room-temperature being noted, the number of drops in a given volume is counted, and then compared with the corresponding number got at the same temperature from pure 20 per cent. alcohol. A plus of about 1·6 drop per cent. in the former case indicates about 0·1 per cent. fusel oil; one of about 3·5 drops 0·2 per cent. fusel oil, and so on. Even 0·05 per cent. can be certainly determined, and, while the author considers this arrangement quite sufficient for practice, he describes an improved form of his method which admits of determining 0·02 per cent. fusel oil, as well as etheric oils, &c. In this the fusel oil is first expelled from its solution by means of certain salts.

IN a recent series of experiments on the resistance of materials to frost, Herr Blümcke took the method of putting cubes of various kinds of stone in distilled water, under the receiver of an air-pump, and after the air was exhausted and the cube saturated with liquid, exposing the latter to a freezing mixture. He finds that a material is more resistant the less the weight of particles it loses in a given number of freezings. The results corresponded pretty much to experience. Besides the well-known visible phenomena of weathering, there is, even in the first action of frost, a loss of extremely fine particles, not perceptible in the material itself. The appearance of the visible phenomena occurs sooner the more water the stone has taken up. The mode of working has a not unimportant influence on the resistance of materials.

AT the monthly meeting of the Linnean Society of New South Wales, held October 26, Dr. Oscar Katz read a paper on three new kinds of phosphorescent Bacteria, in addition to three already recorded by the author at the meeting of last June: (1) *Bacillus argenteo-phosphorescens liquefaciens*, obtained from sea-water at Bondi; its cultures, liquefying gelatine, emit in the dark a silvery light, which, however, is the weakest of the six kinds hitherto found; (2) *Bacillus argenteo-phosphorescens* II., derived from a luminous piece of a small squid (*Loligo*), and, at the same time, from luminous pieces of the Sydney Garfish (*Hemirhamphus intermedius*, Cant., *H. melanochir*, Cuv. and Val.); (3) *Bacillus argenteo-phosphorescens* III., from the squid already mentioned. Neither of the latter micro-organisms causes liquefaction of the gelatine. They give off in the dark a handsome silver light, much more intense than that of the first-mentioned, but resembling that of the previously-exhibited *Bacillus argenteo-phosphorescens* (now to be designated I.). From this latter Nos. II. and III. distinctly differ. Fuller details about all these luminous Bacteria will be forthcoming shortly.

DURING his last journey to the Amdo, M. Potanin discovered an interesting manuscript containing a Tibetan version of the Mongolian epics of Hesser-Khan. Speaking of this discovery, Prof. Vasilieff has lately expressed his belief that travellers might, if they tried, find many valuable manuscripts in Eastern Turkistan

—relics of the earlier Buddhist era and of the Chinese dominion. Such treasures are probably also to be found in Japan and Corea. It is known that there are Japanese versions of Hiuan Thsang's journey; and Prof. Vasilieff has been informed that manuscripts written on palm-leaves, and brought from India, have been seen in Corea. Many Coreans formerly visited India as Buddhist pilgrims.

A LIST of publications issued by the authority of the Department of Science and Art has just been published. It includes publications specially relating to instruction in science and art, publications relating to the South Kensington Museum, catalogues of reproductions and of loan collections, miscellaneous publications, hand-books, books of photographs, and diagrams.

MESSRS. GIARD AND BONNIER have just published a valuable memoir on the anatomy of the Bopyridæ, with good illustrations.

WHALES—the so-called "herring" whales, which follow the shoals of that fish—are very numerous off the west coast of Norway this winter, and large catches have been made.

IN the Report of a Committee appointed by the British Association "for the purpose of investigating . . . the quantity and character of the water supplied to various towns and districts" from the permeable formations of England, a very misleading statement as to the character of the water-supply of Cheltenham was made. In a later Report of the same Committee, just issued, the error is frankly admitted. "In the Eleventh Report of your Committee," we read, "by a most unfortunate misprint, the reservoirs are described as 'dry' during the drought of 1884, instead of 'short,' as reported by a correspondent, in which statement he was obviously incorrect. Your Committee much regret that the condition of the Cheltenham Waterworks should have been misrepresented by them, as they were fully aware of the ample supply and pure quality given to the town by the Corporation, the purity of which has been testified to by Drs. Allen Miller, Frankland, Way, and Tidy, and Prof. Voelcker."

THE *Brighton Herald* says it is expected that the medallion portrait of the late Dr. Thomas Davidson, F.R.S., Gold Medallist of the Royal Society, Wollaston Medallist of the Geological Society, and first Chairman of the Brighton Museum Committee, executed in marble for the Committee of the Davidson Memorial by Mr. Thomas Brock, A.R.A., will be unveiled early in the new year. The work is said to be an excellent likeness.

A SHOCK of earthquake was reported from Oberhausen on December 9. The direction was from west to east.

LAST week Sir John Lubbock delivered an interesting address in Queen Street Hall, Edinburgh, to the members of the Edinburgh Philosophical Institution on "The Sense and Senses of Animals." He said one would gratefully admit that the dog was a loyal and true and affectionate friend, but when we came to consider the nature of the animal our knowledge was very limited. That arose a good deal from the fact that people had tried rather to teach animals than to learn from them. It had occurred to him that some such method as that which was followed in the case of deaf-mutes might prove instructive if adapted to the case of dogs. He had tried with a black poodle belonging to himself. He then went on to relate several experiments he had made with pieces of cardboard with different words marked upon them. He had taken two pieces of card, one blank and the other with the word "food" upon it. He had put the latter on a saucer containing some bread and milk, and the blank card he put on an empty saucer. The dog was not allowed to eat until it brought the proper card to him. This experiment was repeated over and over again, and in about ten days the dog began to distinguish the card with the letters on it from the plain card. It took a longer time to make the dog realize the difference between different words. In order to try and discover whether the dog could distinguish colours, he prepared six cards, marking two of them blue, two yellow, and two orange. He put one of each on the floor, and tried to get the dog to bring to him a card with the same colour as one which he showed the dog in his hand. After trying this for three months, he found that his experiment in this direction was a failure. He had always felt a great longing to know how the world appeared to the lower animals. It was still a doubtful point whether ants were able to hear. From experiments which he had made, he had come to the conclusion they had not the power of addressing each other. His impression on the whole was that bees and ants were not deaf, but that they heard sounds so shrill as to be beyond our hearing. There was no doubt about insects seeing. He related several experiments he had made with the view of discovering whether different insects could distinguish different colours and had any preference for particular colours. The colours of objects produce upon insects an impression very different from that produced on human beings. The world to them might be full of music which we could not hear, colours which we could not see, and sensations which we could not feel.

THE additions to the Zoological Society's Gardens during the past week include a White-crested Guan (*Pipile jacutinga*) from Guiana, presented by Captain J. Smith, s.s. *Godiva*; two Silky Bower Birds (*Ptilonorhynchus violaceus*) from New South Wales, deposited; two Viscachas (*Lagostomus trichodactylus*) born in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JANUARY* 1–7.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 1

Sun rises, 8h. 8m.; souths, 12h. 3m. 39·5s.; sets, 15h. 59m.: right asc. on meridian, 18h. 46·0m.; decl. 23° 2′ S. Sidereal Time at Sunset, 22h. 42m.

Moon (at Last Quarter on January 6, 12h.) rises, 17h. 29m.*; souths, 1h. 31m.; sets, 9h. 25m.: right asc. on meridian, 8h. 12·0m.; decl. 19° 10′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	7 33 ...	11 19 ..	15 5 ...	18 1·7 ..	24 19 S.
Venus ...	4 19 ...	8 55 ...	13 31 ...	15 37·1 ...	16 38 S.
Mars ...	0 21 ...	6 9 ...	11 57 ...	12 50·3 ...	3 6 S.
Jupiter ...	4 37 ...	9 0 ...	13 23 ...	15 42·1 ...	18 47 S.
Saturn ...	17 59* ...	1 49 ...	9 39 ...	8 29·2 ...	19 31 N.
Uranus ...	0 50 ...	6 23 ...	11 56 ...	13 3·9 ...	6 5 S.
Neptune.	13 19 ...	20 59 ...	4 39* ...	3 43·0 ...	17 57 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Jan.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
1 ...	*d*′ Cancri ...	... 6 ...	4 13	near approach	200 —
5 ...	*b* Virginis...	... 6 ...	2 16 ...	3 24 ...	53 209

Jan.	h.	
1 ...	9 ...	Saturn in conjunction with and 0° 55′ north of the Moon.
2 ...	16 ...	Venus in conjunction with and 1° 51′ north of Jupiter.
4 ...	1 ...	Mercury at greatest distance from the Sun.
6 ...	9 ...	Mars in conjunction with and 2° 46′ south of the Moon.

Saturn, January 1.—Outer major axis of outer ring = 45″·8; outer minor axis of outer ring = 15″·1; southern surface visible.

Variable Stars.

Star	R.A. (1888·0) h. m.	Decl. (1888·0) ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Jan.	5, 22 42	*m*
Algol	3 0·9	40 31 N.	,,	3, 18 55	*m*
λ Tauri	3 54·5	12 10 N.	,,	3, 18 23	*m*
ζ Geminorum	6 57·5	20 44 N.	,,	3, 23 0	*M*
R Canis Majoris	7 14·5	16 12 S.	,,	2, 20 46	*m*
			,,	4, 0 2	*m*
U Virginis	12 45·4	6 10 N.	,,	7,	*M*
δ Libræ	14 55·0	8 4 S.	,,	1, 21 9	*m*
R Libræ	15 47·3	15 54 S.	,,	5,	*M*
U Ophiuchi	17 10·9	1 20 N.	,,	3, 3 52	*m*
		and at intervals of		20 8	
R Lyræ	18 51·9	43 48 N.	Jan.	1,	*m*
R Sagittarii	19 10·1	19 3 S.	,,	3,	*M*
R Sagittæ	20 9·0	16 23 N.	,,	3,	*m*
T Vulpeculæ	20 46·7	27 50 N.	,,	3, 4 0	*m*
Y Cygni	20 47·6	34 14 N.	,,	1, 21 26	*m*
			,,	4, 21 19	*m*
δ Cephei	22 25·0	57 51 N.	,,	2, 19 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ζ Cancri	119°	16° N.	Bright and swift.
,, θ Ursæ Majoris	140	57 N.	Very swift and short.
The *Quadrantids*	228	53 N.	January 1, 2, and 3.

GEOGRAPHICAL NOTES.

THE two great medals of the Paris Geographical Society have been awarded to General Alexis de Tillo for his great topographical work on Europe and Asia, and to M. Alphaud, Inspector-General of the Ponts et Chausées, who, "by inspiring a feeling for the beautiful and of the necessities of hygiene, has done so much to improve the topography of the capital." Medals of the first class were awarded to M. Enguehard, geographical draughtsman; Prof. François Bazin; Prof. Maxime Mabire; Prof. Paul Gaffarel, for a work on the soil of France; M. Fauve, for his fine topographical works; M. Ch. Lasalle, for a work on the defences of France; M. Pierre Collet, for his relief plans; Lieut. Somprou; M. Poinet; and M. Verragen.

LIEUT. VON FRANÇOIS and Dr. Wolf will start shortly on a scientific mission to Togoland, one of the German possessions on the West Coast of Africa.

IN the new part of the Journal of the Manchester Geographical Society there is an instructive paper by the Rev. R. P. Ashe on Uganda, and the manners and customs of its people.

NEWS from Africa states that the well-known African traveller, Herr Gottlob Adolf Krause, has returned to Accra on the Gold Coast. In May 1886 he commenced his exploring expedition. Starting from Accra and crossing the River Acropang-Volta at Kpang, he proceeded in an easterly direction, passing through Kpando, Krahje, Salaga, Dagomba, Walawala, East Gurunsi, and Busanga to Wagaduga and Ban Djagara, penetrating to within a few miles of Timbuctoo. On his return he journeyed through West Gurunsi, the Ashantee District, Kintimso, Salaga, Sogede, Baleta, Gheshi, Atakpama, and Pla. Lieut. Kund on his journey to Cameroon met Herr Krause at Accra, and sends this report. Herr Krause states that to the north of Salaga the influence of the Sahara is most prominent, and the country is more desolate the further north one goes. Rice and tobacco are universally cultivated. The principal articles of commerce are kola-nuts and salt, the district being chiefly inhabited by the Fula tribe. Nearer to the coast there are several other tribes and dialects, but the Haussa language is most generally spoken. Most of the population is still heathen, but some of the merchants and better educated families are Mohammedans. Herr Krause was not enabled to proceed as far as Timbuctoo, owing to the unfriendliness of the Sheikh Tidchani.

THE *Bollettino* of the Italian Geographical Society for October and November publishes a valuable paper by Sig. A. Borda on the geography, history, and present social conditions of the Republic of Columbia (New Grenada), which promises to enter on an era of peace and prosperity under its enlightened and popular President Nunez, who was elected last June for a term of six years. The present population is calculated on official returns at about four millions, including 200,000 still living in the tribal state in the more inaccessible forest regions. These forests are described as abounding in a great variety of valuable trees yielding the finest cabinet woods, balsams, gums, dyewoods, alimentary and medicinal products. The flora and fauna are scarcely exceeded by those of any other land in diversity of types, while the country contains vast supplies of minerals, such as gold, silver, platina, rubies, emeralds, crystals, porphyry, salt, and sulphur. Since the conquest till the present time the yield of the precious metals is estimated at £130,000,000, mined chiefly in the departments of Canea and Antioquia. Mining operations, which had suffered much from the unsettled state of the country, have recently received a fresh stimulus by the introduction of foreign capital and improved engineering appliances. The metalliferous districts, which occur at various elevations, and especially along the river valleys, are stated to be generally salubrious, and foreigners are now enabled to purchase mines on the same terms as the natives. But the great natural resources of Columbia still lie almost untouched, chiefly through the lack of good and regular communications, the roads being generally impracticable for wheeled traffic, while the railway system is little developed. Besides the Panama, Bolivar, and Cucuta lines already open, others are in course of construction in the departments of Canea, Antioquia, Cundinamasca, Tolima, and Santander. The great water highway of the Maddalena has a fleet of twenty-five steamers, and is connected with the seaport of Cartagena by the Digue, a navigable canal branching off at Calamar. The yearly imports from Europe and the United States average £3,000,000, and the exports £1,600,000. The revenue for 1887–88 is estimated at £4,000,000, the expenditure £4,600,000, and the public debt £4,500,000, half internal and half foreign. The Government has still at its disposal extensive domains, which are granted on favourable terms to immigrants as well as to native and foreign speculators. At present the country is in the enjoyment of profound peace, with improved external and internal relations, and a general desire to close once for all the era of aimless political revolutions.

JOURNAL OF THE ROYAL AGRICULTURAL SOCIETY.[1]

THE most recent number of this Journal well keeps up the credit of its predecessors in spite of the grievous loss the Society sustained a year ago in the death of its talented editor, Mr. H. M. Jenkins. The contributors include the Earl of Coventry, Sir F. Bramwell, F.R.S., Drs. J. Voelcker and P. Vieth, Major Craigie, Principal W. Robertson of the Royal Veterinary College, Mr. James Macdonald, of Edinburgh, Messrs. Bernard Dyer, Albert Pell, Charles Whitehead, William C. Little, Charles Clay, Herbert J. Little, and others. Since these remarks were penned, we regret to hear of the sudden death of Principal Robertson, of the Royal Veterinary College.

The contents may be classified as—strictly agricultural, comprising articles on ensilage, sheep-feeding experiments, and reports on the prize-farm competitions in Northumberland; statistical, as presented in papers upon twenty years' changes in our foreign meat supply; engineering, as represented in trials of portable engines, and report of the consulting engineers at Newcastle; and purely scientific, as in papers on micro-organisms and their action on milk and milk products, on protective inoculation for anthrax and quarter ill, and on the progress of the Hessian fly.

Few of the papers possess such a wide general interest, both scientific and sanitary, as that of Dr. P. Vieth, on the action of micro-organisms on milk. Milk is subject to *lactic* fermentation, caused by the presence of a bacillus, consisting of short motionless rods propagating by segmentation. The effect of these bacilli is to cause the milk to sour and lose its liquid character, and assume the appearance of a gelatinous mass. The milk, in fact, "turns," and a separation of the curd from the whey follows, as though rennet had been added, but from a different cause. It is also now shown that lactic fermentation requires to be induced by the introduction of bacilli from without, after the milk is drawn from the cow, and that it is not inherent

[1] "Journal of the Royal Agricultural Society," Series II. vol. xxiii. Part II., 1887. (John Murray, Albemarle Street.)

in the milk. Lactic fermentation cannot take place unless in the presence of free oxygen, and at temperatures ranging from 50° to 114° F. Below and above these limits the process is arrested. The *butyric* fermentation is caused by a bacillus of larger size than that to which lactic fermentation is due, and occurs in milk free from lactic acid and of alkaline reaction. The bacillus of butyric fermentation will withstand a higher degree of heat, and the spores will stand a boiling heat for five minutes very well. *Alcoholic* fermentation is induced in milk which has already passed through the stage of lactic fermentation by means of a special ferment which has been used from time immemorial in the Caucasus under the name of kephir. By the action of this ferment a preparation similar, if not identical with koumiss is produced. *Slimy* fermentation gives what is known in Norway as ropy milk, where it is used as an article of diet. With this fermentation a micro-organism is also associated. Cheese is a product of fermentation from beginning to end. Not only is it a fermentative process by which the curd is separated from the whey, but the processes of ripening also depend upon various micro-organisms. It is generally thought that the differences between the cheese made in various localities, and which so well evade imitation, are due in a measure at least to the propagation and prevalence of micro-organisms of a sort which may be rare or wanting in other districts; and that consequently it may be easy to make cheese of a particular flavour or character in one district which it will be found impossible to produce in another district.

The able paper by Major Craigie, on twenty years' changes in our foreign meat supply, is well worth reading. The paper is deeply interesting to agriculturists, and deals with the probable sources of animal food for the constantly increasing human family. The enormous increase of population in the United States of America is especially noticed, and the following extract from the Commissioner of Agriculture's (Mr. Colman's) address to the "Cattle Kings" assembled at Chicago is significant and hopeful for the future of agriculturists:—"In 1880 we had 50,000,000 of inhabitants; in 1905 we should have 100,000,000; in 1930, 200,000,000; in 1955, 400,000,000; in 1980, less than 100 years hence, 800,000,000 of inhabitants. Where are these teeming millions to live? On what are they to subsist? Where and how are the cattle to be bred and reared that must be relied upon to furnish beef?" In answer to all of which questions we may be permitted to point out that many disturbing causes may operate to check this uniform future development of the population of the States. The wonderful results of geometrical progression have often astonished schoolboys; and as naturalists we also know what *ought to* happen in the case of insects, or even of mammals, if their actual increase in the least degree corresponded with their natural powers of expansion. Even the human family does not always increase as rapidly as it might. Stress is laid upon the fact that most of the available land for cattle-ranching has already been laid hold of, and that further extension of this industry has for the present received a check from which it is not likely to recover. Also the singular diminution in the numbers of sheep throughout the Old World, and the less noticed fact that since 1883 the sheep stock of the United States has lost 6,000,000, must bear upon the price of mutton sooner or later. In the United Kingdom, including islands, we had in 1867 111 sheep to every 100 inhabitants; in 1887 we have 79. In France they had in 1867 80 sheep to every 100 inhabitants, but in 1887 they only have 59. The same story is told in every European country without exception, and the sheep population of the world would have most disastrously decreased had it not been for the large increase in stocks in the Australasian colonies and the Argentine Republic.

The experiments upon ensilage are particularly worthy of attention. The process of ensilage has its devotees, who, like Prof. Rogers, consider it to be a panacea for agricultural distress. This sanguine view has been supported by the experience of many agriculturists, who have not the least doubt as to the superiority of silage over hay, and who also look upon the peculiar succulence of silage as a fact of great importance. One thing, however, appears certain—that, valuable as ensilage may be, it cannot equal in nutrient properties young growing grass. Hay or silage may be of more value in winter than is grass in summer, but intrinsically grass is that perfect product of unaided Nature which no art can better.

An optimist view thus stated may be challenged, and the propounder asked if the fresh clusters of the grape are equal to the ripened vintage wine? The matter requires to be dealt with scientifically, and it is with a view to clearing up the matter that the Royal Agricultural Society has with the aid and concurrence of the Duke of Bedford carried out a series of crucial experiments upon the value of ensilage as a stock food in comparison with the value of hay. Such an experiment is liable to many sources of error. The Wilmington experiments of 1886 abounded in them. There was no guarantee that the hay as hay was as good as the silage as silage. There was no record as to the comparative areas of land required to produce the hay or the silage. The large amount of silage eaten by the bullocks bore an unsatisfactory relation to the small quantity of hay eaten, indicating that the ensilage was good and palatable, while the hay was unpalatable. This inference is borne out by the *dictum* of the Society's chemist, that the hay at Wilmington was "very inferior indeed," while the silage "was really well made." Such sources of error invalidate the results obtained, and if, as was the case, the cattle fed on good silage did better than those fed on bad hay, all we can say is that no other result could very well have been expected. At Woburn the experiments were more strictly conditioned. "5½ acres of ground were carefully measured out, and the grass was only cut as it was wanted for carting to the silo, not being allowed to lie on the field any length of time. Two carts going side by side were filled simultaneously, and then taken to be weighed. After weighing, one cart went to the silo, into which grass was to be filled, and the other went to a meadow, where the grass was spread and left for haying." I must not take up space by explaining the complete system of sampling the grass, and the two products of hay and silage. Suffice it to say that the utmost pains was taken to obtain thoroughly representative samples for purposes of analysis. The hay and silage thus obtained might be considered as strictly comparable with one another, and if the process of silage is preferable to the older and more fragrant system of hay-making, the comparison might here be instituted with every prospect of deciding the question. The experiment was made upon twelve Hereford steers, six of which were placed on a diet of 3 lbs. of cotton cake, 5 lbs. of maize meal, with hay *ad libitum* and water *ad libitum*. The other six were given 3 lbs. of cotton cake, 5 lbs. of maize meal, with silage *ad libitum*, and water *ad libitum*. The conditions were the same except with regard to the hay and the silage. The bullocks were practically of equal size and weight, although the six bullocks which were placed on the ensilage side of the experiment had the advantage of 9 lbs. over the hay-fed lot, and weighed 60 cwt. 1 qr. 20 lbs. The result after thirty days' feeding was that the hay-fed bullocks had increased more in weight, the comparative merits of the two systems of feeding being as follows:—

Gain per day per head of bullocks receiving			hay .	2·3 lbs.
Ditto	ditto	ditto	silage .	2·1 lbs.

During the succeeding month the result was in favour of the ensilage, but in the total period of 84 days, which terminated on March 10, 1887, the result was:—

Gain of hay-fed bullocks		. .	1·96 lbs. per day.
Ditto	silage	ditto . . .	1·98 lbs. ditto.

A very curious result was arrived at with reference to the relative amounts of hay and water and of silage and water consumed during this period. The six bullocks receiving hay consumed of hay 20·3 lbs. per head per day, and drank 70·7 lbs. of water, or a total of hay and water of 91 lbs. each. The six bullocks receiving silage consumed of silage 51 lbs. and of water 40·1 lbs., or a total of 91 lbs. each. This very closely accordant result appears to point to the conclusion that the only difference between hay and silage is water, and that hay with plenty of water is quite as good a food for fattening bullocks as silage with less water.

The progress of the Hessian fly is a topic of considerable public interest, and no one could more satisfactorily enlighten us on the subject than Mr. Charles Whitehead. We are told by this excellent practical entomologist that the Hessian fly appeared first in America in 1779, and that a great scare prevailed in England at that time, which turned out to be unfounded. The nearest country to us at present affected with the pest is Russia, which appeared to first receive this unwelcome visitant in 1879, and it is still a moot point whether our Hessian flies have arrived from America or from Russia. That we have it rather bad is plain from the fact that the insect has been proved present in twenty English counties. The theatre of its operations is likely

to be extended during next summer, and we shall probably soon have the satisfaction of knowing whether our climate is suitable to its tastes. If so, it will probably obey the mandate of increasing and multiplying; but its tendency will be towards deplenishing rather than replenishing the earth. The prospect is not exactly nice, but we may take some comfort from Prof. Riley's expressed opinion that the Hessian fly will not prove a very serious plague to British agriculturists.

Downton, December 10. JOHN WRIGHTSON.

THE REPRODUCTIVE ORGANS OF ALCYONIDIUM GELATINOSUM.

IN some specimens of the Polyzoon *Alcyonidium gelatinosum* dredged last summer, I noticed that the colony, in place of being nearly homogeneous in colour and semi-translucent, as is usually the case, had a blotched appearance, caused by the presence of a number of small rounded spots of an opaque grayish-white or pale yellow colour. These average about 0·5 mm. in diameter, and are scattered irregularly through the colony. On teasing up a small part in sea-water, and on making a few rough sections of the living colony, I found that the opaque spots were cavities filled with fully developed active spermatozoa. No ova were visible in the polypides of any of the parts examined, so these colonies were evidently in the condition of sexually mature males. It at once occurred to me that this species of *Alcyonidium* might be unisexual—some colonies male and others female—the males being distinguishable when mature by their spotted appearance. The specimens were preserved for future examination.

On returning to Liverpool, and looking up the literature of the subject, I find that Hincks states ("British Marine Polyzoa," introduction, p. lxxxvi.) that "*Alcyonidium gelatinosum*, according to Kölliker, is unisexual," and I gather from the context that it is the individual polypides that are unisexual, and not the whole colony. Hincks, however, does not give a reference to any paper by Kölliker, and I have not been able to find in the literature of the Polyzoa, or in the bibliographies I have consulted, any paper of Kölliker's which would be likely to contain observations on the reproduction of *Alcyonidium*; therefore I am still uncertain how far Kölliker's remark is intended to apply—to the whole colony, or only to the individual polypides. I know of no other investigations on the subject.

I have now examined a number of thin sections, of both the spotted colonies (including the one formerly dissected) and the usual translucent ones, and I find:—

(1) In the spotted colonies there are a number of greatly distended polypides, with their cœloms filled with fully developed spermatozoa. There are also a few ordinary large, but not distended, polypides, containing each a few young ova.

(2) In the ordinary clear colonies there are neither ova nor spermatozoa to be found.

It is evident, then, that the *colony* is hermaphrodite, whatever the polypide may be. But it is also evident that the spotted colonies are virtually males. Their spermatozoa are fully developed, while their ova are still quite immature. Probably, then, *Alcyonidium gelatinosum* is, like many of the Compound Ascidians, an hermaphrodite in which the reproductive systems arrive at maturity at different times in the life-history. Most of the Compound Ascidians in which I have found this the case are proterogynous (the female organs maturing first), but *Alcyonidium gelatinosum* appears to be proterandrous. If the polypides are unisexual, then the proterandry refers only to the colony as a whole, but it is possible that each polypide may be a proterandrous hermaphrodite, developing ova after it has got rid of the spermatozoa. I hope to investigate this matter further by keeping some colonies alive at the Puffin Island Biological Station, and examining their condition from time to time.

In *Alcyonidium gelatinosum* both the ova and the spermatozoa occur in ordinary polypides, and not, as Hincks states is the case in the closely related species *A. mytili*, in "gonœcia" (cells containing no polypides). In my sections the alimentary canal and tentacles are found cut across here and there in the masses of spermatozoa. The large cavities containing the spermatozoa are evidently ordinary polypides, with the cœlom greatly distended.

W. A. HERDMAN.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 15.—"Note on the Development of Feeble Currents by purely Physical Action, and on the Oxidation under Voltaic Influences of Metals not ordinarily regarded as spontaneously oxidizable." By Dr. C. R. Alder Wright, F.R.S., and C. Thompson, F.C.S.

The authors have noticed that if two or more different kinds of aëration plates be set up on the surface of the fluid contained in a shallow basin in which the oxidizable metal is immersed, and sufficient time be allowed to elapse to enable the films of air attracted to the aëration plates to attain a condition of equilibrium, different constant values are usually obtained for the E.M.F.'s generated by opposing to the oxidizable metal first one and then the other of any given pair of aëration plates, the currents generated being rendered throughout of too small density for "running down" to take place during the observations by interposing a large resistance in the circuit. If when this state of constancy has been attained the two aëration plates be opposed to each other with a considerable resistance in circuit, a current passes from the one giving the higher value when opposed to the oxidizable plate through the external circuit to the other; this current at first is of such magnitude as to correspond exactly with the E.M.F. due to the difference between the E.M.F.'s exhibited when the two plates respectively are opposed to the oxidizable metal; but after some time it gradually diminishes; even after several days, or even weeks, however, it is usually still measurable; and if a miniature silver voltameter be included in the circuit, in many cases an appreciable amount of crystalline silver is found to be slowly deposited on the negative electrode of the voltameter, which may conveniently be a thin gold wire immersed to a depth of a few millimetres in silver-nitrate solution, a silver plate or wire forming the positive electrode. Various experiments are described in illustration.

It is obvious that during the passage of a current the dilute sulphuric acid between the two plates must be electrolysed, so that hydrogen would tend to be liberated on the surface of the plate acquiring the higher potential, and oxygen on that of the other; the hydrogen whilst nascent would necessarily be more or less completely oxidized to water by the oxygen of the film of condensed air; so that on the whole the net chemical action in the cell itself would be either *nil* (if all hydrogen were so re-oxidized) or one *absorbing* heat (if some of the hydrogen escaped oxidation). The oxygen slowly evolved would escape as such, being dissolved by the surrounding fluid. The effect of this should accordingly be that the efficiency of the air-film on the first plate would be more or less depreciated, and that on the second exalted; in point of fact, if the two aëration plates in such an arrangement which has been generating a current for some time be (by means of an appropriate switch) disconnected from one another and successively opposed to a given oxidizable plate, the one does give a considerably lower and the other usually an appreciably higher value than the constant ones previously obtained (before the two aëration plates were directly opposed to one another) on opposing each severally to the oxidizable metal; whilst on allowing the cell to stand for some time generating no current, the lower value gradually rises and the raised one falls until sensibly the old constant values are again obtained.

When *silver* plates are used in conjunction with a fluid capable of dissolving silver oxide (such as dilute sulphuric or acetic acid or ammonia solution), distinctly larger amounts of current are usually developed than with platinum or gold plates, and simultaneously silver passes into solution, the plate acquiring the lower potential diminishing in weight, and, in short, behaving precisely as though it were an oxidizable metal, such as zinc or copper. Obviously this is due to the circumstance that with silver the ion liberated attacks the metal of the plate acquiring the lower potential; but the remarkable part of the action is that this attack is only partial, so that the amount of silver dissolved is invariably *less than that equivalent to the current passing*, *i.e.* less than that deposited in a silver voltameter included in the circuit.

Various illustrative experiments are described which show that the difference between the silver dissolved and that deposited by the current is relatively much larger with the weakest currents.

It is obvious that if silver will dissolve in acids, &c., under the comparatively feeble oxidizing influence of an aëration plate,

much more rapid solution might be anticipated by substituting for such a plate platinum immersed in a powerfully oxidizing fluid such as strong nitric acid, or sulphuric acid solution of chromic anhydride. In point of fact, the authors have found that on setting up such cells where the silver was immersed in dilute sulphuric acid (*i.e.* Grove's cell with silver instead of zinc, and so on), electromotors of notable power are produced, at any rate until the silver plate becomes coated with sparingly soluble sulphate. Even in these cases, however, perfect correspondence between the amount of silver dissolved and that deposited in a voltameter included in the circuit does not subsist, the latter being always measurably the greater.

Just as silver is capable of being dissolved in an appropriate fluid when opposed to an aëration plate, so may several other metals not ordinarily prone to atmospheric oxidation; thus mercury with dilute sulphuric acid as fluid, and an aëration plate of platinum sponge, generates a measurable continuous current, forming *mercurous sulphate* in so doing, so that after some time the liquid becomes turbid through separation of that sparingly soluble salt, and the filtered fluid precipitates calomel on addition of dilute hydrochloric acid. Acetic acid acts similarly, but far less energetically. Potassium cyanide solution, on the other hand, causes a much more rapid solution of mercury, forming *mercuric potassiocyanide;* it is noticeable that in this case only 100 parts of mercury go into solution for 108 of silver deposited in the voltameter, whereas when sulphuric acid is used 200 parts of mercury become sulphate per 108 of silver deposited.

If gold be substituted for mercury in this latter arrangement, rapid solution takes place with formation of *aurocyanide of potassium*, 196 parts of gold being dissolved per 108 of silver thrown down in the voltameter. Palladium behaves precisely as gold, 52 parts of metal being dissolved per 108 of silver deposited; local action sometimes causes in each case a slight *excess* of amount dissolved relatively to the current passing, the opposite result to that observed with the silver cells above described.

Of course, if more powerful oxidizing agents are used than simple aëration plates (such as platinum in sulphuric-chromic solution) the action goes on in all such cases still more rapidly.

"On the Functions of the Occipital and Temporal Lobes of the Monkey's Brain." By Dr. Sanger Brown and Prof. E. A. Schäfer, F.R.S.

The authors gave an account of experiments upon the brain of monkeys, involving the removal of the occipital and temporal lobes respectively. These experiments show that removal of the whole of one occipital lobe produces permanent hemiopia, and that removal of both occipital lobes produces complete and permanent blindness of both eyes; and, further, that for the production of these effects it is not necessary that the angular gyrus should be involved in the lesion.

They also show that not only the superior temporal gyrus but even the whole temporo-sphenoidal lobe can be removed on both sides of the brain in monkeys without producing any appreciable permanent effect on hearing.

The reading of the paper was illustrated by diagrams exhibiting the extent of the lesions, as well as by casts of the brains.

Royal Meteorological Society, December 21.—Mr. W. Ellis, President, in the chair.—The following papers were read:—The mean temperature of the air at Greenwich, from September 1811 to June 1856, by Mr. H. S. Eaton. This is a discussion of the meteorological journals of the late Mr. J. H. Belville, and those of the Royal Observatory. The general results of this investigation are:—(1) That there was no appreciable change in the mean annual temperature of the air at Greenwich in the period 1812 to 1855 inclusive. (2) That on the eminence on which the Royal Observatory is situated the average temperature at night, or rather the early morning, is in all cases higher than over the lower grounds. (3) That with a north-wall, or possibly a north-window exposure, higher maximum temperatures are found at the lower stations. (4) That the movements of the thermometer are retarded with a north-wall exposure as compared with an instrument on an open stand, especially where the situation is a confined one, the indications of the thermometer not following changes of temperature so promptly owing to the modifying influence of the adjacent building.—Report on the phenological observations for the year 1887, by the Rev. T. A. Preston. The past season was a most exceptional one. For flowers it was disastrous; fruit was generally a failure, though there were exceptions; those kinds which promised well turned out very small or spoilt by insects. Vegetables were universally poor, roots were destroyed by insects or drought, and green crops soon passed off. The wheat crop, however, was better than was expected. Barley on light lands was poor, but that which was sown early was satisfactory. Meadow hay was not up to an average crop, but clover and seed hay were much more nearly so. In Kent the fruit crops turned out lighter than usual, but the prices have ruled higher.—Earth tremors and the wind, by Prof. John Milne, F.R.S. The author has made a detailed examination of the tremor records obtained in Tokio, and compared them with the tri-daily weather maps issued by the Imperial Goverment of Japan. From this comparison the following conclusions have been drawn:—(1) Earth tremors are more frequent with a low barometer than with a high barometer. (2) With a high barometric gradient tremors are almost always observed, but when the gradient is small it is seldom that tremors are visible. (3) The stronger the wind the more likely it is that tremors should be observed. (4) When there has been a strong wind and no tremors the wind has usually been local, of short duration, or else blowing inland from the ocean. (5) When there has been little or no wind in Tokio and yet tremors have been observed, in most cases there has been a strong wind in other parts of Central Japan. (6) From 75 to 80 per cent. of the tremors observed in Tokio may be accounted for on the supposition that they have been produced either by local or distant winds. (7) The only connection between earth tremors and earthquakes in Central Japan is that they are both more frequent about the same season.—Pressure and temperature in cyclones and anticyclones, by Prof. H. A. Hazen. The author has made a comparison of the observations at Burlington and on the summit of Mount Washington, U.S.A., and as the result of a study of about 4000 observations from two days before till two days after the passage of cyclone and anticyclone centres, he has arrived at the following conclusions:—(1) In both cyclones and anticyclones the pressure lags from 10 to 11 hours at the summit of Mount Washington. (2) The temperature change at the base precedes very slightly the pressure change, but at the summit the change occurs nearly 24 hours earlier. (3) The temperature appears to be a very little earlier at the summit than at the base, and certainly varies much more rapidly at the former. (4) In a cyclone the difference in temperature between base and summit is less than the mean before the storm, but the difference rapidly increases after the centre has passed. Just the contrary is true in an anticyclone. (5) The total fall in pressure in a cyclone at the summit very nearly equals that at the base, and likewise the rise in an anticyclone. (6) The fluctuation of temperature—that is, from the highest to the lowest—at the summit is double that at the base in a cyclone; but it is only a little greater in an anticyclone.

EDINBURGH.

Royal Society, December 5.—The Hon. Lord Maclaren, Vice-President, in the chair.—After reading an opening address, the Chairman presented the Victoria Jubilee Prize to Sir W. Thomson, for his contributions to the Society's publications on various subjects in hydrokinetics.—Sir W. Thomson read a paper on Cauchy's and Green's doctrine of extraneous pressure to account for Fresnel's wave-surface. The object of his investigation was to place Green's treatment of the subject on a more satisfactory basis than it had been left by its author.—Sir W. Thomson also exhibited models of the minimal tetrakaidekahedron, a figure which he discusses in the *Philosophical Magazine* for this month.—The second part of a paper on micro-organisms, by Dr. A. B. Griffiths, was communicated by Prof. Crum-Brown.—Prof. Wallace laid on the table a paper on the blackening of the skin of domesticated animals in tropical regions.

PARIS.

Academy of Sciences, December 19.—M. Janssen in the chair.—Generation of algebraic surfaces of any order, by M. de Jonquières. The theorem here demonstrated supplies a fresh instance of the intimate and essential part played by the properties of numbers in several questions of general geometry, and especially in those concerned with the generation of surfaces and curves, as well as with the number of double and multiple points with which the latter may be endowed.—Reply to M. Wolf's communication entitled, "Comparaison des divers systèmes de synchronisation des horloges astronomiques," by

M. A. Cornu. The regulating apparatus introduced into his system of synchronizing clocks by M. Cornu, and objected to by M. Wolf as useless and even inconvenient, is shown to be free from these drawbacks, and in fact indispensable for strict accuracy. To these remarks M. Wolf replies that the system at work at Greenwich for twenty-seven and in Paris for seventeen years dispenses altogether with any such arrangement as that proposed by M. Cornu.—On the cause of the deviation of the arrows indicating the direction of the wind on synoptical charts of cyclones, by M. Faye. This deviation is traced entirely to the friction or resistance of the ground over which the cyclone is moving, and harmonizes in no way with the erroneous hypothesis of ascending cyclones. It is greater on land than at sea, and imperceptible in the case of waterspouts and true tornadoes. It also diminishes with the distance from the centre of the cyclone, disappearing altogether near the central calm.—On the state of the sulphur and phosphorus present in plants, in the ground, and in cultivated soil, and on their quantitative analysis, by MM. Berthelot and André. Having already studied the relations of potassium and nitrogen to the vegetative functions, the authors here deal in the same way with sulphur and phosphorus. The question is treated especially with a view to determining and analyzing the complementary manures best suited for restoring the fertility of exhausted lands.—Note, by M. Albert Gaudry, on the discovery of a gigantic turtle by Dr. Donnezan. This specimen was found, with numerous other fossils, in the Middle Pliocene of Perpignan during the recent excavations connected with the erection of the fortress of Serrat in the Eastern Pyrenees. The carapace, 1·20 metre long, was extracted with great difficulty from the hard rock in which it was completely embedded, the innumerable fragments being carefully put together by Dr. Donnezan, by means of about a thousand brackets. This turtle, which he has named *Testudo perpiniana*, and which he has presented to the Paris Museum, considerably exceeds its living congeners, being equal in size to the *T. grandidier*, a sub-fossil species found in Madagascar. Its survival down to the close of the Middle Pliocene is important for the study of the Glacial period, tending to show that the south of France even then still enjoyed a warm climate.—Experiments with a new hydraulic machine employed for irrigating-purposes, by M. A. de Caligny. By means of this apparatus, which is a modified form of that described by the author in the *Comptes rendus* for December 18, 1882, water with a normal fall of 2·40 metres may be raised to a height of 9·45 metres above the level of the upper stream.—On the degrees of oxidation of chromium and manganese in their fluorescent compounds, by M. Lecoq de Boisbaudran. With a view to solving this question the author describes certain experiments which he has made chiefly with alumina and chromium, gallina and chromium, magnesia and chromium, alumina, potassa, and manganese, lime and manganese; confining himself for the present to a statement of the facts observed.—Elements and ephemeris of the planet Anahita, 270, by M. E. Viennet. By means of the ephemeris deduced from the already published provisional elements the author has been enabled to compare all the observations made down to November 16, and thus determine six normal places for October 12, 15, 18, 21, 27, and November 16. With these fresh elements an ephemeris has been calculated, by which astronomers will be enabled to observe the planet down to the end of the present opposition. The magnitude should then be about 11 or 12.—On the value of the solar parallax deduced from the observations taken by the Brazilian Missions during the transit of Venus in 1882, by M. Cruls. From the reports of the observations made at the three stations of St. Thomas (West Indies), Olinda (Brazil), and Punta-Arenas (Strait of Magellan), the horizontal equatorial parallax of the sun at its mean distance from the earth is found to be $8''\cdot848 - 0''\cdot040 = 8''\cdot808$. The reports are now nearly printed, and copies may soon be expected in Europe.—On the specific heat of tellurium, by M. Ch. Fabre. These experiments show that under its several forms tellurium possesses much about the same specific heat, at least at a temperature of 100° C. or thereabouts. But the differences may possibly increase at higher temperatures, and especially near the point of transformation from amorphous to crystallized tellurium.—Study of a specimen of Welsh coal, by MM. Scheurer-Kestner and Meunier-Dolfus. This was a piece of the so-called "Nixom's Navigation," from Glamorgan, which the authors undertook to examine for Mr. Donkin, and which was found to be so pure that it yielded 88 per cent. of hard bright coke, 4·39 of hydrogen, and not more than ·69 of sulphur.—On sidereal evolution, by M. Ch. V. Zenger.

Berlin.

Physiological Society, December 2.—Prof. du Bois Reymond, President, in the chair.—Dr. Salomon spoke on the physiological action of paraxanthin. Since Fisher's researches have thrown light on the chemical constitution of caffein and theobromin, and shown that the former is trimethyl-xanthin, the latter dimethyl-xanthin, experiments on the physiological action of caffein, theobromin, and xanthin have acquired an increased interest. All these substances produce a double effect when given to a frog—namely, one on the central nervous system, and a curious effect on the muscles, which pass into rigor; the three substances exhibit these properties in graduated degree, a fact which is sufficiently explained by the close relationship of their chemical constitution. It hence appeared to the speaker to be a matter of some importance to investigate the physiological action of the two xanthin-derivatives which he had found in urine—namely, paraxanthin and heteroxanthin. From his researches it appears probable that paraxanthin was also a dimethyl-xanthin, that is, an isomer of theobromin; heteroxanthin, on the other hand, had only been obtained in such minute quantities that its chemical constitution could not be determined, but Dr. Salomon suggested that it might be the missing member in the above series of xanthin-derivatives—namely, monomethyl-xanthin. When the somewhat insoluble paraxanthin was administered locally by subcutaneous injection, it produced a stiffness and rigor of the neighbouring muscles: when given in larger doses, some of the animals became sluggish and died, but in many cases they remained uninjured. When given internally, paraxanthin rarely led to any appearance of poisoning, but, when it did, the effect was limited to a stiffening of the fore-limbs and a general sluggishness of the whole animal. Paraxanthin, therefore, exhibited a physiological action analogous to that of the other xanthin-derivatives. Paraxanthin also possesses a distinct action on the respiratory apparatus, since, in all cases in which any effect was produced, the lungs were found to be strongly inflated. He was unable to examine the action of heteroxanthin, from the smallness of the quantity in which it can be obtained.—Dr. Baginski demonstrated the reducing action of certain Bacteria, using, as a reagent, methylene-blue, which becomes colourless by reduction. The Bacteria were obtained from the intestines of healthy cows. Both *Bacterium lactis* and *Bacterium coli* produced a powerful reducing action in pure cultivations, where the nutrient fluid was coloured with methylene-blue; in those places where oxygen had access the blue colour reappeared. A third Bacterium discovered by the speaker exhibited no reducing power.—Prof. Gad explained, on behalf of Mr. Donaldson, the method introduced by Prof. Martin, of the Johns Hopkins University, Baltimore, of isolating the mammalian heart, and of making observations on its activity for several hours, when isolated from the body and connected only with the lungs. The defibrinated blood which flows from the aorta passes into two Mariotte flasks which are in communication with each other, and thence into the right auricle. By means of this arrangement it can be shown that the heart, when separated from all its nerves, works quicker when the temperature of the blood is raised, and slower when it is lowered. An increase of pressure in the aorta was found to be without any effect, whereas an increased venous resistance increases the cardiac activity. It could not be shown that the heart exerts any suctional action during its diastole.

Meteorological Society, December 6.—Prof. von Bezold, President, in the chair.—The President drew attention to Prof. Hann's two most recent publications, namely, the "Atlas of Meteorology" and "The Barometric Pressure in Middle and South Europe according to Observations extending over Thirty Years," and gave a short account of their contents.—Dr. Assmann gave an account of the experiments he has carried on during the last year and a half with a view to determining the true temperature and humidity of the air. After describing the methods previously used to determine the true temperature of the air and his own unsuccessful attempts before he arrived at a satisfactory result, he explained the principle of the thermometers as finally employed, and demonstrated the same by exhibiting several of them. These instruments consist of a fine sensitive mercurial thermometer, of which the small bulb is surrounded by a highly burnished cylinder of nickel-plated brass, open at the lower end.

At the upper end the brass cylinder has a lateral opening by which its interior can be connected with an india-rubber aspirating ball. The chief difficulty met with was in the construction of a suitable valve for the aspirating ball. Finally he succeeded in making a valve such that no air was ever driven back towards the thermometer when the ball was compressed, but only drawn over the bulb of the instrument during the aspiration at the rate of ·2 to 2·5 metres per second. Within these limits the rate at which the air is drawn over the bulb had no influence on the temperature recorded by the thermometer. Of extreme importance, as showing the suitability of the instruments, were the speaker's observations on the temperatures recorded by two of his thermometers, of which one was exposed to the direct rays of the sun, while the other was shaded by a distant shutter: the two thermometers recorded the same temperature, while at the same time an actinometer exposed to the sun showed a temperature 17° C. higher. The same exactness in the determination of the humidity of the air is obtained when a pair of these thermometers is used, and the bulb of one is wrapped round with a piece of moist cloth. This instrument is specially suitable for observations in a balloon. The speaker explained that only shortly before the present meeting he had found that a similar instrument had been constructed by Welsh about the year 1850.—Dr. Robert von Helmholtz gave an account of experiments which he had carried on conjointly with Dr. Sprung with a view to determining the humidity of the air. They had both arrived, independently of each other, at the idea that the determination of the dew-point might best be made, not, as in the usual way, by the condensation on the bulb of a thermometer, but by measurement of the amount of rarefaction which the air must undergo in order that a mist may be produced. In a previous research the speaker, when determining the vapour-tension over solutions of salts, had compressed the air in a closed space, and then obtained a formation of mist by suddenly reducing the pressure again to that of the atmosphere. By determining the general excess of pressure which is thus requisite, the dew-point may be determined. Dr. Sprung has compared the dew-point as thus determined and as obtained by Regnault's apparatus. The experiments are not yet carried sufficiently far to yield any numerical results, but even now it may be said that this new method of determining the dew-point is extremely trustworthy.

Physical Society, December 9.—Prof. du Bois Reymond, President, in the chair.—Dr. Badde developed the mathematical formulæ by means of which he can determine the vibrational condition not only of a vibrating string, but also of a square plate—formulæ which make it possible to determine the relation between the pitch of the note and the vibration-amplitude of the vibrating plate.—Dr. Pringsheim gave an account of the experiments he has made, in conjunction with Dr. Summer, to determine the quotient (k) of the specific heat of gases. The value of k is determined either by measuring the rate of propagation of sound in gases which obey Mariotte's law, or else from the ratio of temperature to pressure when the volume is kept constant. Up to the present time the rate of transmission of sound has not been so exactly determined that the values can be used for deducing the value of k. Similarly the second method has as yet given very discordant results, while at the same time the experiments have not been free from errors. Drs. Pringsheim and Summer have compressed air in a glass balloon whose capacity was sixty litres, and determined its temperature by means of a fine silver wire passing through it whose electrical resistance was known. Hereupon the pressure in the balloon was allowed to sink to that of the atmosphere by opening a tap leading into it, and the cooling thus produced measured by means of the wire. Immediately upon this the tap was again closed, the air becoming warmed by the heat which passed into it from the air surrounding the balloon, and the rise of temperature again measured. During these experiments it was found to be of no consequence whether the rarefaction of the compressed air took place rapidly through a tap with a large bore, or through one with a narrow aperture; the wire always showed the same amount of cooling, thus proving that it follows the alteration of temperature of the air very rapidly. Similarly the length of the wire was found to have no effect on the results, thus showing that the temperature of the surroundings has no influence on the temperature recorded by the wire. The resistance of the wire was determined by the bridge-method, partly by means of a galvanometer, partly by means of a telephone. The ratios of the alterations in resistance of the wire to alterations of temperature were determined, within the necessary limits, for several fine wires. The speakers considered that the only objection which can be raised to their experiments is that the above determination was not made with the same wires which were used in their experiments, and they propose to do away with even this objection by some later experiments which have not as yet been carried out. All other possible objections have been set aside by varying the conditions of their work while obtaining constant results. As a mean of the separate measurements they obtained as a value for k the number 1·384; the deviation for the mean value amounted only to a few hundredths per cent. The above value for k cannot however be taken as being absolute until it has been proved that there is a proportionality between the temperature and resistance of the silver wire which they used in their experiments.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Ferrets and Ferreting, 2nd edition (U. Gill).—Massachusetts Institute of Technology: 23rd Annual Catalogue of the Officers and Students, &c. (Boston).—Die Theekultur in British-Ost-Indien; Hist. Naturwissenschaftlich und Statistich (Prag).—Quarterly Journal of the Royal Meteorological Society, October (Stanford).—Annalen der Physik und Chemie, 1887, No. 12 (Leipzig).—Archives Italiennes de Biologie, tome ix. fasc. 1 (Turin).—Journal of the Royal Microscopical Society, December (Williams and Norgate).—Elementary Text-book of Physiography: W. Mawer (Marshall).—Management of Accumulators, 3rd edition: Sir D. Salomons (Whittaker).—Sewage Treatment, Purification and Utilization: J. W. Slater (Whittaker).—Flour Manufacture: F. Kick, translated by H. N. P. Powles (Lockwood).—Photography Simplified, 3rd edition (Mawson and Swan).—Transactions of the Sanitary Institute of Great Britain, vol. viii. (Stanford).—A Treatise on Chemistry, vol. iii. Part 4, Organic Chemistry: Roscoe and Schorlemmer (Macmillan).—Present Religion, Part 2: S. S. Hennell (Trübner).—Die Altchristliche Fresko und Mosaik-Malerei: Dr. O. Pohl (Leipzig).—Recherches sur l'Isolement du Fluor: H. Moissan (Gauthier-Villars, Paris).—Journal of Physiology, vol. viii. No. 6 (Cambridge).—Morphologisches Jahrbuch, xiii. Band, 2 Heft (Williams and Norgate).

CONTENTS.

THURSDAY, JANUARY 5, 1888.

ELECTRICITY FOR PUBLIC SCHOOLS AND COLLEGES.

Electricity for Public Schools and Colleges. By W. Larden, M.A. (London: Longmans, Green, and Co., 1887.)

THIS is a book which possesses many good points, but which becomes, on close acquaintance, painfully disappointing, and even irritating, to the reader. The author has undoubtedly spared no pains to make it full of information; but its very fullness becomes bewildering, owing to the way in which the material is cut up and put together. One might almost imagine that it had been reduced from a much larger work, chiefly by means of deletions, and without the rounding-off of the angularities which such a process would inevitably develop.

The science of electricity and magnetism is, without question, an experimental science; and the author of the present work does not offer his book to his readers as a book on the mathematical side of this experimental science; but as an elementary book suitable for higher schools and for Colleges. He confines himself as to mathematics by assuming "no more mathematical knowledge than is usually possessed by the higher boys in a classical school." Under these conditions we should expect to have a book containing exact and well-finished descriptions of experiments and apparatus, along with explanations of phenomena and with theory brought down to correspond with our present knowledge on this most fascinating subject. Expectations or hopes such as these are very far from being realized in the book before us.

The amount of material collected by the author is undoubtedly very great. The number of instruments and machines referred to and described is enormous. The descriptions are, however, often unsatisfactory, nor are they written with any attempt at finish or good taste The book is supposed to be for the use of well-educated beginners; but we think it would be difficult to find a worse model for boys or young men as to the writing of descriptions of apparatus or of experiments. Often the heading of a paragraph has half the duty to perform; the remainder may be done by a diagram, which is lettered in a tantalizing way, as if a description had been intended.

Here, for example, are the descriptions of two of the most important frictional electric machinés:—

"II. *The Common Plate Machine.*—In this there is nothing essentially different from the cylinder machine. A glance at the figure will explain all. There are generally two rubbers; and in this form of machine they cannot well be insulated, if required; so the machine cannot be used as a source of both + and − electricity. Instead of glass, ebonite plates may be used, the rubbers being of amalgamated silk.

"III. *Winter's Plate Machine.*—In this the rubber and the points of the prime conductor are more widely separated; and the prime conductor can therefore acquire a higher level (or potential) of charge without discharge over the glass to the rubber. The rubber can be insulated or not, as required. A curious feature is an addition to the prime conductor in the shape of a large ring of brass inclosed in baked wood. This ring increases the 'capacity' of the prime conductor."

The description of Winter's machine is not even supplemented with a figure; and we doubt if any student reading the description will form the faintest conception of the nature of the machine or of the "ring of brass inclosed in baked wood." "Amalgamated silk," too, is a shortened expression, which is, to say the least, as inappropriate as it is uncommon. These descriptions have not been specially chosen for inadequacy. There are numbers no more complete than these.

Probably the chapter which will be found most satisfactor by learners is the long and important Chapter X., which deals with electro-static potential. This, with the exception of the first two or three sections, is very complete and well given. The subject is explained with great clearness, and with abundant reference to numerical calculation.

The chapters on dynamo-electric machines and on motors may also be considered fairly good for an elementary text-book. The learner will obtain in these chapters a sufficient account of the principles of these machines, given with satisfactory clearness.

In his treatment of the subject of units, and particularly of the electro-magnetic units, the author is singularly unhappy. In an elementary book, or in any book on this subject, whether elementary or advanced, it must be considered a fundamental mistake to omit a full and clear explanation of the foundation and derivation of the absolute electro-magnetic unit of resistance; and it is utterly unsatisfactory to give, as a definition of the unit of resistance, the remark merely that "Ohm's law defines the unit of resistance as that through which unit electromotive force gives unit current." It was not in this way that the absolute unit of resistance was fixed upon, and the original definition is certainly worthy of the attention of the student. Taking the statement given above, however, and turning to "Ohm's law" for information, the learner finds no statement of this law in words, but merely the following:—

"Ohm's law is that—

$$C \text{ is proportional to } \frac{E}{R},$$

or

$$C = k\frac{E}{R}."$$

Such a statement as this might perhaps, if reproduced for the benefit of an examiner, serve to conceal the ignorance of the individual under examination, and might leave the examiner so uncertain that he would be obliged, though unwillingly, to award half marks to the answer; but to the student it can do no real good.

There is but one other remark on the electro-magnetic unit of resistance, and it is almost equally infelicitous with what is quoted above. It is contained in a "note" on "Determination of Units," and is to the effect that "resistance can be measured by observation of the heat evolved when a known current flows through the conductor in question." A very slight acquaintance with possibilities in experimenting would dispel any such idea.

In connection with explanations regarding units we meet here the customary sections on "Dimensions of

Units," but it seems to us that too much importance is commonly given to the well-known table of dimensions. Without very full and very clear explanations of the whole subject (and these are not to be found in the book before us), the table and the remarks given in connection with it are worse than useless; they only serve to confuse an intelligent pupil whose own common-sense will carry him safely through any calculation which he can be reasonably expected to make.

We cannot avoid calling attention, before closing this notice, to some most painful defects in style, because it is, we are of opinion, of the utmost importance that students should be trained from the very beginning to write and speak with due respect to ordinary proprieties of literary composition. It is not to the faults of English grammar and to uncouthness of language that we would call attention most particularly, though such faults abound. Thus, on p. 16 the student is recommended to "puzzle this out"; and on p. 72 we find the following sentence:—

"There *will be* a fall of potential, or an electrical hill, from this body A down to the walls, &c. (*sic*). Such *was* the case if only we change the sign of the charge in Chapter IV. § 14."

On p. 248 we find the following:—

"*Failure of a Smee's Cell to decompose Water.*—If the back E.M.F. *e* of an electrolytic cell *would be* greater than the *E.M.F.* E of the battery, then such a battery *will* fail to drive a current through, and decompose, such a cell."

The worst fault in style is, however, the introduction, on every page of the book, often in every line, of contractions of all sorts. Thus we have, through the whole of the electro-static part, + and − for "positive" and "negative"; and we have bodies "+ly electrified" and "−ly electrified." Throughout the electro-chemical part we scarcely once have hydrogen, or zinc, or sulphuric acid mentioned by name, but always H, Zn, and H_2SO_4. This becomes confusing, to say the least, when the author is dealing with the "*Connection between E.M.F.'s and 'Heats of Combination.'*" Here, "if we represent by by H_{Zn} the heat in calories evolved by the solution of 1 gramme of Zn in dilute H_2SO_4, then H_{Zn} is called the heat of combination of zinc with dilute H_2SO_4."

Worse, perhaps, is the use of "ΔV" and "the algebraic sum of the different ΔV's" where "ΔV" stands for the words "difference of potentials." This is done everywhere throughout the first part of the book; and by and by, when we are introduced to ΔV's producing E.M.F.'s, and to "Thomson and Peltier E.M.F.'s," human patience absolutely fails. In the list of abbreviations we are told that a second of time is denoted by 1″ "sometimes." It ought to be *never*.

It is scarcely necessary to say that, were there no merits in the book before us, it would hardly be worth while to enter into a discussion of its faults. But the work is really of great value, and were the materials somewhat rearranged, the writing improved, nine-tenths of the paragraph breaks taken away, and the multitude of notes incorporated with the text, it would prove a most important text-book in electricity and magnetism.

INDO-CHINA AND THE INDIAN ARCHIPELAGO.

Miscellaneous Papers relating to Indo-China and the Indian Archipelago. Reprinted for the Straits Branch of the Royal Asiatic Society. Second Series. Two Volumes. (London: Trübner and Co., 1887.)

THE new series of papers relating to Indo-China, like its predecessor, consists of reprints from various periodicals which are not within the reach of ordinary readers. Thus in the present volumes we find papers of great interest, and some of considerable importance, reproduced from the Journal of the Royal Geographical Society of forty years ago, from the Journals of the Asiatic Society of Bengal, of the Royal Asiatic Society, and from the publications of various Dutch Societies. With regard to the latter, it may be said that they are the most valuable papers in the volumes, for the Dutch have long studied with great assiduity the land and people under their rule in the Malay Archipelago. Their scientific services, in Java especially, are recruited from Holland with the utmost care; the members are spread over the scattered Dutch possessions from Northern Sumatra to New Guinea; they are constantly studying the problems presented to them by man and Nature around them; and the consequence is that the *Verhandelingen* of the Society of Arts and Sciences at Batavia, the *Indische Tijdschrift*, and other publications in the mother country, as well as in Java, are full of papers written by skilled and qualified persons who have devoted special attention to subjects connected with the Malay Archipelago. The editor of these volumes is indebted to these Dutch publications for such papers as that on the rocks of Pulo Ubin, by Mr. J. R. Logan, the greatest English student of this region that ever lived, although there are certain members of the Straits Civil Service who promise to rival him; for Mr. Groeneveldt's "Notes on the Malay Archipelago and Malacca," a modest title under which is concealed a learned examination of a vast quantity of Chinese literature with a view to ascertaining what the Chinese knew about the region; Father Borie's account of the Mantra tribe, and several others.

The experiment of collecting in this way from various sources the papers relating to a particular region is, we believe, a novel one. In this instance it appears to be a success. Here in four volumes, obtainable at a moderate price, we have the contents—so far as they relate to the Malay Peninsula and Archipelago, and appear to a skilled editor to be of permanent value—of more than a score of periodicals, many of which are quite inaccessible to ordinary students, and which, even in London, could only be examined in the British Museum, the India Office, and possibly the Royal Asiatic and Royal Geographical Societies. The Council of the Straits Society, which advanced the funds for this excellent undertaking, is to be congratulated on its public spirit, and we trust it will not lose, even in a pecuniary sense. Whether it does or not, it has placed every student of the region within which its members labour under an obligation by the production of these volumes. Other learned Societies in various parts of the globe might well emulate this example, for there is nothing more laborious or bewildering than to hunt through old periodicals without adequate indexes

and without an exact reference, for a valuable paper. The number of papers of permanent value in these old periodicals is very small: subsequent researches have thrown them out of date; the mere efflux of time has proved some of them to be useless; many deal with temporary subjects, which are now of no importance to anyone. Such a periodical, for example, as the old *China Repository*, printed partly in Canton—the Canton of the old days—now fetches an absurd price. Sets have been sold in recent years at from £30 to £50; yet all that it contains of value now could be placed in two volumes such as these before us. The demand for special works of this character, however, is too small to induce any publisher to incur the risk of producing them; and hence it is that we are thrown back on the learned Societies, which represent the students of to-day, to place within our reach the labours of past generations of scholars, and of the literary and intellectual fathers that begat them. This, however, is a question for the Societies themselves, for their own members must feel more acutely than anyone else the truth of these observations.

We have already mentioned a few of the papers of scientific interest in the present series. If Mr. Logan's paper on the peculiar rocks of Pulo Ubin, an island near Singapore, is not out of date at present, it probably soon will be if the long-promised survey of the part of the Malay Peninsula under British influence is to be thoroughly carried out. Dr. F. Stoliczka has a short paper on some species of Malayan Amphibia and Reptilia, and a longer one on the land-shells of Penang; while Father Borie describes the Mantras, amongst whom he laboured as a missionary for some years. This is one of the aboriginal tribes of the peninsula, which were driven inland by the great Malay invasion of the twelfth century. Of these, the Karens inhabit the north and part of Burmah, the Semangs the States of Kedah, Perak, and Selangore, the Mantras the region lying between the latter territory and Mount Ophir, the Jakons and Sambinbangs the southern part of the peninsula. The writer describes the manners and habits of the people in some detail. A most interesting paper, and one of the longest, is Dr. Friederich's account of the language, literature, religion, and castes of the people of Bali, an island which occupies a peculiar relation in the history of the civilization of the Malay Peninsula and Archipelago. The editor in his introduction describes that position in these words:—

"The continued existence, in unabated vitality, of a nationalized Hinduism, blended with pre-Hindu customs and practices, among a spirited and vigorous people is not only . . . a kind of commentary on the ancient condition of the natives of Java, it allows us also to draw a fair inference as to the kind of Hinduism at one time prevailing in other parts of Malaysia less favoured by historical records, where ruthless Islam has since obliterated to a great extent the traces of other creeds, traditions, and institutions. It is, indeed, essential to a proper understanding and estimate of the religious and social condition of the various and wide-spread Malayan tribes that the influence which Hindu civilization has, in a greater or lesser degree, exerted upon them, should as far as possible be investigated."

It should be mentioned that the last number of the Proceedings of the Dutch Geographical Society contains a paper on the same subject by Count Limburg Sturm, who visited the island last year.

Finally, there are certain "Notices on Zoological Subjects," and "Descriptions of Malayan Plants," reprinted from an English periodical published at Bencoolen nearly seventy years ago, with a note to the letter by Sir Joseph Hooker and Mr. Hervey, correcting the terminology. In the preface the editor quotes part of a letter from Sir Joseph pointing to a speedy investigation of the flora of the Malay Peninsula, for which he has urged the Colonial Government to contribute funds. Seeing that Dr. Rost has had to go back to 1820 for an account of the flora, it seems almost time that Sir Joseph Hooker's advice should be taken by Sir Cecil Smith and the Legislative Council of the Straits Settlements.

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873–76, *under the command of Capt. George S. Nares, R.N., F.R.S., and the late Capt. F. T. Thomson, R.N.* Prepared under the superintendence of the late Sir C. Wyville Thomson, Knt., F.R.S., &c., and now of John Murray, one of the Naturalists of the Expedition. Zoology—Vol. XXII. (Published by Order of Her Majesty's Government, 1887.)

VOLUME XXII. contains the Report, by Dr. Günther, Keeper of the Department of Zoology in the British Museum, on the deep-sea fishes collected during the cruise.

Originally it was intended to fix an arbitrary depth as distinguishing between the shore and deep-sea fishes, and accordingly, in the author's previous Report on the shore fishes of the *Challenger* Expedition, all those fishes captured at a less depth than 350 fathoms were treated as more or less littoral forms. However, the subsequent Norwegian and North American explorations brought to light instances of fishes with an unmistakably bathybial organization occurring at a much shallower depth than the forms discovered by the *Challenger*; or, on the other hand, showed that certain littoral forms descend not only to 100 but even to beyond 300 fathoms.

In the present Report, the 100-fathom line is adopted as the boundary at which, with the extinction of sunlight, the bathybial fauna commences, sporadically, no doubt, and largely mixed with surface forms.

The material which forms the subject of this Report consisted of 794 specimens, of which 610 were obtained during the voyage of the *Challenger*, 88 on the cruises of the *Knight-Errant* and *Triton*, and 96 from other sources. These specimens are referred to 266 species, 177 falling to the share of the *Challenger*, and 14 being due to the exploration of the Faroe Channel. The number of new species discovered by the *Challenger* amounts to 144, whilst by the deep-sea exploration of the Faroe Channel 10 species have been added to the fauna of the British seas.

In the introduction we have a history of our present knowledge of the fish-fauna of the deep-sea, some account of the characteristics of deep-sea fish, and an account of their vertical and horizontal distribution.

While no distinct bathymetrical zones, characterized by peculiar forms, can be defined, yet the following table clearly shows that the abundance of fish life decreases with the depth. There have been found between

Fathoms.	Species.
100–300	232
300–500	142
500–700	76
700–1500	56
1500–2000	24
2000–2900	23

While no doubt this decrease in numbers is partly due to the extreme difficulty of investigating the deep-sea fauna, it cannot but be also regarded as pretty certain that, while locally abundant as to individuals, the number of species found is but small.

The descriptions of the new genera and species, though abounding with interesting details in reference to the many strange forms described, cannot with the space at our disposal be even summarized,—they are such as would have been expected from the known skill and judgment of Dr. Günther; but we must find room for some allusion to the Report on the structure of the phosphorescent organs, on the head of Ipnops, by Prof. H. N. Moseley, and on the structure of the phosphorescent organs of fishes, by Dr. R. von Lendenfeld.

In *Ipnops murrayi* the eyes as well as the optic nerves are completely absent, but a pair of symmetrical luminous organs are to be found on either side of the median line of the upper flattened surface of its head, the upper wall of the skull where it covers them being completely transparent.

These phosphorescent organs are composed of hexagonal columnar masses, arranged with considerable regularity in rows, and resting inferiorly on a pigmented connective-tissue layer. Each hexagonal column is composed of a number (from thirty to forty) of transparent rods, disposed side by side at right angles to the outer surface of the organ, and with their bases applied against the concave surface of large hexagonal pigment cells, one of which forms the base of each hexagonal column. The basal pigment cells are also hexagonal in outline, and are cup-like, concavo-convex in form, and of the same breadth as the hexagonal columns. These organs receive a rich blood supply, and there appears little room for doubt but that the nerve supply comes from the fifth nerve. No trace of any other nerve supply has been found. From a comparison of these organs with those of a similar nature in other fish, the author concludes that they are but highly specialized and enormously enlarged representatives of the phosphorescent organs on the heads of such allied Scopelids as *Scopelus rafinesquii* and *S. metopoclampus*.

The Report of Dr. von Lendenfeld is of a more general character, treating as it does of the phosphorescent organs of most of the known phosphorescent fishes, though not alluding to those of Ipnops. These organs are classified into the regular ocellar organs and the irregular glandular organs. Both these classes are again subdivided in reference to their form or position; and in conclusion we have a comparison of the different phosphorescent organs of fishes, and of these as compared with similar organs in other animals.

Dr. von Lendenfeld sums up his investigations as follows:—(1) The phosphorescent organs of fishes are more or less modified glands which have partly been developed from simple slime-glands in the skin, and partly in connection with the slime-canal system; (2) the typical clavate cells are modified gland-cells; (3) the accessory reflectors and sphincters are developed from the skin around and below the gland; (4) the large suborbital organs are innervated by a modified branch of the trigeminus, and the other organs by the ordinary superficial nerves.

A splendid atlas of plates accompanies this volume. Of these, sixty-six represent the new species described by Dr. Günther, and several of them are folding plates; two illustrate the anatomy of the phosphorescent organs of *Ipnops murrayi*; and the remaining five are drawn by Dr. von Lendenfeld and illustrate in a very beautiful manner his Report on the phosphorescent organs just alluded to.

SALINE DEPOSITS.

Die Bildung des Natronsaltpeters aus Mutterlangensalzen. By Dr. Carl Ochsenius. (Stuttgart: E. Koch, 1887.)

THIS book is a very valuable contribution to the history of saline deposits in general, but it is especially useful on account of the author's detailed description of the salt-beds of Chili and Peru, to the study of which he mainly devotes his attention. He discusses the various theories which have from time to time been advanced to account for the formation of Chili saltpetre (sodium nitrate), and shows that it must be regarded as the product of the action of oxidizing guano on certain mother-liquors containing carbonate of soda.

The salt-beds on the west coast of South America are found in the rainless district which stretches from Payta (near Amotape), in Peru, as far south as the twenty-sixth parallel. This region forms a narrow strip along the coast-line, and rarely exceeds twenty-five miles in width. It is bounded on the east by a chain of the Andes, and in the southern portion of the district the coast is fringed with low-lying hills, known as the coast Cordilleras. The author considers that, before the upheaval of the Andes, salt began to deposit in certain bays, which had been wholly or partially shut off from the sea by the gradual formation of an intercepting bar. Then, while the process of evaporation was still incomplete, the district was raised by volcanic action, and the mother-liquors from the salt lakes eventually escaped, running down into the valleys, and, where they encountered no obstacle, reaching the sea. The coast Cordilleras acted as a barrier in the southern portion of the district; while in the northern part the liquors doubtless returned to the sea. The volcanoes which produced the aforesaid upheaval exhaled immense volumes of carbonic acid gas, and the author considers that a portion of the sodium chloride in the mother-liquors was thus converted into sodium carbonate. (The co-existence of borates goes far to confirm the source of carbonic acid.) The coast in this part of Chili is studded with small islands containing deposits of guano

rich in ammonia. The guano dust is carried by the prevailing west winds far into the country, and would fall into the mother-liquor lakes, where, on exposure to the air at a warm temperature, it would gradually oxidize to nitrate, and, acting on the sodium carbonate, would form sodium nitrate (Chili saltpetre).

The "caliche" (crude saltpetre) is most variable in appearance and in the percentage of nitrate which it contains. The various substances, other than sodium nitrate, which are found in the Tarapaca and Atacama deposits are described at length by the author, who compares them with those which are found at Stassfurt, and he traces in the comparative prominence of the more soluble salts in the Chilian deposits a further confirmation of his theory that the nitre-beds are formed from mother-liquor salts.

The book is well indexed, and is supplied with a map and several sections of the district described.

J. I. W.

OUR BOOK SHELF.

Tenerife, and its Six Satellites. By Olivia M. Stone. In Two Vols. (London: Marcus Ward, 1887.)

A GOOD book on the Canary Islands, which have been of so much service to many an invalid, has long been wanted; for, as Mrs. Stone says, many parts even of the best-known islands of Tenerife and Gran Canaria are untrodden ground to English people, and are but little known to persons of any other nationality. Mrs. Stone supplies all the information that can be needed by the most exacting visitor to the islands, or by persons who may wish to read about them at home. As she has already shown in her "Norway in June," she has excellent powers of observation, and knows how to give a clear and effective account of all that she sees in her travels. In the present work her descriptions are all the more vivid because they were written "on the spot," when everything she wished to set down in her narrative was still fresh in her mind. To the Island of Hierro, to which she and her husband seem to have been the first English visitors, she devotes a good deal of attention; and what she has to say about that "solitary, happy, singular" island is full of interest, and would alone have justified her, if justification had been necessary, in making her travels in the Canary Islands the subject of a book. In an appendix she presents a useful epitome of all necessary expenses connected with her tour.

Through Central Asia. By Henry Lansdell, D.D. (London: Sampson Low, 1887.)

THIS is a popular edition of the author's well-known "Russian Central Asia, including Kuldja, Bokhara, Khiva, and Merv." He has omitted many whole chapters and most of the notes, thinking it best that the present edition should consist chiefly of a personal narrative. Any student who may desire fuller information regarding Central Asia is referred to the original work, in which Dr. Lansdell gives 4300 species of fauna and flora in about twenty lists with introductions, adds a bibliography of 700 titles, and treats more or less fully of the geography, economy and administration, ethnology, antiquities, history, meteorology, geology, zoology, and botany of all parts of Russian Turkistan, Kuldja, Bokhara, Khiva, and Turkmenia, down to the frontier of Afghanistan. To the new and abridged edition he has added an appendix on he delimitation of the Russo-Afghan frontier.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

The Star of Bethlehem.

REFERRING to the hypothesis in your last week's issue, that the star of Bethlehem was Venus, I would point out that 1180 synodical periods of Venus (*i.e.* 1180 × 583·92 = 689,025 mean solar days) take us back from October 28, 1887—when Venus was at her maximum brilliancy as a morning star—to only May 3 of the year 1 A.D. instead of December 25 of the year 1 B.C. For the number of days from October 28, 1887, to December 25, 1 B.C., is 689,155 (viz. 1887 × 365·2425 = 689,213 − 64 + 6 = 689,155). This would appear to show, either that the birth of Christ took place about May 3, or that Venus at her maximum brilliancy as a morning star was not the star of Bethlehem. I should be glad of your remarks on this.

JOHN T. NICOLSON.

20 Thirlestane Road, Edinburgh, December 26, 1887.

I INFER from the article entitled "The Star of Bethlehem" (NATURE, December 22, 1887, p. 169) that the writer supposes the craze he deals with did not exist until Venus became a morning star. It was equally prevalent here when, early in the year, she was an evening star, as the following fact will show. On May 21, 1887, a lady wrote me as follows:—"Will you kindly tell me what people mean about '*a wonderful star*'? All our servants are talking about it. . . . Some call it '*the star of Bethlehem.*' . . . I hear it is '*wonderfully bright!*'"

Torquay, December 26, 1887. WM. PENGELLY.

IN regard to the so-called "star of Bethlehem," Prof. C. A. Grimmer, in "Life from the Dead," No. 69 (August 1879), p. 267, wrote:—"It will be seen in 'Cassiopeia's Chair,' and will be accompanied by a total eclipse of the sun and moon. The marvellous brilliancy of the 'star of Bethlehem' in 1887 will surpass any of its previous visitations. It will be seen even at noonday, shining with a quick flashing light the entire year, after which it will gradually decrease in brightness, and finally disappear." E. COATHAM.

January 2.

On some Apparent Contradictions at the Foundations of Knowledge.

IN Chapter III. of Mr. Herbert Spencer's "First Principles" (p. 47, under heading, "Ultimate Scientific Ideas"), are treated the subjects of space and time. Here contradictions and difficulties of an apparently insuperable character are encountered in the attempt to define the nature of space and time, and the existence of these difficulties is frankly acknowledged. But with all the respect that is here due, it appears difficult to admit that these apparent contradictions are necessary, and in regard to space, in the first place, it will be my object here to suggest a remedy.

I will first quote some passages from the "First Principles" (5th edition) relating to this question, viz. as follows:—

"Thus as space and time cannot either be nonentities, nor the attributes of entities, we have no choice but to consider them as entities. But while on the hypothesis of their objectivity, space and time must be classed as things, we find on experiment that to represent them in thought as things is impossible" (p. 47).

It will be observed here that we encounter the apparent contradiction that those are classed as things which it is found impossible to represent in thought as things.[1]

[1] Experiment would then indirectly say that space and time were *not* things.

Then it is remarked, "To be conceived at all, a thing must be conceived as having attributes" (p. 47); and yet the author admits that it is impossible to assign any attribute to space (p. 48). So that it would appear from the last impossibility that space is *not* a thing (or entity).

It is added, "All entities, which we actually know as such, are limited" (p. 48). But, on the other hand, it is allowed that, "Of space and time we cannot assert either limitation or the absence of limitation" (p. 48).

It is observed also as follows:—"Nor are space and time unthinkable as entities, only from the absence of attributes" (p. 48). This would involve the conclusion apparently that that is considered to be an entity which is absolutely "*unthinkable*" as such.

Must there not be some flaw here, and some solution possible?

I have to propose—and this may appear very bold at first sight—that space is a non-entity. I must explain my meaning more fully. The first question or difficulty will be, How can we conceive of space (a void) or even talk of it, if it be a non-entity or nothing? In fact, on p. 177 is the remark, "Nothing cannot become an object of consciousness."

In reply to this, I would venture to suggest that under certain conditions, nothing can become an object of consciousness, viz. *by contrast with something*. We can be conscious of an absence. Darkness can become an object of consciousness by contrast with light. So space in itself—which I contend is nothing—is an object of consciousness [1] by contrast with matter.

We consider space to be an entity, I fancy, because of our experience with palpable air, &c., which (for convenience, but inaccurately) is called space. Space *per se*, an absolute void, we have no experience of. We measure all so-called spaces with matter—standards made of matter. We estimate how much solid matter is absent in a room (for instance), which we call its "volume." Mathematical lines are unconsciously figured as material no doubt from our habit of drawing them; and the spaces of triangles, &c., are usually filled out with solid matter.

It would be ridiculous (as it seems) to ask what would happen if a void disappeared. It cannot disappear because it is *already* nothing.

In regard to matter, we can conceive a certain volume of it, a certain volume added to that, &c.; and no doubt we cannot easily limit the conceivability thus extending to a larger volume. But we are not forced (by necessity as it were) to conceive an infinite volume of any entity or actually existing thing; and it appears that a void is excluded from the category of the unknowable, as we cannot expect to know anything about nothing.

Why do we hear of the creation of matter speculated about (as an inadequate attempt at explanation), but the creation of space regarded as absurd? [2] Because the first is an entity and the second is not. A non-entity cannot be supposed to be created, or it is absurd to ask the question.

One may encounter difficulties of explanation *by assuming too much to exist*—too much to explain, it appears. So I account for some of the startling contradictions supposed to exist at the basis of knowledge. What is nothing, if a void be not nothing? In order to be face to face with nothing and contrast it with something, we should not have to abolish a void, I venture to think.

Another matter seems important. On p. 34 ("First Principles") is the following, viz.:—"Did there exist nothing but an immeasurable void, explanation would be needed as much as now. There would still arise the question, How came it so? If the theory of creation by external agency be an adequate one, it would supply an answer; and its answer would be—Space was made in the same way that matter was made. But the impossibility of conceiving this is so manifest, that no one dares to assert it. For if space was created it must have been previously non-existent. The non-existence of space cannot, however, by any mental effort be imagined. . . . We are unable to conceive its absence either in the past or in the future."

[1] It appears that in order to assert an existence there must be a conception of non-existence as a contrast; otherwise the word "existence" would seem to have no distinct meaning. If matter be an existence, its absence (or a void) must be a non-existence. In other words, an absolute void (vacuity) is contemplated as the absence of existence.

[2] The author remarks of space, on p. 48, as follows:—"The only attribute which it is possible for a moment to think of as belonging to it, is that of extension; and to credit it with this implies a confusion of thought. For extension and space are convertible terms."

In regard to the commencing passage, viz. "Did there exist nothing but an immeasurable void, explanation would be needed as much as now," it might be asked, When would you be satisfied with an explanation? Explanations must finish somewhere; they finish at existences, I should fancy, and cannot extend to their absence. It is this demanding explanation perpetually, without conceived limit, that leads to the contradictions and attempts at defining nothings—as seems manifest. Extraordinary as this view taken by the author appears, it is consistent with his assumption that an absolute void is an existence or thing, whereby it is put on the same footing as matter. But observe to what this further leads.

First, the inconceivable existence of an infinite thing without attributes is assumed. Second, its non-existence cannot "by any mental effort be imagined." This means, in my view, that all attempts to imagine it *more* nothing than it is, are futile. What better definition of nothing could we have than that we cannot assert it to have "either limitation or the absence of limitation," or it is "unthinkable" as an entity "from the absence of attributes"?

Well, in this way, actual existence of something which is put on the same footing as matter seems to be made a necessity for an infinite past time; as (unlike matter in this respect) we cannot even *imagine* change here—in fact, the original creation of this thing (a void) no one dares to assert." In the same way, no one would venture to assert the creation of a mathematical line, or a mathematical plane, *i.e.* the creation of extension [1] of one, two, or three dimensions.

From the author's conclusion that space is an entity, it may be reasoned, then, that, since we must apparently have one existence for an infinite past time, we may as well have two, or include matter. Hence, with all the deference which the views as a whole in the "First Principles" demand, I would point out that in this way support is given to the idea of existence for an infinite past time (impossible to grasp fairly, as the author concedes)—which, as I contend, is not warranted by the facts.

S. TOLVER PRESTON.

30 Rue de la Clef, Paris, December 1887.

[1] The author remarks that "Extension and space are convertible terms" (p. 48). I may express my agreement with the author as to the inadequacy of the theory of the "creation" of matter, as an explanation.

Christmas Island.

HAVING read with much interest the description of Christmas Island by Captain Aldrich and Mr. Lister, I have endeavoured to interpret some of the facts there given in the light of my own examination of similar islands in the Western Pacific. As pointed out by Captain Wharton, the complete casing of an island, 1200 feet in height, with coral rock is somewhat unusual. This may find its explanation in in the absence of stream-courses and ravines, a circumstance from which I infer that the island has not been exposed sufficiently long, since its upheaval, to the denuding agencies. When its surface has been extensively carved out by the action of running water, the old volcanic peak, which these upraised reefs envelop, will in all probability be exposed. Christmas Island, therefore, has still the early part of its story to unfold.

The three tiers of cliffs evidently mark pauses in the elevation. As they appear to decrease in height with the ascent, it would seem that older lines of cliffs on the upper slopes of the island have been removed to a great extent by denudation. The principal features of the movement of upheaval appear to resemble those of which similar upraised coral islands give evidence in the West Indies, Western Pacific, and other regions of elevated coral reefs. Protracted elevatory movements of from 100 to 300 feet are separated by long pauses, during which cliffs are worn back by the waves, and the reefs grow seaward: hence the terraced profiles of these islands. I have pointed out that in the Solomon Group these protracted movements consist of a succession of small upheavals of usually 5 or 6 feet at a time.

17 Woodlane, Falmouth. H. B. GUPPY.

A Mechanical Cause of the Lamination of Sandstone not hitherto noticed.

THE lamination of sedimentary rocks is usually attributed to the successive deposition of sediment of varying degrees of fineness or coarseness. Currents of water have a selective action

on the materials that are swept along by them, by which grains of one size and weight are laid down at one time, and of another size and weight at another. Changes in the nature of the material in suspension also occur through which the deposit may be at different times more siliceous, argillaceous, or calcareous. This is doubtless in most cases a true explanation of the cause of lamination in rocks, but it is not a full one, nor does it account for stratification such as I am about to describe.

In sand dunes composed entirely of siliceous grains such as are seen on the west coast of Lancashire between Liverpool and Southport, a strong false-bedded lamination is often beautifully developed. This is best seen when the sand-hills are moist from recent rainfall, and the talus has been cut away by a high tide, leaving a vertical face of sand to the shore side. After this has occurred a gentle wind will weather out the structure of the sand-hill in a remarkable manner. The layers often stand out several inches in projecting mouldings and fillets, while the finer laminæ are wonderfully developed. I have often minutely

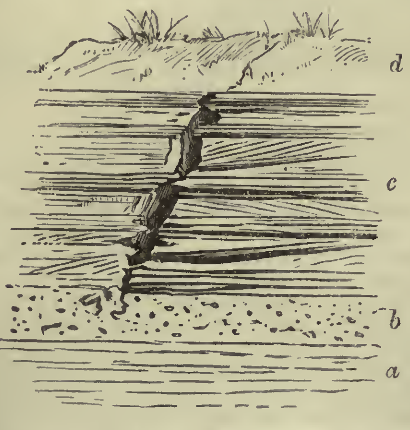


Fig. 1.—View of sand-dune, showing the bedding and laminæ weathered out by denudation. *a*, shore; *b*, loose talus; *c*, vertical cliff of sand; *d*, surface of sand-dune.

examined the constitution of these beds, but have been unable to detect any difference in the sizes of the constituent grains of the several beds or laminæ. What makes the fact more striking is that the grains are generally and in many cases much rounded. An examination, however, shows that the laminæ projecting from the face of the sand-cliff are much harder and more solid than the portions between them that have weathered back. They can, in fact, be broken off in pieces by the fingers without crumbling. The grains of sand, I must observe, are only temporarily bound together by the capillary attraction of the water.

The explanation which suggests itself to me is that the grains of sand, according to the state of the weather during deposition, are at one time more completely aggregated than at another. The shore sand, I have noticed, is greatly affected by the state of the water it is laid down by. In one place may often be seen a stretch of hard fine sand, while in another at the same level the sand may be soft, both being at the same point of saturation. It is well known to builders that pouring water on loose sand tends to solidify it, therefore it is most probable that the state of the weather influences the solidity of aggregation of the surface of the sand dunes and assists to build up layers of different density.

Between the projecting fillets already described as weathered out of the sand cliffs the sand is looser and more porous, and, drying faster, falls away from the face at a greater rate than the compacted beds. In sand heaped together by the wind there are few, I should think, would *a priori* look for much internal structure; yet here are the most undoubted evidences to the contrary which are generally passed by, being looked upon as a matter of course not demanding further thought! If we consider in what way the constituent grains naturally arrange themselves by gravity, we shall, I think, get an additional clue to the cause of lamination. Even if the grains were as round as shot they would by gravitation tend to arrange themselves in parallel planes, the upper grains falling into the interstices of those next below them, so—

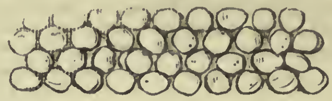


Fig. 2.

If, on the contrary, the grains have a long and short axis, they will tend to lie with the longer axis parallel to the plane of deposition, so—

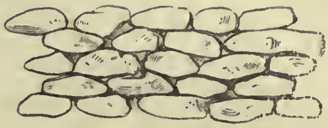


Fig. 3.

With irregular fragments the arrangement will not be so perfect, but they also will tend to be laid down in definite planes.

An examination of specimens of laminated sandstone shows that a fracture vertical to the plane of lamination exhibits a more jagged surface than a fracture parallel to the plane of lamination. This it is that gives the strength to sandstone to resist transverse stress.

It is thus seen that nature adopts the same principle to build up sandstone that a mason does to build a wall. From the way in which the particles arrange themselves a natural "bond" is produced. The grains "break joint," as it is technically called—that is, the joints are not vertically over each other—while the planes of deposition correspond to the "courses" of a wall. The principle here explained I have seen well exhibited in conglomerates formed of flattish oval pebbles. The mode of aggregation of the particles of a sedimentary rock, due to the ordinary dynamical laws governing deposition, and independent of the coarseness or fineness of the grains of successive layers, is an important factor in its constitution, which seems hitherto not to have attracted much attention. T. Mellard Reade.

Total Solar Eclipse of October 29, 878.

In Nature for March 11, 1875, vol. xi. p. 365, a computation is given of this eclipse, based on an entry in the "Annales Fuldenses," which runs thus: "Sol quoque in 4 kal. Novembris post horam nonam ita obscuratus est per dimidiam horam, ut stellæ in cœlo apparent, et omnes noctem sibi imminere putarent."

The computer found that the sun rose on that day at Fulda at 7h. 12m. apparent time, 6h. 57m. mean time, that the partial phase began at 0h. 56m., and ended at 3h. 24m., totality commencing at Fulda at 2h. 9m. 32s. local mean time, and continuing 1m. 41s. till 2h. 11m. 13s. He seems to have been puzzled, however, by the statement of the annalist that the darkness occurred "post horam nonam," observing plausibly enough that the ninth hour from sunrise would be 4 p.m.

It is shown, however, in Dr. Smith's "Dictionary of Christian Antiquities," vol. i. p. 793, that the day then employed by the Church was the natural day extending from sunrise to sunset, which was conceived to be divided into twelve hours (shorter of course in winter than in summer); so that the first hour was the twelfth part of the natural day, which began with sunrise; the sixth hour that which ended when the sun crossed the meridian, and so on.

The question, then, which arises is this: At what point of local mean time did the ninth natural hour end at Fulda on October 29, 878?

The sun rose at 7h. 12m. apparent time: this would give a semi-diurnal arc of 4h. 48m., or 9h. 36m., as the duration of the natural day, one-twelfth part of which, or 48 minutes, would be the length of the natural hour. As nine such hours would contain 432 minutes, it is clear that the ninth hour after 6h. 57m. the local mean time of sunrise would end at 2h. 9m., and the half-hour of darkness mentioned would have extended from 2h. 9m. to 2h. 33m. As the computer reckoned that totality lasted from 2h. 9m. 32s. to 2h. 11m. 13s., the obscuration would have been gradually passing away during that period.

The coincidence between the record and the calculation is a very striking one, and testifies at the same time to the veracity

of the Benedictine monks of Fulda, the trustworthiness of the lunar and solar tables, and the accuracy of the computer who brought out so marvellously correct a result without knowing that it agreed exactly with the true meaning of the record.

No doubt equal credit may be given to the computer's statement that this eclipse was total in London, totality continuing at St. Paul's from 1h. 16m. 20s. to 1h. 18m. 10s. local mean time.

C. S. TAYLOR.

Height of T'ai Shan.

A FORMER student of mine, Mr. S. Couling, has recently ascended T'ai Shan, the loftiest of the sacred mountains of China, and one of the most ancient and popular places of pilgrimage. He believes that the height of it above the surrounding plain has never before been measured, and has sent me his observations to reduce. The elevation from the plain to the summit comes out at 4780 feet; whilst a temple vaguely stated to be about 400 feet below the summit is, as ascertained by barometer, 4485 feet above the plain.

SILVANUS P. THOMPSON.

The Shadow of a Mist.

LIVING on the Blue Mountains at an elevation of 5000 feet, I am frequently astonished at the ever varying beauty of the mists and clouds. But a short time ago it was my good fortune to see the shadow of a mist, itself not visible.

On the evening of November 16, shortly after 7 o'clock in the evening, I was watching the electric light with which the military authorities were experimenting at Port Royal, 15 miles distant in a straight line. The light at times was so brilliant that the shadow of a person standing 20 feet from the house was distinct on the white-painted front, even when he held a lamp partially turned down close to his body on the side next the house. Rain was falling, but so slightly that there was no need for an umbrella. No mist or cloud was visible in the direct line to Port Royal, and yet a net-work of shadow was thrown on the house, the meshes of which were 3 or 4 inches in width. The shadows were all in motion, moving from east to west, in the direction of the scarcely noticeable breeze; individual portions of the meshes disappearing and re-forming as they moved, so that it was quite dazzling to look at the shadow, reminding me of the ripple on water as seen against a strong light. A puff of tobacco smoke had a shadow only when an inch or two from the house, so that the mist must have been much denser, and yet it cannot have been of any great breadth, or the shadow would have been uniform instead of reticulated. No doubt many of your readers can explain this appearance, which to me seemed so singular.

W. FAWCETT,
Director of Public Gardens and Plantations.

Cinchona, Gordon Town P.O., Jamaica, December 1, 1887.

The Ffynnon Beuno and Cae Gwyn Caves.

IT would seem that so long as the controversy with regard to the contents of these caves is confined to Dr. Hicks, Prof. Hughes, and Mr. W. G. Smith, the points at issue will never be decided. Dr. Hicks argues most needlessly for the *pre*-Glacial age of the cave deposits; Prof. Hughes calmly assumes that the outside deposits are *post*-Glacial; and many geologists must be heartily tired of hearing these two gentlemen contradict one another without defining what they mean by the terms Glacial and post-Glacial.

The fact is that the St. Asaph drift (to which Prof. Hughes now admits the outside deposits belong) is part of the later Glacial series of Northern England; and Prof. Hughes has no right to call it post-Glacial without defining what he means by that term. Most people call them Glacial deposits. If therefore the cave-deposits are older than this drift, they are not necessarily *pre*-Glacial, as Dr. Hicks maintains, but only anterior to what Mr. Mellard Reade terms the marine low-level boulder-clays. Now many think that these clays and their associated sands are coæval with, or newer than, the so-called post-Glacial river-gravels of Southern England. It is not surprising therefore that the cave fauna should be the same as that of the river-gravels, and it is perfectly needless to compare it with the fauna of the Cromer Forest bed.

In Lincolnshire the same marine shells occur in sands and gravels beneath the latest sheet of boulder-clay, and a gravel beneath the same clay at Burgh has yielded teeth and bones of *Elephas antiquus*, *Rhinoceros leptorhinus*, and *Bos primigenius*. These beds are on the same line of latitude as St. Asaph, and are probably of the same age as that drift; but it may be that neither of them are older than the oldest river-gravels of the Cam or Thames valleys.

It has been repeatedly pointed out that the terms Glacial and post-Glacial cannot be used as conveying any idea of relative age except along one and the same parallel of latitude, and it is rather surprising that the Woodwardian Professor of Geology should seem to be unaware of this. If by post-Glacial Prof. Hughes means later Glacial or newer Pleistocene, everyone will probably agree with him, but he confuses the issue by his bad choice of terms.

The palæontological evidence is really of no value—the argument leads nowhere; what we want is an expression of opinion by some geologist who has seen the locality and the recent excavations, regarding the explanation proposed by Prof. Hughes, viz. that the present position of the bones beneath the marine drift is due to the falling in of the roof of the cave near one entrance, while the animals may have got into the cave by another opening. Many geologists have visited the locality—will some of them communicate their views on this point?

A. J. JUKES BROWNE.

Southampton, December 28, 1887.

THE OLD MOUTH AND THE NEW: A STUDY IN VERTEBRATE MORPHOLOGY.

"THE question of the nature of the mouth," says Prof. Dohrn in one of the first of his celebrated "Studien zur Urgeschichte," "is the point about which the whole morphological problem of the Vertebrate body revolves." According to Dohrn, the present mouth of Vertebrates arose from the coalescence of a pair of gill-clefts. In this we have an example of Dohrn's principle of change of function, and also, as I hope soon to demonstrate, of Kleinenberg's law of the substitution of organs. I do not now wish or intend to give an account of the researches by which Dohrn showed that the mouth in some cases first arises as a pair of lateral invaginations of epiblast, still less of my own small contribution to this question, which consisted in recording the facts that the mouth also resembles a gill-cleft in some other particulars.

It suffices here to say that these researches have not yet been refuted, and that the view that the present mouth of Vertebrates is, so to speak, a new structure, rests on a very sound foundation.

With the blastopore as the foundation of mouth and anus I have here no concern, nor have I any sort of sympathy with the upholders of a theory which has been condemned and rejected by embryologists such as Lankester, Kleinenberg, and Salensky.

The problem I have to discuss is, granted that the present Vertebrate mouth is a new[1] structure, what traces, if any, are to be found of the old mouth? It is conceivable, and I strongly emphasize the point, that the old mouth might have disappeared, even from the development, without leaving a trace behind.

We seem to be gradually getting out of the idea that ontogeny is even a fair repetition, much less a perfect one, of phylogeny, for absolutely rudimentary organs (organs performing no function at all) are only retained as larval or embryonic organs, as the basis or *Anlage* of other organs, or, finally, because they are inseparably connected with the development of other organs. Of the latter a fair case, it seems to me, is to be seen in the rudiment of the parietal eye in the higher Vertebrates. This organ, functionless except in a few fishes and reptiles, possibly only reappears in the development because it is intimately connected in some way or other with the paired eyes.

A still better example is, I think, to be met with in the

[1] It is rather paradoxical to speak of a thing as new which has existed in its present form for untold millions of years.

rudiments of the gill sense-organs and ganglia described by Prof. Froriep in Mammalia. (Of these I hope to give a fuller account in connection with other work.) I find them in lizards, crocodiles, and birds; and there can be little doubt that they exist as rudiments in all animals above fishes and amphibia. Their recurrence has its explanation in that they probably form the *Anlage* for certain portions of the cranial ganglia.

It was Dohrn who first hinted, in his work on "Der Ursprung der Wirbeltiere," published in 1875, that the hypophysis cerebri represented the last remains of the old mouth, and that it must have opened on the dorsal surface, after passing between the crura cerebri.

This idea he soon gave up, and indeed, in the work above mentioned, he inclined to the view that the opening lay somewhere in the region of the medulla oblongata. Since then he has relinquished, for the time, the search for the old mouth, and has advised others to do the same.

His first hypothesis has more recently been advanced as new by Prof. Owen and Mr. J. T. Cunningham. Both of these writers hold very slightly different views from those originally suggested by Dohrn.

Some of the statements which I am about to make appear on the surface to bear slight resemblance to Cunningham's views, but, as I hope will be seen, nothing could be further from the truth. Cunningham, starting from Balfour's well-known, and now universally accepted, belief that the spinal cord and brain were once an open plate, advocated, as the latest discovery of Vertebrate morphology, the view that the infundibulum, whose walls consist of nervous matter and nothing else, is the vestige of the old mouth which pierced the brain.

One cannot but marvel at the rashness of an hypothesis which annexes, without more ado, a portion of the nervous system, and proclaims it to all the world as the remnant of a former passage from the exterior to the stomach of the animal!

Cunningham overlooks entirely the nature and exceedingly complicated development of the processus infundibuli, or nervous portion of the hypophysis.

Although, thanks to Rabl-Rückhard and others, we have obtained a certain amount of light on the nature of the pineal gland or epiphysis, the body (hypophysis), at the opposite end of the third ventricle still remains one of those organs on which all sorts of speculations may be made, with impunity. Some of the explanations offered are in accordance with certain facts of its development. Others, on the contrary, accord with no known fact of embryology.

The nervous part—or, as I shall call it, the *neural hypophysis*—has been considered by Rabl-Rückhard as a gland secreting cerebro-spinal fluid. I must, however, express a strong opinion that such a glandular function is extremely improbable, for the conversion of a piece of nervous tissue into a gland is absolutely without parallel.

Goette and Wiedersheim both regard the nervous part as a remnant of a sense-organ; against which view *a priori* little or nothing can be said. The mouth part or *oral hypophysis* was finally classed by Dohrn as the rudiment of a pair of gill-clefts—a supposition not wholly unsupported by its developmental history. It has also, not unnaturally, been looked upon as a remnant of a mouth-gland.

Prof. Hubrecht made it the basis of his comparisons of Nemertea and Vertebrata, and saw in it the remains of the Nemertean proboscis, the Vertebrate notochord being the homologue of the proboscis sheath—comparisons which appear to me to be as little capable of support as those of the same investigator between the Vertebrate and Nemertean nervous systems.

And so, after all, on turning to Wiedersheim's latest book, "Der Bau des Menschen," we read: "The hour of the release of the hypophysis cerebri from its obscure position has not yet struck, and the problems it presents are rendered more difficult in that it develops from two different points—from the brain (infundibulum) and from the epiblast of the primitive pharyngeal involution."

For what we know of the facts of its anatomy and development we are mainly indebted to five distinguished morphologists: Profs. W. Müller, Goette, Mihálkovics, Kölliker, and Dohrn. In the following very brief summary I partly follow Kölliker's account (in his valuable "Entwickelungsgeschichte des Menschen," 1879), which, for the time it was written, is by far the most complete we possess.

My own researches on Sharks, Ganoids, Dipnoi, Cyclostomata, Amphibia, Lizards, Snakes, Crocodiles, Birds, and Mammals, mainly confirm Kölliker, who, in his turn, has taken the greater portion of his account from the beautiful classic of Mihálkovics.

The hypophysis cerebri is composed of two parts: the one, *neural hypophysis*, derived from the nervous system; the other, *oral hypophysis*, from the epiblast in the region of the mouth.

The oral hypophysis is formed early in development as an epiblastic involution towards the end of the notochord, *i.e.* towards the hypoblast, and in the direction of the base of the brain. In some cases it may even grow in the direction of a process of hypoblast immediately below the anterior end of the notochord. But, except in Myxine, it never fuses with the hypoblast. It afterwards becomes pinched off from the pharynx, and gets thus to lie on the floor of the skull, becoming finally converted into a compound gland-like organ.

The neural hypophysis, or hinder lappet of the hypophysis, on the other hand, develops ventrally as a process of the basal portion of the thalamencephalon, or hinder part of the fore-brain. At first composed of tissue of exactly the same character as the rest of the thalamencephalon, it becomes solid below and converted into indifferent tissue; the portion of the process which remains hollow, and forms the base of the infundibulum, alone retains a nervous structure. Kölliker records that in pig embryos of 3 centimetres in length longitudinal bundles of nerve-fibres pass into the developing neural hypophysis, or processus infundibuli as it is called, from the base of the thalamencephalon.

In most cases, especially in Mammalia and also in Dipnoi, the neural hypophysis becomes closely and almost inseparably connected with the oral hypophysis. Usually the *Anlage* of the oral hypophysis lies in the region of the mouth epiblast; in Petromyzon and Myxine it lies in front of and outside the mouth. The process by which it got into the mouth involution cannot be explained without numerous figures.

According to Dohrn, the oral hypophysis arises in Petromyzon as an invagination of epiblast in front of the mouth between the oral and nasal depressions. It grows towards the base of the infundibulum, and comes into close relationship with the end of the notochord, *i.e.* with a structure derived from hypoblast, while it approaches a special process of hypoblast itself, with which, however, in Petromyzon it does not fuse. In Myxine, although the development is unfortunately not yet known, we may assume that this fusion is effected, for in that animal it opens throughout life into the gut (see figure, *O.M.*).

In Ammocœtes it gives off a certain number of gland-follicles, which, according to Dohrn, become pinched off in the Petromyzon. While I am not yet quite convinced of the certainty of this latter point, I find, in Myxine, numerous small glandular follicles opening into the oral hypophysis. In Petromyzon and Myxine the neural hypophysis is present, and, as I believe, not rudimentary. It appears to supply nerve-fibres to the oral hypophysis.

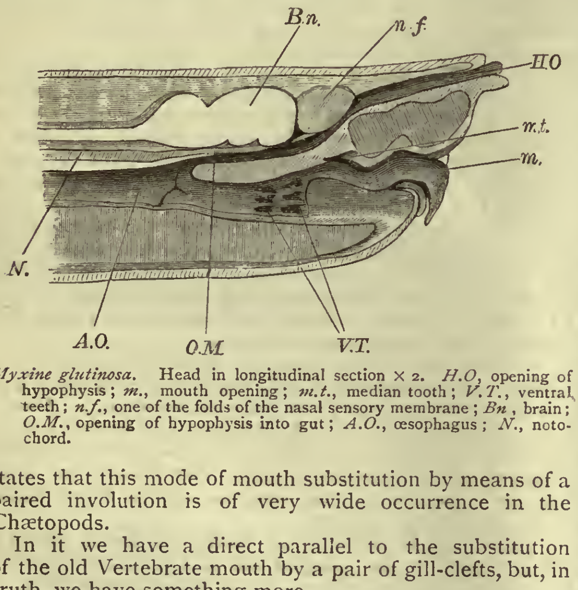
Dohrn finds in Hippocampus traces of a paired origin of the oral hypophysis. This is important.

I propose to divide the oral or glandular hypophysis into two parts, viz. a duct or main oral hypophysis and a glandular part or glandular hypophysis. The whole structure is without doubt in nearly all cases rudimentary, and of little or no functional importance. A mass of information bearing upon it has recently come to light in the study of the developmental history of Annelids, chiefly at the hands of Kleinenberg and Salensky.

From the results of Kleinenberg's work, more especially, we are placed in a position to compare the structure and development of the hypophysis with those of certain organs in the worms. To my mind, the comparison which follows is one of the neatest in the whole range of comparative morphology; I would therefore, before proceeding further, give a brief *résumé* of Kleinenberg's results so far as they here concern us.

In the first place, he records how the larval stomodæum or mouth is replaced in a very complicated manner by the Annelid permanent mouth or *Schlund*. The latter is formed as a paired involution of the stomodæum, *i.e.* of the epiblast, and this he considers to have originally represented stomodæal glands. It encroaches upon and swallows up the old mouth, and, finally fusing with the hypoblast, it opens into the gut.

The replacement of the larval mouth in Annelids by a new structure was already known, but Kleinenberg describes the steps of the process in great detail, and he



Myxine glutinosa. Head in longitudinal section × 2. *H.O*, opening of hypophysis; *m.*, mouth opening; *m.t.*, median tooth; *V.T.*, ventral teeth; *n.f.*, one of the folds of the nasal sensory membrane; *Bn*, brain; *O.M.*, opening of hypophysis into gut; *A.O.*, œsophagus; *N.*, notochord.

states that this mode of mouth substitution by means of a paired involution is of very wide occurrence in the Chætopods.

In it we have a direct parallel to the substitution of the old Vertebrate mouth by a pair of gill-clefts, but, in truth, we have something more.

Another phenomenon of extreme interest is the formation of the special mouth (or *Schlund*) nervous system. *This apparatus is only concerned with the innervation of the permanent* Schlund, *and takes no share in the innervation of the hypoblastic alimentary canal.* It arises as a special process of the hinder part of the subœsophageal ganglion: this grows towards the developing Schlund, becomes closely attached to the latter, fuses with it and gets pinched off from the larger portion of the subœsophageal ganglion, which is left as the first ganglion of the ventral chain.

I must here digress in order to discuss the question of the presence or absence of any representative of the supraœsophageal ganglion of Annelids in Vertebrates,[1] and here again Kleinenberg comes to our assistance.

I have myself devoted a good deal of attention to this point, and have arrived at the conclusion (held also, I believe, by Prof. Dohrn) that there is no likelihood at all of our finding an area in the Vertebrate brain which was ever pierced by the œsophagus—pierced so as to divide the brain into a supraœsophageal and a subœsophageal portion, which might be compared respectively to such divisions of the Annelidan nervous system. At first sight, this appears like an admission that the Annelidan theory of the origin of Vertebrates is untenable. But such is not the case.

From a large number of researches, including those of Bergh, Salensky, and Kleinenberg, we know that the supraœsophageal ganglion of Annelids certainly arises independently of the ventral chain, and that it only later becomes connected with the latter by the development of the circumœsophageal collar.

Kleinenberg's brilliant researches also teach us that the permanent Annelidan nervous system arises through substitution, and partial or entire disappearance of whole larval nervous apparatuses and sense-organs. And, indeed, after reading his beautiful work, one is fully prepared for one of the closing statements in it—that possibly the supraœsophageal ganglion is entirely absent in Vertebrates.

Personally, I have no hesitation at all in accepting this as probably true; but the grounds for my belief, or some of them, I can only hint at here. They arise out of as yet unpublished developmental researches. Briefly stated, I see in the development of the gill-clefts, with their special sense-organs and ganglia[1]—all of which lie in the region which is under the control of a system comparable to the ventral nerve-cord of Annelids—a probable cause of the disappearance of the supraœsophageal ganglion in the ancestors of Vertebrates,—in a similar way to that in which, according to Kleinenberg, the dislodging and destruction of the special larval ganglionic centres takes place in the Annelid.

I believe that in the ancestors of the Vertebrates, by the development of the eyes, and of the important gill sense-organs and ganglia, the ventral chain came to obtain control over a very extensive system of ganglia, sense-organs, and muscles; and, having already a control over the mouth or *Schlund*, it entirely deposed the supraœsophageal ganglion (and its sense-organs). The entire *raison d'être* of the latter being thus disposed of, it naturally degenerated and finally disappeared.

If it be admitted that the supraœsophageal ganglion of Annelids is absent in Vertebrates, and that the brain and spinal cord of the latter may be compared directly with the ventral cord of Annelids, then a whole host of direct structural relationships between Annelida and Vertebrates may be established. Kleinenberg expresses his opinion that the spinal ganglia of Vertebrates have their parallel in the parapodial ganglia of Annelids,—a comparison which, as I shall elsewhere show, is entirely justifiable for the spinal ganglia and for certain portions of the cranial ganglia also.

Let me now briefly review the conditions demanded of any structures in the Vertebrate which are to be homologized with the permanent mouth of Annelids. Such ought to arise as a paired involution of epiblast (though it is conceivable *a priori* that the paired character might be lost). This involution must fuse with, and open into, the cavity of the hypoblast. It must also give rise to certain glands, and it must have a special nervous system of its own derived from the hinder part of the first ventral ganglion or its homologue—which nervous system must supply it alone, and no other part of the alimentary canal.

All these conditions are fulfilled by the complex called hypophysis cerebri.

In at least one case (Hippocampus) the oral hypophy-

[1] I postpone the consideration of Prof. Semper's views on this point, and on the nature of the mouth in Annelids and Vertebrates.

[1] The cranial ganglia of Vertebrates are far more complicated morphologically than has hitherto been recognized. In addition to parts which appear to correspond morphologically to the posterior root ganglia of the spinal nerves plus the sympathetic ganglia, they also contain the special ganglia which are formed in connection with the gill sense-organs.

sis[1] is known to arise as a paired epiblastic involution (Dohrn). In the Cyclostomata it is formed as an epiblastic involution (possibly paired) at the extreme anterior end of the body. In one Vertebrate alone, Myxine (*vide* figure), it still opens into the hypoblast; in all others it approaches the hypoblast in development, but does not fuse with that layer. It always lies in very close relationship with the extreme end of the notochord—that is, with the end of a structure derived from the hypoblast.

In adult Petromyzon, in which the tube of the oral hypophysis has the same relationships as in Myxine, except that the posterior opening into the hypoblastic sac is absent, it nevertheless has an astonishing length, and ends blindly very close to the gut. In Myxine and Petromyzon, tubular glands are developed in connection with it. In all the higher Vertebrates, in which the oral part is very rudimentary, it always has a distinct glandular character.

And now, what of the last condition? This also is satisfactorily met. In all cases the oral hypophysis has a special, and indeed large, process of nervous matter (the processus infundibuli, or neural hypophysis), which is derived from the posterior part of the fore-brain, from the base of the infundibulum. This process is concerned with the innervation of the oral hypophysis alone. In Myxine and Petromyzon alone, so far as my researches extend (possibly also in Protopterus), this nervous system is not rudimentary. In most Vertebrates the neural hypophysis, which, as Kölliker aptly remarks, is at first composed of the same cell elements and fibres as the rest of the brain, degenerates, and in very many full-grown animals forms a mass of tissue, the structure of which many observers have compared to that of the suprarenal bodies (known to be masses of degenerated tissue).

The neural hypophysis is thus the most remarkable structure in the whole of the Vertebrate central nervous system. Though degenerated, it still clings to the traditions of its ancestry, for even, as it were, in its death it is closely and almost inseparably connected with the rest of the hypophysis, especially in Mammalia and in Dipnoi. In Myxine alone, of all Vertebrates, the old mouth still retains some of its functions as a mouth; it conducts the water of respiration to the gills. In this case, even, changes have occurred, for the nose[2] (see figure, *n.f.*) has got partly involved in the passage of the old mouth. If it be true that the nose was originally a branchial sense-organ—which view, in spite of Gegenbaur, I still maintain—its assumption of a position in the passage of the old mouth in Myxine is, on purely physiological grounds, intelligible.

It is well known that that which I call the old mouth in Myxine is purely respiratory, conducting water into the gills; and what then could be more likely than that one of the branchial sense-organs should be, as it were, told off to do duty at its entrance. It is certain, from Goette's and Dohrn's researches, that these passages in Myxine and Petromyzon are the representatives of the oral hypophysis. I have gone over and extended these observations, and can fully confirm Dohrn in nearly every point, and all I claim here is the identification of the hypoblastic opening in Myxine as the (modified) opening of the old mouth into the gut.

If the above morphological comparison can be maintained (and I believe it can), the importance of its bearing on the morphology of Vertebrates can hardly be over-estimated.

A number of other problems and conclusions arise out of all this, but I reserve the consideration of these for a much more exhaustive work, in which the literature of the subject will receive full attention. J. BEARD.

Anatomisches Institut, Freiburg i/B., November 16.

[1] I believe it is very frequently paired, though not at its point of origin.
[2] In Petromyzon, Dohrn finds that the nose is at first a special depression apart from the hypophysis invagination. The latter lies between the nose and mouth.

TIMBER, AND SOME OF ITS DISEASES.[1]

III.

HAVING now obtained some idea of the principal points in the structure and varieties of normal healthy timber, we may pass to the consideration of some of the diseases which affect it. The subject seems to fall very naturally into two convenient divisions, if we agree to treat of (1) those diseases which make their appearance in the living trees, and (2) those which are only found to affect dead timber after it is felled and sawn up. In reality, however, this mode of dividing the subject is purely arbitrary, and the two categories of diseases are linked together by all possible gradations.

Confining our attention for the present to the diseases of standing timber—*i.e.* which affect undoubtedly living trees—it can soon be shown that they are very numerous and varied in kind; hence it will be necessary to make some choice of what can best be described in this article. I shall therefore propose for the present to leave out of account those diseases which do injury to timber indirectly, such as leaf-diseases, the diseases of buds, growing roots, and so forth, as well as those which do harm in anticipation by injuring or destroying seedlings and young plants. The present article will thus be devoted to some of the diseases which attack the timber in the trees which are still standing; and as those caused by fungus parasites are the most interesting, we will for the present confine our attention to them.

It has long been known to planters and foresters that trees become rotten at the core, and even hollow, at all ages and in all kinds of situations, and that in many cases the first obvious signs that anything is the matter with the timber make their appearance when, after a high gale, a large limb snaps off, and the wood is found to be decayed internally. Now it is by no means implied that this rotting at the core—"wet-rot," "red-rot," &c., are other names generally applied to what is really a class of diseases—is *always* referable to a single cause; but it is certain that in a large number of cases it is due to the ravages of fungus parasites. The chief reason for popular misconceptions regarding these points is want of accurate knowledge of the structure and functions of wood on the one hand, and of the nature and biology of fungi on the other. The words disease, parasitism, decomposition, &c., convey very little meaning unless the student has had opportunities of obtaining some such knowledge of the biology of plants as can only be got in a modern laboratory: under this disadvantage the reader may not always grasp the full significance of what follows, but it will be at least clear that such fungi demand attention as serious enemies of our timber.

It will be advantageous to join the remarks I have to make to a part description of some of the contents of what is perhaps one of the most instructive and remarkable museums in the world—the Museum of Forest Botany in Münich, which I have lately had the good fortune to examine under the guidance of Prof. Robert Hartig, the distinguished botanist to whose energy the Museum is due, and to whose brilliant investigations we owe nearly all that has been discovered of the diseases of trees caused by the Hymenomycetes. Not only is Prof. Hartig's collection unique in itself, but the objects are classical, and illustrate facts which are as yet hardly known outside the small circle of specialists who have devoted themselves to such studies as are here referred to.

One of the most disastrous of the fungi which attack living trees is *Trametes radiciperda* (Hartig), the *Polyporus annosus* of Fries, and it is especially destructive to the Coniferæ. Almost everyone is familiar with some of our common Polyporei, especially those the fructifications of which project like irregular brackets of various colours from dead stumps, or from the stems of moribund trees;

[1] Continued from p. 207.

well, such forms will be found on examination to have numerous minute pores on the under side or on the upper side of their cheese-like, corky, or woody substance, and the spores which reproduce the fungus are developed on the walls lining these many pores, to which these fungi owe their name. *Trametes radiciperda* is one of those forms which has its pores on the upper side of the spore-bearing fructification, and presents the remarkable peculiarity of developing the latter on the exterior of roots beneath the surface of the soil (Fig. 11).

This is not the place to discuss the characters of species and genera, nor to enter at any detail into the structure of fungi, but it is necessary to point out that in those cases where the casual observer sees only the fructification of a Polyporus, or of a toadstool, or of a mushroom (projecting from a rotting stump or from the ground, for

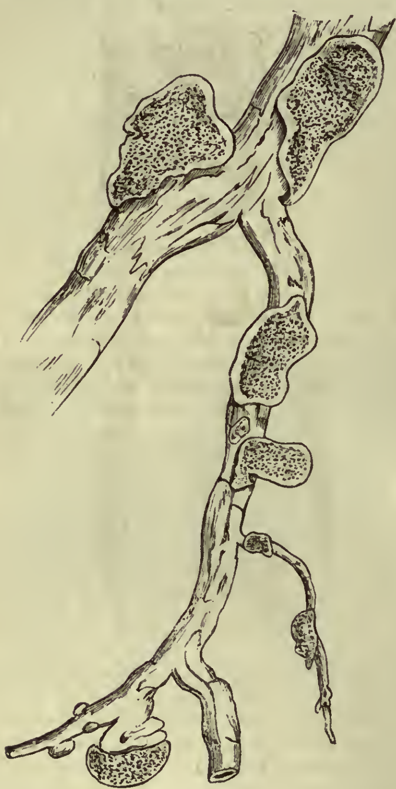


FIG. 11.—Portion of root of a spruce-fir, with fructification of *Trametes radiciperda* (after Hartig). Each fructification is a yellowish-white mass of felt-like substance spread over the root, and with minute pores, in which the spores are produced, on its outer surface; the mycelium which has developed it is in the interior of the root.

instance), the botanist knows that this fructification is attached to, and has taken origin from, a number of fine colourless filaments woven into a felt-like mass known as the mycelium, and that this felt-work of mycelium is spreading on and in the rotten wood, or soil, or whatever else the fungus grows on, and acts as roots, &c., for the benefit of the fructification.

Now, the peculiarity of the mycelium of this *Trametes radiciperda* is that it spreads in the wood of the roots and trunks of pines and firs and other Conifers, and takes its nourishment from the wood-substance, &c., and it is to the researches of Hartig that we owe our knowledge of how it gets there and what it does when there. He found that the spores germinate easily in the moisture around the roots, and put forth filaments which enter between the bark-scales, and thus the mycelium establishes itself in the living tree, between the cortex and the wood (Fig 12). It is curious to note that the spores may be carried from place to place by mice and other burrowing animals, since this Trametes is apt to develop its fructification and spores in the burrows, and they are rubbed off into the fur of the animals as they pass over and under the spore-bearing mass.

When the mycelium obtains a hold in the root, it soon spreads between the cortex and the wood, feeding upon, and of course destroying, the cambium. Here it spreads in the form of thin flattened bands, with a silky lustre, making its way up the root to the base of the stem, whence it goes on spreading further up into the trunk (Fig. 12).

Even if the mycelium confined its ravages to the cambial region, it is obvious, from what was described in Articles I. and II., that it would be disastrous to the tree; but its destructive influence extends much further than this. In the first place, it can spread to another root

FIG. 12.—Piece of root of spruce-fir, with the mycelium of *Trametes radiciperda* (after Hartig) enlarged about 3 times. The white mycelium spreads in a fan-like manner over the surface beneath the cortex, as seen in the figure where the latter has been lifted and removed (*a*). Here and there the mycelium bursts through the cortex in the form of white protuberances (*b*), to form the fructifications.

belonging to another tree, if the latter comes in contact in the moist soil with a root already infected; in the second place, the mycelium sends fine filaments in all directions into the wood itself, and the destructive action of these filaments—called hyphæ—soon reduces the timber, for several yards up the trunk, to a rotting, useless mass. After thus destroying the roots and lower parts of the tree, the mycelium may then begin to break through the dead bark, and again form the fructifications referred to.

Since, as we shall see, *Trametes radiciperda* is not the only fungus which brings about the destruction of standing timber from the roots upwards, it may be well to see what characters enable us to distinguish the disease thus induced, in the absence of the fructification.

The most obvious external symptoms of the disease in a plantation, &c., are: the leaves turn pale, and then yellow, and die off; then the lower part of the stem begins to die, and rots, though the bark higher up may preserve its normal appearance. If the bark is removed

from one of the diseased roots or stems, there may be seen the flat, silky, white bands of mycelium running in the plane of the cambium, and here and there protruding tiny white cushions between the scales of the bark (Fig. 12); in advanced stages the fructifications developed from these cushions may also be found. The wood inside the diseased root will be soft and damp, and in a more or less advanced stage of decomposition.

On examining the timber itself, we again obtain distinctive characters which enable the expert to detect the disease at a glance. I had the good fortune to spend several pleasant hours in the Münich Museum examining and comparing the various diseases of timbers, and it is astonishing how well marked the symptoms are. In the present case the wood at a certain stage presents the appearance represented in the drawing, Fig. 13. The general tone is yellow, passing into a browner hue. Scattered here and there in this ground-work of still sounder wood are peculiar oval or irregular patches of snowy white, and in the centre of each white patch is a black speck. Nothing surprised me more than the accu-

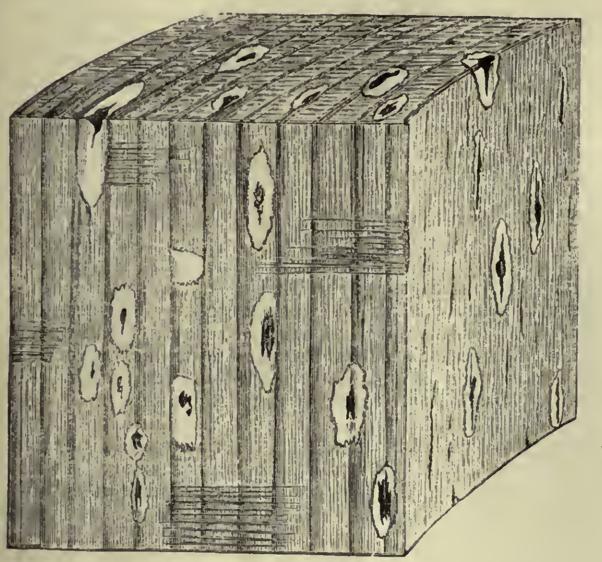


FIG. 13.—A block of the timber of a spruce-fir, attacked by *Trametes radiciperda*. The general colour is yellow, and in the yellow matrix of less rotten wood are soft white patches, each with a black speck in it. These patches are portions completely disorganized by the action of the mycelium, and the appearance is very characteristic of this particular disease. (After Hartig.)

racy with which Prof. Hartig's figures reproduce the characteristic appearance of the original specimens in his classical collection, and I have tried to copy this in the woodcut, but of course the want of colour makes itself evident.

It is interesting and important to trace the earlier changes in the diseased timber. When the filaments of the fungus first begin to enter the wood, they grow upwards more rapidly than across the grain, piercing the walls of the cells and tracheides by means of a secretion—a soluble ferment—which they exude. This ferment softens and dissolves the substance of the walls, and therefore, of course, destroys the structure and firmness, &c., of the timber. Supposing the filaments to enter cells which still contain protoplasm and starch, and other nutritive substances (such as occur in the medullary rays, for example), the filaments kill the living contents and feed on them. The result is that what remains unconsumed acquires a darker colour, and this makes itself visible in the mass to the unaided eye as a rosy or purple hue, gradually spreading through the attacked timber. As the destructive action of the fungus proceeds in the wood, the purple shades are gradually replaced by a yellowish cast, and a series of minute black dots make their appearance here and there; then the black dots gradually surround themselves with the white areas, and we have the stage shown in Fig. 13.

These white areas are the remains of the elements of the wood which have already been completely delignified by the action of the ferment secreted by the fungus filaments—*i.e.* the hard woody cell-walls have become converted into soft and swelling cellulose, and the filaments are dissolving and feeding upon the latter (Fig. 14). In the next stage of the advancing destruction of the timber the black dots mostly disappear, and the white areas get larger; then the middle-lamella between the contiguous

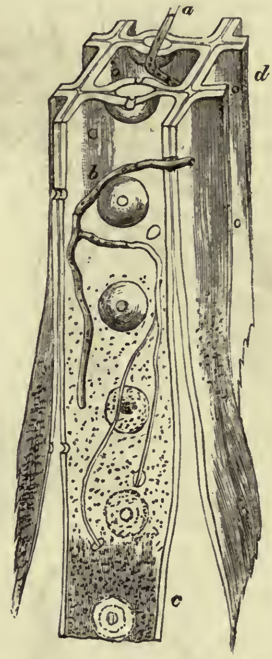


FIG. 14.—Sectional view of a tracheide of the spruce-fir, attacked by the hyphæ (*a*, *b*) of a *Trametes*, highly magnified (after Hartig). The upper part of the tracheide has its walls still sound, though already pierced by the hyphæ; the lower part (*c*) has the walls completely delignified, and converted into cellulose, which swells up and dissolves The middle-lamella is also undergoing diss lution. The holes in the walls have been bored by hyph e.

elements of the wood becomes dissolved, and soft places and cavities are produced, causing the previously firm timber to become spongy and soft, and it eventually breaks up into a rotting mass of vegetable remains.

It will readily be understood that all these progressive changes are accompanied by a decrease in the specific gravity of the timber, for the fungus decomposes the substance much in the same way as it is decomposed by putrefaction or combustion, *i.e.* it causes the burning off of the carbon, hydrogen, and nitrogen, in the presence of oxygen, to carbon-dioxide, water, and ammonia, retaining part in its own substance for the time being, and living at its expense.

H. MARSHALL WARD.

(*To be continued.*)

PROFESSOR ALEXANDER DICKSON.

THE close of 1887 has been marked by a long death-roll in the ranks of science. In the company of botanists it has been especially heavy, and now the sad news of the tragically sudden death of Prof. Alexander

Dickson, at the early age of fifty-one, comes upon us with startling unexpectedness. Two days before Christmas Prof. Dickson left Edinburgh, in his usual health and vigour, for Hartree House, his Lanarkshire residence. During the following days he spent much of his time in the favourite pastime of curling, which he much enjoyed. On Friday last, December 30, 1887, he was in exceptionally good spirits on the ice; his side was winning a close match, and he entered keenly into the excitement of the moment, when, without warning, he dropped dead in the act of making a shot.

Alexander Dickson was born in Edinburgh on February 21, 1836, the second son of David Dickson, of Hartree and Kilbucho, extensive estates in Lanarkshire and Peebles-shire, to which he afterwards succeeded, his elder brother having predeceased his father. Educated when a boy at home, he proceeded to the University of Edinburgh, where he graduated M.D. in 1860, obtaining a gold medal for a thesis on "The Development of the Seed-vessel of *Caryophyllaceæ*." After graduating, he soon abandoned medicine, and devoted himself to botanical pursuits. During the year 1862 he acted as Deputy Professor of Botany in the University of Aberdeen for Prof. Dickie, then in bad health; in 1866 he was appointed to the Chair of Botany in the University of Dublin, vacant by the death of Dr. W. H. Harvey; and a year later he added to this appointment that of Professor of Botany in the Royal College of Science for Ireland. In 1868, on the death of Dr. G. W. Walker-Arnott, he succeeded to the Chair of Botany in the University of Glasgow, which he held until 1879, when he was appointed Professor of Botany in the University, and Regius Keeper of the Royal Botanic Garden, in Edinburgh, upon the resignation of these offices by Dr. J. H. Balfour. He held these appointments at the time of his death. He received the honorary degree of M.D. from the University of Dublin, and that of LL.D. from the University of Glasgow, and was a member of various learned Societies. Besides his scientific life he had another important part to play as a laird with large properties in three counties, and he was a model landlord. He had the highest ideas of the duties of his position, and acted up to them. Money, time, and energy were given with self-denying devotion to the improvement of his farms and of the condition of his tenants, and no better-ordered estates could be found than those which he controlled. He was a Deputy-Lieutenant of Peebles-shire, and took an active share in all the functions which his position entailed.

By the death of Alexander Dickson the botanical world loses one of its best morphologists. He wore the mantle of the old French school typified in Mirbel, Richard, St. Hilaire, and Payer, of which Baillon is at present the foremost French representative; and at a time like the present, when it is a fashion to decry morphology, his loss falls all the more heavily. No botanist in this country had so full and accurate a grasp of organography. His published papers, numerous and valuable as they are, afford but an imperfect idea, significant indeed, of the wealth of his knowledge, and the keenness of his perception. Those who came in contact with him will remember the fascination of his discourse, and the surprising variety and aptness of the illustrations which he could bring up one after the other to support his own views or confound those of an opponent. In all his scientific work the strong conservatism of his nature found expression. His cautious and logical mind did not allow of his following with enthusiasm rash speculations of the more ardent botanical workers; and the flood of literature on botanical subjects which is poured out year by year had no terrors for him, as he acted upon the principle, which many will agree is a sound one, that, if you leave the literature until it is a year or two old, what is worth reading sifts itself. The soundness of his judgment upon scientific problems may in some measure be traced to the influence of the precept and example of that glorious band of real teachers, which at the time of his University career made Edinburgh a centre of attraction in the intellectual world; and a good illustration of his force of mind is to be found in his attitude towards the much-discussed question of the growth of the cell-wall. Having satisfied himself that the apposition theory was a sufficient explanation, he consistently opposed Nägeli's intussusception theory during the years when it was all but universally accepted; and now the botanical world has come round again to regard an apposition theory as that which has the better basis in fact.

All organographic questions had a peculiar interest for Dickson. A considerable portion of his own work was devoted to the elucidation of the true nature of the flowers in *Coniferæ*. As the result of his researches on *Dammara* and observations on other *Coniferæ*, he adopted Baillon's view of the carpellary nature of the integument in *Pinus*, and, notwithstanding the defection by Strasburger, who originally supported this view, he continued to maintain it. Phyllotaxis was a subject to which he devoted great attention, and upon which he published several important papers. Amongst his most valuable researches are those on the embryogeny of *Tropæolum*, in which he traced the history of the peculiar roots by which the embryo is nursed in the seed; and the records of his embryological researches in *Pinguicula*, *Ruscus*, *Zostera*, *Phœnix*, *Delphinium*, and other plants, are very interesting and valuable contributions to knowledge. In recent years he gave considerable attention to the construction and development of pitcher-plants, and proved the true nature of the parts of their complex organs; and the structure of the *Hepaticæ* also engrossed him, one of his last papers being upon some species of this group, in which he joined issue with Leitjeb upon some fundamental points. In all his work there may be seen the scrupulous accuracy and attention to detail which was a leading feature in his character, and no man ever worked with more care and jealous regard for truth and with a more generous appreciation of the work of others.

Amidst the work of his scientific life and the duties connected with his estates he found time to cultivate the artistic side of his nature, which was developed in no ordinary degree. He was an accomplished and enthusiastic musician, and in later years found peculiar pleasure in collecting Gaelic airs. At botanical excursions to the Highlands he might be frequently found noting down an air as it was droned by a gillie or whistled by a herd, and he amassed a considerable number of these airs, which at one time he thought of publishing. He was also a very skilful draughtsman, and his drawings in chalk on the slate were quite a feature of his lectures.

In the discharge of every duty he was most conscientious, and his unostentatious kindliness attracted everyone who had dealings with him. Quiet and retiring in disposition, he was endeared to all by the nobility of his character and his sympathetic nature. As Professor his students loved him; as laird his tenants loved him. It has been said of him he could never lose a friend, for he never could say an unkind word or omit to do a kind action, and in this estimate all who knew him will concur. The news of his death will be heard with sorrow by a wide circle of friends, and bring sadness to many a heart which will mourn for one who had fine generosity of the kind that lets "not the right hand know what the left hand doeth."

NOTES.

THE Municipal Council of Paris proposes to establish in the Faculty of Sciences a new professorship devoted to the philosophy of biology, and especially to the teaching of the doctrines of Darwin. This distresses some of the older French zoologists,

but "their reign," a correspondent writes to us from Paris, "is coming to an end, and notwithstanding their obstinate and unintelligent opposition, Darwinism is the creed of all the younger French naturalists. The only trouble with regard to the new professorship will be to put the right man in the right place."

PROF. BONNEY'S course of lectures on geology begins at University College on Wednesday, January 11, and will be continued on the Thursdays, Tuesdays, and Wednesdays following. The course on economic geology begins on the following Friday, and will be continued on that day in each week.

A COURSE of about six lectures on "Photographic Chemistry" will shortly be delivered by Prof. R. Meldola, F.R.S., at the Finsbury Technical College. The course will begin on Wednesday, January 18, at 7.30 p.m., and be continued on successive Wednesdays. The object of the lecturer will be to develop the scientific principles upon which modern photography is based, so as to enable professional and amateur photographers to keep abreast of recent advancements in the subject. Those who attend the lectures will have the opportunity, if they desire it, of receiving practical laboratory instruction in the testing and valuation of photographic chemicals.

AT the opening meeting of the session of the Society of Telegraph-Engineers and Electricians, on Thursday, the 12th instant, the new President, Mr. Edward Graves, will deliver his inaugural address.

IN the abstracts of the Proceedings of the Chemical Society (Jan.–Dec. 1887) we find the following list of grants made from the Research Fund of the Society during the year:—£25 to Prof. E. H. Rennie, for the further study of the red colouring-matter of *Drosera whittakeri*; £25 to Mr. Holland Crompton, for the study of the action of nitric acid on copper-zinc and copper-tin alloys with the object of determining whether the metals exist in combination or admixed; £10 to Mr. C. H. Bothamley, for experiments on the use of dyes in photography, and especially on the sensitizing action of the dye; £25 to Mr. W. P. Wynne, for the determination of the nature of the products formed on oxidizing nitric oxide by admixture with oxygen; £10 to Mr. A. Wynter Blyth, for the study of the constitution of butter-fat.

METEOROLOGY is indebted to Dr. J. Hann for an exhaustive discussion of the distribution of atmospheric pressure over Central and Southern Europe, based upon the monthly and yearly means at 205 stations, for the thirty years 1851–80. Very few such discussions have been undertaken since the appearance of Buchan's great work, about twenty years ago. The author insists on the application of the correction for gravity, which up to the present time has been generally neglected. Only a few observations for France have been used in this discussion, but a work of a similar nature is in hand for that country by M. Angot. Charts are also drawn for each month and for the year, showing the isobars for every ·02 inch at the sea-level, and also for four months and the year at the level of 500 metres above the sea. The work forms Part 2, vol. ii. of Dr. Penck's "Geographische Abhandlungen" (Vienna, 1887).

THE Chief Signal Officer of the United States has issued a circular, dated December 6, stating that as, in his belief, the great value of simultaneous maps consists in showing the general features of the weather of the northern hemisphere, he has decided to reproduce daily a chart showing the general outlines of the pressure and wind for certain selected stations, although as before stated (NATURE, Dec. 8, p. 137), he cannot guarantee their continuance for any great length of time. The charts have commenced with October 1, 1886, and are based on the observations taken at noon, Greenwich time; the temperature observations are not represented, owing to the limited means available for such work.

THE Central Physical Observatory of St. Petersburg has issued a very useful table, showing for all its telegraphic stations the normal temperatures for 7 a.m. for each month, calculated from the number of years available for each place. From these values the Observatory constructs diagrams showing the annual march of the temperature, and from these curves the normal temperature may be calculated very closely for each day of the year. These data enable the Observatory to introduce into its Daily Weather Reports the departure of the temperature day by day from its normal value. The normal temperatures for foreign stations are to be similarly dealt with subsequently.

THE Swedish Government has given notice that in the beginning of the year 1888, a fog signal will be established near Hallands Wäderö lighthouse, on the eastern shore of the northern approach to the Sound, Kattegat. The signal will be a steam syren, which, during thick or foggy weather, will give *two blasts every minute*, in the following manner; a low note of *seven seconds* duration, an interval of *three seconds* silence, then a high note of *three seconds* duration, followed by an interval of *forty-seven seconds* silence.

THE Government *Gazette* of the Colony of Lagos of July 30 last contains monthly meteorological means for the year 1886. The observations are made at the Colonial Hospital, lat. 6° 27′ N., long. 3° 26′ E.

ON December 11, about 5.30 p.m., a brilliant meteor was seen in and around Christiania. It moved slowly in a south-easterly direction, and disappeared behind a bank of clouds. Its light, of a yellow-green colour, was very intense. The passage occupied about five seconds.

EARTHQUAKES on December 16 and 17 are reported from Prinpolje and Plewlje, in Bosnia. At Werny, in Turkistan, a shock occurred at midnight on December 16. A shock was noticed at Geneva on December 19 between 5 and 6 p.m. A telegram from Mexico states that a sharp shock occurred there at half past 7 on January 2.

A NUMBER of highly interesting experiments upon the behaviour of passive iron towards nitric acid when placed in a powerful magnetic field have recently been made by Messrs. Nichols and Franklin (*Amer. Journ. of Science*, December 1887). About 8 cubic centimetres of nitric acid, specific gravity 1·368, were poured upon a gramme of powdered iron contained in a perfectly clean test-tube. This test-tube was immersed in water contained in an outer glass vessel, and the temperature of the contents of the tube could be accurately ascertained by means of a thermometer suspended in it. The whole apparatus was then carefully arranged between the poles of an electro-magnet specially constructed to give a field as uniform as possible. Before actuating the magnet it was found that the iron remained perfectly passive in presence of the nitric acid until the temperature was raised to 89°, when the usual explosion consequent upon loss of passivity occurred. But on repeating the experiment when a powerful current was traversing the coils of the electro-magnet, effervescence commenced at once, and at 51° the explosion occurred in a most violent manner, projecting most of the liquid out of the tube. The remainder of the liquid, however, remained quiescent until the iron was touched with the thermometer-bulb, when a second explosion occurred. In a third experiment the magnet was not actuated until the apparatus was heated to 60°; but the moment the current was allowed to pass the explosion

occurred instantaneously. Hence it appears that the action of the magnet is to lower the temperature of transition from the passive to the active state. In attempting to determine the cause of this singular phenomenon, it was found that when two iron bars placed parallel to the lines of force in the magnetic field were submerged in any liquid capable of attacking iron, the ends of one bar and the central portions of the other being alone allowed to come into actual contact with the liquid, the bar with ends exposed became in relation to the other as zinc to platinum, so that on connecting the bars by wires a permanent current was found to flow. Hence it is supposed that, in case of a single mass of iron, local currents will be set up between those parts in which magnetic poles are induced and the intermediate parts, and Messrs. Nichols and Franklin are of opinion that these local currents are the cause of the curious behaviour of passive iron in the magnetic field.

A MEETING was held at Philadelphia on December 12 to celebrate the hundredth anniversary of the birth of Thomas Hopkins Gallaudet, the pioneer of the movement for the instruction of the deaf in America. A short biographical sketch of Gallaudet was read, and one of his poems was recited by four deaf girls in the sign-language. Prof. Graham Bell delivered an address, which was interpreted into the sign-language as rapidly as it was spoken, and, according to *Science*, was greatly appreciated by the many deaf persons in the audience. The two sons of Gallaudet, both of whom are engaged in continuing the work of their father—one as the President of the deaf-mute College at Washington, the other as a pastor for the deaf—were present, and made remarks suitable to the occasion.

MESSRS. MACMILLAN AND CO. will publish immediately a new Treatise on Algebra, by Mr. Charles Smith, of Sidney Sussex College, Cambridge, whose previous text-books on Conic Sections, on Solid Geometry, and on Elementary Algebra have been very favourably received. The new book is designed for the use of the higher classes of schools and the junior students in the Universities. One important change is made from the usual order adopted in English text-books on algebra, in that some of the tests of the convergency of infinite series are considered before such series are made any use of. A knowledge of the elementary properties of determinants being of great and increasing practical utility, Mr. Smith has introduced a short discussion of their fundamental properties, founded on the treatises of Dostor and Muir. No pains have been spared to insure variety and interest in the examples, which have been selected from numerous examination-papers and from the mathematical journals.

MESSRS. MACMILLAN AND CO. have in the press a treatise on Higher Arithmetic and Elementary Mensuration, by Mr. P. Goyen, Inspector of Schools in New Zealand. Feeling the defect in most text-books of arithmetic that the worked-out types are all of the simplest character, while the exercises which follow them abound in difficulties, Mr. Goyen has worked out an immensely large number and variety of graduated types, and taken great pains to adapt the exercises to them. In the mensuration, wherever the geometrical proof of a rule is quite simple, it is given. A chapter on surds is inserted, because a knowledge of surd operations is useful in mensuration, and is required in many public examinations.

A GOOD address on some sociological aspects of sanitation was lately delivered before the Philosophical Society of Glasgow by Dr. James B. Russell, President of the Society. This address has now been published. It contains some excellent remarks on the extent to which the State has a right to limit individual freedom in the attempt to establish the conditions of public health.

THE Journal of the Straits Branch of the Royal Asiatic Society (No. 18), just received, contains the Malay text, with an English translation, of "Raja Donan," a Malay fairy-tale. This is one of a series of *cheritras*, taken down, word for word, from the lips of Mir Hassan. Among the other contents of the number are an essay (continued from No. 17) towards a bibliography of Siam, and an English, Sulu, and Malay vocabulary.

THE editorship of the well-known Brunswick scientific journal, *Globus*, changed with the new year, or, rather, with the commencement of the fifty-third volume on December 19. Dr. Emil Deckert takes the place of Dr. Richard Kiepert, and at the same time an alteration is made in the sub-title. Where, formerly, this read, "*mit besonderer Berücksichtigung der Anthropologie und Ethnologie*," it now reads "*mit besonderer Berücksichtigung der Ethnologie, der Kulturverhältnisse, und des Welthandels*," and in an address to the reader the editor and publishers explain that the alteration represents a corresponding alteration in the programme. As before, every effort will be made to supply abundant information of a geographical and ethnological character; but as German national interests have largely developed and extended in the last few years, in future a good deal more attention will be devoted to questions connected, as we understand it, with German possessions and German interests abroad. The practical effects of thus enlarging the scope of the journal are not apparent in the number before us, but it may be hoped that *Globus* will not lose its character as a popular educator in geography and the allied subjects. Why a similar journal has not been established in this country is a mystery.

AN American journal devoted to geology and the allied sciences has just been started. It is called the *American Geologist*, and will for the present be published at Minneapolis, Minn.

MR. A. SIDNEY OLLIFF, of the Australian Museum, Sydney, writes to the January number of the *Entomologist* about giant Lepidopterous larvæ in Australia. The larva of *Chalepteryx collesi*, a large moth which was unusually abundant during the past summer in the vicinity of Sydney, often, he says, attains the length of 7 inches, and is robust in proportion. This moth feeds on various Eucalypti, and is of a rich satiny-brown colour; each segment, except the first, is furnished with eight yellow verrucose spots, which emit long brown bristles; the anal extremity, a yellow band on the first segment, and two additional verrucose spots on the second and third segments also give rise to bristles. The cocoon, as well as the larva of this species, is armed with fine and exceedingly sharp bristles, which, if carelessly handled, readily penetrate the skin, causing considerable irritation. The larva of the beautiful swift (*Zelotypia stacyi*) measures 8 inches when full grown, and Mr. Olliff has seen several *Cossus* larvæ of similar dimensions.

IN the *Entomologist* for January, Mr. Alfred Bell offers some suggestions about post-Glacial insects. So far as his experience goes, insect remains are by no means common, and belong chiefly to the Coleoptera. He gives thirty species, nearly all of which belong to this division of the insect world. As Mr. Bell points out, however, it does not follow that Lepidoptera were not present during the post-Glacial period, since they occur in beautiful preservation in deposits of much older date in England and on the Continent. The nature of the post-Glacial soils was not favourable to the preservation of soft-bodied animals. "Hence," says Mr. Bell, "if anyone knows of Lepidoptera retained in a fossil state, it will be of real service to science if he will say where they were found, and under what conditions."

IN the January number of the *Zoologist*, Mr. Allan Ellison has an interesting article on the autumnal migration of birds in Ireland. He says that the migration movement of last autumn

in Ireland was in all respects a most exceptional one. Some of the migrants appeared unusually early, and all in much larger numbers than Mr. Ellison had ever before observed. On October 8 he saw the first flocks, both of starlings and redwings. On the same day, and for about a week after, immense numbers of golden plovers were passing over, flying towards the west and south-west in large V-shaped strings. This was about the usual time for starlings and redwings, but early for golden plover. On the 11th again both redwings and starlings were constantly passing. On the 16th he observed a great host of fieldfares, many thousands in number, winging their way across the sky towards the south-west. From October 17 to the beginning of November the starling migration was at its height, the flocks being much larger and more numerous than he had ever observed in former years. He saw four within a quarter of an hour on the afternoon of the 18th. At 4 p.m. on the 22nd, the largest flock he ever saw passed over. It was in the form of a column, perhaps nearly a mile long, and must have numbered thousands, spanning the sky from horizon to horizon for more than half a minute, and was followed in a short time by two smaller flocks. All the latter part of October skylarks were from time to time flying over, generally large straggling flocks or scattered individuals, flying nearly out of sight, but their call-notes being distinctly audible. Mr. Ellison hopes that those who are favourably situated for observing the arrival of winter birds will report whether they have noticed a corresponding abundance of migrant this season.

THE new number of *Mind* opens with an able and suggestive article on pleasure, pain, desire, and volition, by Mr. F. H. Bradley. Mr. J. McK. Cattell has an interesting paper on the Psychological Laboratory at Leipzig. Mr. T. Whittaker writes on individualism and State action; and Mr. D. G. Ritchie on origin and validity.

PARIS is soon to have a Museum of Religions. M. Guimet, of Lyons, who has been a great traveller, has been engaged for years past in collecting altars, priests' robes, and other objects relating to religious ceremonies. These objects he presented some time ago to Paris on condition that a building should be specially devoted to them. This building, close to the Trocadéro Palace, has just been finished, and the collection will soon be transferred to it.

THE additions to the Zoological Society's Gardens during the past week include two Spotted Ichneumons (*Herpestes nepalensis*), a —— Fox (*Canis* ——) from Afghanistan, presented by Lieut.-Colonel Sir Oliver B. C. St. John, K.C.S.I.; a Common Otter (*Lutra vulgaris*), British, presented by Mr. Edward Hart; a Red-throated Diver (*Colymbus septentrionalis*), British, presented by Mr. Charles A. Howell; two Greater Sulphur-crested Cockatoos (*Cacatua galerita*) from Australia, presented by Master Rankin.

OUR ASTRONOMICAL COLUMN.

BRAZILIAN RESULTS FROM THE TRANSIT OF VENUS.—M. Cruls, in a note to the Paris Académie des Sciences, states that the reports of the various expeditions sent out by the Brazilian Government to observe the transit of Venus in 1882 are almost entirely printed, and will shortly appear. Three stations were occupied, viz. S. Thomas in the Antilles, Olinda in Brazil, and Punta-Arenas in the Straits of Magellan. The Baron de Teffé was in command of the first expedition; M. J. d'O. Lacaille of the second, and M. Cruls himself of the third. The duration of the transit at Punta-Arenas was nearly the mean duration, both ingress and egress being slightly accelerated. The two more northern stations had the duration much shortened, ingress being retarded, and egress accelerated. The chief observer at each station was supplied with an equatorial of 6·3 inches aperture; and at S. Thomas two other telescopes of 4·5 and 4·1 inches respectively were also used. At Olinda likewise there was a second telescope in use, of 4·5 inches aperture. The method of projection was employed in order to get rid of the physiological effects produced by the intensity of the solar light, and in combining the observations made with telescopes of different apertures, weights were given to them proportional to the square of the diameters of the object-glasses, in accordance with the results obtained from the experiments of MM. Wolf and André. The resulting parallax from the internal contacts is 8″·808.

THE ASTEROIDS.—Prof. Daniel Kirkwood, of the Indiana University, has just issued a short essay on the asteroids or minor planets, this group of tiny bodies being entitled on many grounds to more particular consideration than it has yet generally received. The first part of the essay gives a brief sketch of the history of the discovery of the first five asteroids, together with the names of the discoverers and date of discovery of all as yet known to us, and a table giving the elements of their orbits. Prof. Kirkwood makes it clear that the numbers of those still unknown are practically inexhaustible, for if Leverrier's estimate be correct, that the quantity of matter contained in the group cannot be greater than one-fourth the mass of the earth, it would yet require no fewer than 72,000,000 bodies as large as Menippe to make up this amount. Fortunately the rate of discovery appears limited to ten or a dozen per annum, so that there is no immediate danger of our being overwhelmed by the impossibility of following up some few millions of orbits. The second part of the work deals with questions relating to the origin of the group, and with certain relationships apparent in their orbits, particularly with regard to the irregular distribution of the asteroids in their ring, certain districts being left entirely void, viz. those where the asteroid would have a period commensurable with that of Jupiter. Prof. Kirkwood has on former occasions repeatedly shown how Jupiter would tend to eliminate bodies revolving in these positions by increasing the eccentricities of their orbits until their perihelion distances fell within the body of the sun itself, and he has accounted for the gaps in the ring of Saturn upon a similar principle. Prof. Kirkwood is of opinion that several of our periodic comets may have been originally members of the asteroid family. All the thirteen comets whose periods correspond to mean distances within the asteroid zone have direct motion, and inclinations similar to those of the minor planets, and their eccentricities are generally less than that of other known comets; whilst five of these comets have periods respectively corresponding to some of the most marked gaps in the asteroid zone.

Prof. Kirkwood makes no reference to the importance of certain members of the group as affording means for the determination of the solar parallax, which many astronomers will consider to be their most useful function, and as compensating for the enormous labour, both of observation and computation, involved in following the paths of so great a number of wanderers. And it would have been exceedingly useful if he had supplemented his other tables by one showing those asteroids which have only been observed during one opposition. Some of those theoretically the most interesting have not been observed for several years, and are practically lost to us, and it would seem a matter of more pressing importance at the present time that these should be picked up again, if possible, rather than fresh additions should be made to a list already unmanageably long.

OLBERS' COMET.—The following ephemeris for Berlin midnight, by Dr. Krueger (*Astr. Nach.* No. 2818), is in continuation of that given in NATURE for 1887 December 15:—

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Jan. 6	16 50 20	1 22·7 S.	0·2486	0·3821	0·45
8	16 54 8	1 43·4 S.			
10	16 57 51	2 3·4 S.	0·2583	0·3854	0·43
12	17 1 29	2 23·6 S.			
14	17 5 3	2 41·2 S.	0·2679	0·3884	0·40
16	17 8 31	2 59·2 S.			
18	17 11 55	3 16·6 S.	0·2773	0·3910	0·38
20	17 15 14	3 33·4 S.			
22	17 18 29	3 49·6 S.	0·2866	0·3932	0·36

The brightness on August 27 is taken as unity.

Dr. E. Lamp succeeded in seeing the comet for a short time on December 12, and concludes, from a very rough comparison with a star that the ephemeris then required a correction of + 8s. in R.A., but was practically right in Decl.

THE CLINTON CATALOGUE.—The *Sidereal Messenger* for December announces that the great catalogue of 30,000 stars, upon which Dr. Peters and his assistant, Prof. Borst, have been engaged for several years past, is virtually completed, and ready for the press, and its publication is expected during the present winter. In the prosecution of this work Prof. Borst has gathered the stars from the various astronomical publications of the last fifty years, and reduced them to the epoch of the forthcoming catalogue.

OCCULTATIONS OF STARS BY PLANETS.—Herr A. Berberich calls attention in the *Astronomische Nachrichten*, No. 2814, to the importance of observations of occultations of stars by the planets, and supplies a list of stars which may possibly be occulted by either Venus, Mars, Jupiter, or Saturn, during the course of the present year. Such observations have been extremely rare, yet they would prove extremely important, for they would throw light on the extent and density of the planetary atmospheres, and would afford a means in the cases of Mars and Venus for the determination of parallax and diameter. Herr Berberich adds that in the case of the three outer planets the occultation of a star by the primary would afford a specially favourable opportunity for the determination of the positions of the satellites, since micrometer measures of their places as referred to the occulted star would be free from many errors to which the direct comparison of the planet and its satellites is exposed.

The following stars may possibly undergo occultation during the next fortnight:—

Planet.	G.M.T. of Conjunction in R.A.	Star.	Mag.	Pl − ✱ Δδ	Max Duration.
	h. m.	°		′	m.
♀	Jan. 5 16 29·2	S.D. −17 No. 4187	9·7	−0·13	6·0
♀	9 18 1·4	18 4279	9·5	+1·05	5·8
♂	12 3 41·4	4 3445	9·3	−0·18	7·4
♀	12 8 32·3	19 4401	9·3	+0·84	5·7
♀	14 18 40·8	19 4441	9·5	−0·19	5·6
♀	15 1 31·9	20 4446	9·5	+0·38	5·5
♀	17 23 22·5	20 4635	9·3	−0·57	5·4

The maximum duration is the interval between immersion and emersion for a central occultation.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JANUARY* 8–14.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 8

Sun rises, 8h. 7m.; souths, 12h. 6m. 49·1s.; sets, 16h. 7m.: right asc. on meridian, 19h. 16·8m.; decl. 22° 17′ S. Sidereal Time at Sunset, 23h. 18m.

Moon (New on January 13, 9h.) rises, 2h. 14m.; souths, 7h. 34m.; sets, 12h. 44m.: right asc. on meridian, 14h. 43·4m.; decl. 10° 21′ S.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	7 55	11 40	15 25	18 50·0	24 27 S.
Venus.....	4 36	9 1	13 26	16 10·3	18 29 S.
Mars......	0 11	5 53	11 35	13 1·9	4 14 S.
Jupiter....	4 16	8 38	13 0	15 47·4	19 4 S.
Saturn....	17 28*	1 19	9 10	8 27·1	19 39 N.
Uranus...	0 23	5 55	11 27	13 4·3	6 8 S.
Neptune..	12 51	20 31	4 11*	3 42·5	17 56 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultation of Star by the Moon (visible at Greenwich).

Jan.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
9	η Libræ	6	6 18	6 46	341 298

Jan.	h.	
9	11	Jupiter in conjunction with and 4° 12′ south of the Moon.
10	0	Venus in conjunction with and 2° 16′ south of the Moon.

Variable Stars.

Star.	R.A.	Decl.		
	h. m.	° ′		h. m
U Cephei	0 52·4	81 16 N.	Jan. 10,	22 22 *m*
ζ Geminorum	6 57·5	20 44 N.	,, 9,	3 0 *m*
			,, 14,	3 0 *M*
R Canis Majoris	7 14·5	16 12 S.	,, 11,	22 53 *m*
			,, 13,	2 9 *m*
U Monocerotis	7 25·5	9 33 S.	,, 11,	*m*
S Cancri	8 37·5	19 26 N.	,, 9,	23 12 *m*
R Leonis	9 41·5	11 57 N.	,, 8,	*m*
R Ursæ Majoris	10 36·7	69 22 N.	,, 8,	*M*
T Ursæ Majoris	12 31·3	60 6 N.	,, 12,	*m*
W Virginis	13 20·3	2 48 S.	,, 8,	22 0 *m*
δ Libræ	14 55·0	8 4 S.	,, 11,	4 34 *m*
U Coronæ	15 13·6	32 3 N.	,, 8,	0 45 *m*
U Ophiuchi	17 10·9	1 20 N.	,, 8,	4 38 *m*
		and at intervals of		20 8
T Vulpeculæ	20 46·7	27 50 N.	Jan. 11,	0 0 *m*
			,, 12,	1 0 *M*
Y Cygni	20 47·6	34 14 N.	,, 10,	21 7 *m*
			,, 13,	21 0 *m*
W Cygni	21 31·8	44 53 N.	,, 14,	*M*
δ Cephei	22 25·0	57 51 N.	,, 8,	4 0 *M*
			,, 11,	22 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ξ Virginis	174°	9° N.	January 11.
,, ζ Boötis	220	14 N.	Very swift; streaks.
,, β Boötis	222	42 N.	Very swift; streaks.

DUNÉR ON STARS WITH SPECTRA OF CLASS III.[1]

I.

IN publishing a few days before his death the last part of his discoveries relating to the spectra of stars of the third class, D'Arrest pronounced the opinion that henceforward there would be nothing essential to add to the knowledge then possessed of the stellar spectra of this class in the northern heavens. When D'Arrest died, 123 well-developed objects of Class III.*a* were known, and counting all the objects known, 150; the stars known in Class III.*b* were 23. Actually, the well-developed stars of III.*a* are 214, and if all are reckoned, 475; the stars of III.*b* are 55 at least.

The number of objects in Class III. with which we are acquainted has been tripled by recent researches, but, besides, the relation between the numbers of the stars in the two lower classes has been considerably altered, considering that at present there are 8·5 stars III.*a* instead of 6·5, to 1 star III.*b*. However, we should commit a serious error if we drew the conclusion that in reality the spectra III.*b* were not more than nine times rarer than III.*a*. On account of the enormous width of the bands, one is able without any difficulty to recognize the nature of a spectrum III.*b* in very faint stars, which one is not able to do in III.*a*, unless in the rare objects of this class in which the bands are more marked and broader than usual.

I find this opinion confirmed by the fact that the researches of M. Vogel give more than 200 new spectra III.*a*, and have scarcely led to an acquaintance with one new spectrum III.*b*. It is very probable therefore that we are already acquainted with all these stars to the magnitude of 7·5 inclusive; this is rendered still more probable by the following table, which gives the number of the stars III.*a* and III.*b* belonging to different magnitudes:—

Magnitude.	Class III.*a*. Observed.	Class III.*a*. Calculated.	Class III.*b*. Observed.	Class III.*b*. Calculated.
1·0–1·9	2	1	0	0
2·0–2·9	5	3	0	0
3·0–3·9	9	11	0	0
4·0–4·9	31	28	0	1
5·0–5·9	88	90	2	2
6·0–6·9	134	380	11	8
7·0–7·9	151	—	18	24
8·0–8·9	37	—	14	—
9·0–9·9	18	—	10	—

[1] We have already referred generally to M. Dunér's important memoir published in the Transactions of the Swedish Academy. We now give a translation of his general conclusions.—ED.

In the columns headed "Calculated" are the numbers obtained by multiplying by 4/3 the numbers of the stars in the classes of different magnitude given in the "Wunder des Himmels," by Littrow, 5th ed. p. 577, deduced from the *Durchmusterung* of Argelander, and then dividing the result by 15 and 750 respectively. For the stars III.*a* the agreement is almost perfect up to the magnitude of 5·9 inclusive, and for III.*b* up to 6·9; up to 7·9 the agreement is pretty good, whilst after that the numbers observed are more and more in arrear of the numbers obtained by calculation. We may conclude therefore that our knowledge of the spectra III.*a* is almost complete up to the magnitude of 5·9 inclusive, and of spectra III.*b* up to 7·5 inclusive. The researches of M. Vogel have not added any new star III.*a* with a magnitude higher than 5·0, and only very few between 5·0 and 6·0, and, as I have already said, no new star III.*b* above the magnitude 7·5, although he has examined all the stars up to this limit of magnitude between − 2° and + 20° of declination. As to the difference existing between "observation" and "calculation" in the case of the III.*a* feebler than 6·0, we must remember that as yet no systematic spectroscopic research has been made of the stars between − 2° and − 23°, nor between + 20° and the North Pole. Consequently the number of stars III.*a* between 6·0 and 7·5 will probably be much increased before very long, and will approach the theoretic number. On this account I imagine the stars of III.*b* are fifty times rarer than those of III.*a*.

The list of these rare stars is probably already very complete for that part of the sky visible in Europe, for the nearer the researches of an astronomer are to the present time the feebler are the stars with spectra of this class which he discovers (Secchi 6·7, D'Arrest 7·0, Vogel 7·1, Duner 8·3, Pickering 9·1). The conclusions, therefore, which we can draw as to the manner in which these stars are distributed over the heavens deserve some confidence. Such a research is very interesting. We have already seen that the principal bands in these spectra owe their origin to the presence of a carbon compound in the atmosphere of the stars. It is important to know whether there is a certain direction in the heavens in which these stars are more numerous than in others, especially when we consider that the same substance is present in comets, which come from interstellar space. I have made such a research, and have come to the conclusion that the objects in question are grouped similarly to stars in general, being closer together in the neighbourhood of the Milky Way. Setting out from the position of the Pole of the Milky Way given by Heis, R.A. = 12h. 42m., Decl. = + 26° 8′, for the equinox 1900·0, I have calculated the quantities P, or the distances of the stars from this Pole, given in my catalogue. But to have my list a little more complete for the part of the heavens invisible in Europe, I have calculated the same quantity also for the following stars, whose spectra have been examined by M. Pechülé ("Expédition Danoise pour l'Observation du Passage de Venus," 1882, pp. 40–43).

Star.	Magnitude.	R.A. 1900·0. h. m.	Decl. 1900·0. ° ′	P. °
65 Schj.	8	5 40	−46 30	60·6
103 Schj.	8	7 54	−49 43	80·6
125	7·5	9 51	−41 7	78·6
126	8·5	9 57	−59 45	87·0
128	7	10 8	−34 30	71·5
130	6·5	10 31	−39 3	72·6

By the help of the P's found, I have obtained the following table, which indicates the numbers of those stars which are between the different limits of distance of the Poles, boreal or austral, of the Milky Way.

Limits of Polar Distance. ° °	No. of Stars.	Mean Magnitude.
0–35	3	6·6
35–60	8	6·6
60–70	8	7·2
70–80	13	7·4
80–90	29	8·3

It is at once seen that there is an immense accumulation between 80° and 90° of polar distance, and that the polar regions are totally empty up to 19° distance from the Pole; and this relation would doubtless become still more striking if our knowledge of these stars which are invisible in Europe was more complete; for, whilst the two Polar regions are for the most part visible, a great part of the Milky Way is always below our horizon. Besides the number of stars in the different zones, I have also calculated their mean magnitudes, and it will be seen that for them, as well as for other stars, there is this rule—that in the Milky Way the faint stars are much closer together than in the neighbourhood of its Poles.

One might perhaps suppose that there is a certain portion of the Milky Way where the stars III.*b* are more frequent than elsewhere. In order to decide this it is necessary first to calculate for each star the quantity which has the same relation to the Milky Way as the right ascensions have to the equator; and then make a table, on the distribution, having this quantity as its foundation. Such a research cannot, however, lead to good results as long as our acquaintance with the stars between 25° of south declination and the South Pole is almost *nil*. I will only say, then, that there is a great number of these stars around R.A. 305°, Decl. + 40°, but almost an equal number around R.A. 85°, Decl. + 25°. Now both these points are precisely those in which, in the northern hemisphere, the stars are closest together. It seems that they are grouped almost according to the same laws as all other stars, and that, properly speaking, there is no region where stars of the Class III.*b* abound.

A similar research of the stars III.*a* could not give exact results, as our acquaintance with these stars below magnitude 6·0 is still too imperfect. However, the researches which M. Pechülé undertook, with the aid of the *Uranometria Argentina*, on the distribution of the coloured stars, render it probable that these also are closest together in the neighbourhood of the Milky Way.

I have already said that in all probability the spectra of fixed stars must be subject to variations on account of the diminution in the temperature of stars which must take place sooner or later, and I observed that it is precisely on the supposition of such a diminution that the classes of M. Vogel are based. There are, however, eminent *savants* who have combated the correctness of this opinion, and who have formed ingenious hypotheses to prove the possibility that the sun, and consequently the stars also, may regain the heat which emanates from them. But it would be too much to say that these theories have victoriously withstood criticism, and the spectroscopic examination of the stars has given results fatal to them. Although the spectra of stars may be divided into very distinct classes, according to their characteristics, there are, on the other hand, numerous spectra of all possible grades between any two classes, so that it may be difficult, if not impossible, to decide to what class a star belongs, and that even when it is sufficiently brilliant for all the details of its spectra to be distinctly recognized. Besides, we see that the more the star resembles the first class, the brighter is its violet part, whereas the violet part becomes fainter and fainter or even invisible when the spectrum resembles that of α Orionis (III.*a*). On that account it seems certain that the spectra owe their characteristics to the greater or less degree of incandescence of the stars, so that the temperature of stars of Class III. must be relatively low.

Doubtless these changes do take place in the stellar spectra, although we must suppose that, as regards the spectra of the first two classes, they are almost exclusively secular, and operate so slowly that millions of years may pass before they become apparent.

It is different with stars of Class III. These being probably already much cooler than the others, we may reasonably expect that the changes will take place more rapidly, and perhaps also that from time to time temporary augmentations in activity will take place on their surface, followed by periodic changes in their spectra.

In the course of his observations Secchi arrived at the conclusion that the colours and spectra of these stars were subject to remarkable changes in a very short period. My observations led to the same conclusion, if observations from the years 1866 to 1874 may be trusted without reserve. For, without counting the few and unimportant discrepancies which I discovered between the aspect of several spectra and the descriptions given by earlier observers, I found that there are forty stars which have been comprised in Class III., among which there is scarcely one which now belongs to it, and there are some which ought to have been transported from one sub-class into the other. But, for reasons which I will here explain, such a conclusion would certainly be too hasty.

On the one hand, Secchi's observations date from a time which we may call the infancy of spectrum analysis, and the instruments employed were very imperfect; on the other hand, he was the first to introduce a classification of the stars according to

their spectra. Therefore it is easily understood that Secchi was only able to seize gradually the characteristics of the different types (thus it was not until late that he introduced the fourth type); and again, he once changed the order so that the second and third types changed numbers. On account of this change, some errors may have found their way into his publications. Some even may be explained without having recourse to this supposition. All the spectra which I have excluded from the third class are, according to Secchi, indeterminable, except two, which I consider intermediate between II.*a* and III.*a*, and the two stars R.A. = 9h. 18m., Decl. = − 21° 50′, and R.A. = 18h. 14m. 40s., Decl. = + 25°·2, which Secchi found independently of Schjellerup's catalogue of red stars. It might happen then that with a clear sky faint bands might be perceived,[1] and as to the last we may well suspect that there is some gross error in their positions, judging from what Secchi says as to the manner in which he discovered the first of them.[2] Such a supposition would not be admissible for the star 249 Schj. This star is situated, according to Herschel, amongst a mass of stars, and Secchi says of it, "Stella di 9ª rossa con rigoni nello spettro 4° tipo certamente" ("Memoria Seconda," p. 52). I have often examined the cluster in which this star was situated, but without perceiving it. We may therefore believe that it is variable of long period.

I still have to refer to the stars which ought to have been transferred from one *sub*-class to another. In the spectra of these, variability seems to me quite inadmissible, the two subclasses being, as I shall try to prove soon, co-ordinated, and not successive phases of development which every star must undergo. I suppose that at the commencement of his observations of spectra of the third class, when Secchi met with stars III.*b* not very well marked, he did not think them different from III.*a*, and he did not perceive the difference until after having seen several spectra of this class as pronounced as those of 78 and 152 Schj. On the spectrum of the latter he still says in 1867, "In conclusione è tipo di *a* Ercole ma con zone vere mancanti" ("Catalogo," pp. 14, 15).

However, neither Secchi nor even D'Arrest examined a sufficient number of spectra III.*b* to thoroughly understand their characteristics. Both appear to admit that there are fundamental differences between spectra belonging to it. For instance, Secchi says of the spectrum of star 136 Schj., "E difficile dire se sia proprio del 4° tipo" ("Memoria Seconda," p. 42), and of the stars discovered by Wolf and Rayet with bright lines which are not hydrogen,[3] and dark bands in the spectra, and therefore certainly belonging to Class II.*b*, he says, "Accenneremo qui soltanto che esse appartengono al 4° tipo, ma sono di quelle a zone molto irregolari" ("Sugli spettri prismatici delle stelle fisse," p. 194), and "Ad ogni modo sono di 4° tipo, e le righe paiono del carbonio diretto" (*l.c.* p. 216). D'Arrest speaks as follows of the star 74 Schj., "Irregular spectrum of type IV." (*A.N.* 2016); and of the star 155*b* Schj., "Very remarkable spectrum, &c." (*A.N.* 2009). By collecting all my observations on all the spectra of this class it is seen that not one of them really departs from what may be called the normal spectrum. Doubtless there are in different stars notable differences in the darkness of the flutings, and in the brilliancy of the intervals, but all this does not prevent all the spectra being formed according to one constant type, as happens with Class III.*a*. Besides, Secchi seems to think that the aspect of a spectrum may change completely with the kind of spectroscope used. He says of the spectrum of 132 Schj., "Tipo 4° ben deciso, . . . oculare cilindrico. Coll' oculare piccolo sferico tutto questo era sparito e si credette tipo 3°." When instruments are used which give so vague an appearance to an exceptionally well-defined spectrum, presenting essential characteristics, it is easy to commit serious errors in judging of the spectra examined. Therefore I cannot see that the discrepancies which exist between Secchi's observations and mine are a sign of variations in the stellar spectra, although no doubt it is prudent to occasionally examine the stars concerning which these discrepancies have arisen.

It is quite a different case with the discrepancies that I have found between my observations and those of D'Arrest, who was supplied with excellent instruments, and was a most careful and skilful observer. It is necessary therefore to examine more closely into the cases in which differences exist. There are three, two of which concern the stars 24034 Ll. = Weisse XII.ⁿ 793 and DM. + 60° 1461 = A. Oe. 13681. D'Arrest says that the latter has "a bright well-marked spectrum of type III." (*A.N.* 2044) and the former "a clear, fluted spectrum, the flutings being very distinct although pretty fine, III." (*A.N.* 2009). I found both nearly white, and their spectra II.*a*, or continuous. It is true that I examined the positions of these stars by the help of the two catalogues in which they are, and obtained the same positions, nevertheless one is tempted to believe that D'Arrest made some error in the identification of these stars, especially as he did not observe them several times. This supposition is quite inadmissible, however, for the star DM. + 36° 2772 = Ll. 30500, for here D'Arrest expressly says (*A.N.* 2009), "8·3 mag. with beautiful column-like spectrum. It is one of the stars accompanying the great Hercules nebula." I have calculated the position of this star with the help of the catalogue and of Ll., and besides that I examined all the stars in the neighbourhood of the great cluster in Hercules without finding one of Class III.*a*. The star DM. + 36° 2772 is of orange colour, but its spectrum is continuous, or at most II.*a* very poorly developed. As regards this spectrum I shall not even attempt to explain the difference between what I have seen, and the description given by D'Arrest. A variability of the spectrum seems really probable, and the star is certainly deserving of much attention. Besides this star there are others whose spectra I found very feebly developed, whilst D'Arrest says that they are beautiful or even superb. This also may be regarded as a sign of variability, and a fact which also supports this supposition is that D'Arrest has made his observations under atmospheric conditions generally regarded as similar to those found at Lund with a spectroscope of similar construction to one of ours, and a telescope very little larger than the one which I used. But it is remarkable that whereas I have often found expressions used by D'Arrest to describe spectra stronger than I should have used, the contrary is of very rare occurrence. It is possible, therefore, that the differences are only apparent, and that either D'Arrest's observations were made under exceptionally favourable circumstances, or mine under very unfavourable ones. The latter supposition is scarcely probable however, for when such differences have occurred I repeated the observation several times; and besides, Vogel's observations on the stars between − 2° and + 20° agree almost without exception with mine. It appears, moreover, that very often D'Arrest only made one observation of the same star; and, without depreciating his researches, it seems to me more probable that there are small and rare inexactitudes in the observations, than that such great changes have taken place in the stars themselves in the short period of ten years.

My researches already contradict the hypothesis that important changes in the stellar spectra take place so rapidly. My observations embrace a period of six years, and a much larger number of objects than either D'Arrest or Secchi examined. But there is no spectrum in which my latest observations have differed sensibly from my first ones. It is true that my first observations on the spectrum of R Crateris are in direct opposition to the last, the former making its spectrum III.*b* and the latter III.*a*. But that is in no way a sign of variation in the spectrum. When the bands in the spectrum III.*a* of a faint star are exceedingly broad in the green-blue, it is easy to fall into the error of regarding it as III.*b*. At the time of M. Vogel's first observation he also believed that the spectra of stars DM. + 14° 2525 and DM. + 17° 3940 were III.*b*, and it is true that these two spectra, especially the last, are strikingly like the spectra III.*b* in spectroscopes of small dispersion.

It was in order to escape such errors that I determined in the spectra of most of the faint stars of Class III.*b* the approximate wave-lengths of their principal flutings; the wave-length of band 6, and also that of band 10, being a sure mark that the spectra belonged to this section of Class III. This deceptive appearance generally disappears when the star is examined with spectroscopes of considerable dispersion. I am therefore at present of opinion that, excluding the new stars and perhaps η Argûs, which seems to belong to the same category, we have no reason to believe that great and rapid changes take place in the stellar spectra, although it must be confessed that the observations of certain stars, especially DM. + 36° 2772, are such as to render such changes very probable.

(*To be continued.*)

[1] M. Vogel has as a matter of fact seen feeble bands in the spectrum of one of them − 60 Schj., while in the case of ten stars his observations confirm my own; in the spectrum of another star M. Pechülé has not seen any bands.

[2] "Trovata cercando 124 Schj." The position of this star differs by 27m. and 40′ from that of the star in question.

[3] In his observations at Vienna with the great refractor, M. Vogel was able to see the lines of hydrogen either C or F in the spectra of all three stars; they were, however, feeble in comparison with the other bright lines.

THE ART OF COMPUTATION FOR THE PURPOSES OF SCIENCE.

I.

THE art of computation as distinguished from the science of arithmetic it so generally neglected in our ordinary courses of education, that most men and almost all women feel the greatest difficulty and repugnance in dealing with figures. The causes of and remedies for this deficiency are discussed at some length in a paper "On teaching Arithmetic" (*Journal of Education*, May 1885), and the following remarks refer specially to the requirements of students of science.

I must apologize for the use in proving my case of some names of high and well-deserved repute. Instances are given, as far as possible, which have been publicly acknowledged or corrected, with the full admission that this paper is itself a house of glass, and that any stone may impinge even upon Newton, since, as Lord Lytton tells us, "that great master of calculations the most abstruse could not accurately cast up a sum in addition. Nothing brought him to an end of his majestic tether like dot and carry one."

In 1867 Mr. Stone pointed out two numerical errors in Leverrier's determination of the solar parallax.

Prof. J. D. Van der Plats writes (*Chemical News*, July 30, 1886):—"The verification to which I have submitted the calculations of M. Stas seems superfluous seeing that it deals with the experiments of a *savant* who has never had an equal in exactitude. It may perhaps astonish some as much as it did me to find that the original memoirs contain numerous arithmetical mistakes, as well as typographical errors, of which some are considerable."

Mr. J. Y. Buchanan writes (NATURE, vol. xxxv. p. 76):—"There is a statement in NATURE for November 11 that the weight of the column of water between 20 fathoms and 70 fathoms from the surface under the westerly equatorial current is only 88 per cent. of the weight of the same column under the easterly counter equatorial current. I regret that a serious arithmetical error occurs in the calculation on which this statement was founded. There is no such considerable difference of weight in the two columns of water." Suppose at the equator the Guinea current flows from west to east at the rate of 40 knots in twenty-four hours, and that the equatorial current flows at 30 knots in twenty-four hours in the opposite direction. The opposite directions of the two currents cause an additive and subtractive difference in the tangential velocity of the particles of water due to the rotation of the earth, and therefore an apparent difference in the acceleration due to gravity of about 1/46000, or a pressure equal to that of an additional 1/13 of an inch of water on the column of 50 fathoms.

On page 84 of the *first* edition (the second has been corrected) of Prof. Huxley's admirable "Physiography," we read:—"The weight of air on a square mile is about 590,129,971,200 lbs., and the carbonic acid which it contains weighs not less than 3,081,870,106 lbs., or about 1,375,834 tons. The weight of the carbon in this carbonic acid is 371,475 tons."

This short statement contains excellent examples of many of the common arithmetical slips and errors.

The first number is ten times too great, and not quite accurately calculated from the data $(5280)^2 \times 144 \times 14{\cdot}73 = 59{,}133{,}431{,}808$. Multiplying this by $\frac{\cdot 05321}{100}$, the proportion by mass of carbonic acid in the air, we obtain 31,464,899; here, besides a slip, the number is again multiplied by ten. The pounds are reduced to tons correctly, but there is a slip in the reduction to carbon, since

$$\frac{1{,}375{,}834 \times 3}{11} = 375{,}227.$$

Many more instances might easily be brought forward, but the above will suffice to prove that even the highest attainments in science are too often accompanied by inaccuracy in arithmetic. The causes of this defect have been frequently discussed, but, with the exception of De Morgan and his pupils, little advance in the methods of teaching arithmetic seems to have been made since the days of Recorde and Cocker.

The teachers of arithmetic in our public and higher-grade schools are usually good mathematicians who, in their own school-days, have been hurried through the hated subject to higher work, and have had no subsequent experience in the practical computation required in the laboratory, workshop, or counting-house. When compelled to work out a sum for themselves, the theory is supplied by their knowledge of algebra, and the practical work by a table of logarithms. When brought face to face with the fact that their pupils dislike and are very weak in arithmetic, they fall back upon the stock argument that they teach arithmetic as a training for the mind, and not as a useful art. In too many cases it is to be feared that they are not teaching arithmetic at all.

The great majority of the text-books in common use seem to be defective from the point of view of a student of science in at least three points.

More than half the rules and examples are devoted to money, and arithmetic is treated as though it applied only to pounds, shillings, and pence.

Secondly, few give any suggestion as to the use of tables in lightening arithmetical work, and a boy leaves school disgusted with long rows of figures in which he sees no utility, and without any idea to how large an extent the work could be lightened.

Lastly, the various methods of dealing with approximate quantities are omitted, and a painstaking boy calculates vast collections of figures of which only two or three have any meaning.

Thus Prof. Huxley gives the tenth figure, 6, in the expression for the amount of carbonic acid on a square mile, ignoring the facts that while the percentage of carbonic acid varies in the first figure, its density is not known to the fourth, and the pressure of the air varies in the second.

It is convenient to bear in mind the following simple rules, due, I believe, to De Morgan. If two numbers, a and b, each true to the first decimal place, are multiplied together, the result is true to $\frac{a + b}{20}$ only; a second true decimal in *each* number makes the result ten times more correct, and so on. In dividing a/b where each is true to the first place, the result is true to $\frac{a + b}{b^2 \times 20}$ and so on. Any attempt at greater accuracy in calculation than is indicated by these results should be avoided, since it only precludes the use of cheap and handy tables, tires the calculator making him more liable to error in the important figures, and tends to give quite a false idea of the accuracy of the experiments on which the calculations are based; unless, indeed, we take seriously the answer of Dulong when asked why he always gave his results to eight figures, "I don't see why I should erase the last decimals, for, if the first figures are wrong, possibly the last are correct."

The natural tendency of the human mind, even if controlled by mathematical and scientific training, is to exalt the accuracy of one's own experiments. This is well shown by Prof. Ramsay and Dr. Young in discussing the vapour-tension of liquid benzene (Proc. Phys. Soc., January 1887):—

"A curve was drawn to represent these (experimental) relations, and from it three points were chosen, 0° C. 26·54 mm., 40° C. 180·2 mm., and 80° C. 755 mm. The constants for the formula $\log p = a + b\alpha^t$ are $a = 4{\cdot}72452$, $\log b(-) = 0{\cdot}5185950$, $\log \alpha = \bar{1}{\cdot}996847125$." *Nine* places of decimals are given with apparent confidence, when (1) only three of the whole number of experiments were made even in duplicate; (2) the last pressure, 755, was obtained not by experiment at all, but by extrapolation from a freehand curve, the highest experiment being 79°·6 and 743·1 mm.; (3) a difference of $\frac{1}{8}$° at low temperatures produced no change in pressure which was appreciable by the apparatus used. With the above-mentioned constants the author's calculate for 60° C. 388·51 mm. Using their data and a table of four-figure logarithms, I find $a = 4{\cdot}7239$, $b = -3{\cdot}3$, $\log \alpha = \bar{1}{\cdot}99684$, which gives for 60° C. 390 mm. Regnault gives 390·1 mm.

Under suitable conditions the observation of one quantity can be made with great exactness. It is possible that Sir George Airy estimated 1/100 of a second in a day, or 1/8,640,000; that a balance can be made to estimate 1/1,000,000 of the load, though those of Stas were only accurate to 1/825,000; and that Sir J. Whitworth measured the 1/1,000,000 of an inch. These cases, however, are exceptional, and give quite a wrong idea of the accuracy attainable in ordinary observations and experiments, when several operations, each liable to error, have to

be performed, and various corrections introduced by calculation from extraneous data.

The more closely we examine work of the highest accuracy the more convinced we become of the truth of the statement of Thomson and Tait (p. 333): "Few measurements of any kind are correct to more than *six* significant figures." Thus the number of inches in a metre was found by Capt. Kater in 1821 to be 39·37079, and by General Clarke in 1866 to be 39·37043; this fundamental datum therefore is affected by a doubt of nearly 1/100,000, which of course affects all results dependent on it. In 1856 Miller found that a cubic foot of water at 62° F. weighs 62·321 lbs. From Kater's result a cubic foot contains 28·3153 cubic decimetres, and the mean of a large number of experiments, especially those of Lefevre Gineau, and Kupffer, make the cubic decimetre of water at 4° C. to weigh a kilo = 2·20462125 lbs. according to Miller. Hence a cubic foot of water at 4° C. weighs 62·4255 lbs.; and taking the expansion from Förster (1870), which is nearly identical with that used by Miller, the weight at 16°·67 C. becomes 62·355 lbs.; or about 1/2000 heavier than Miller's determination. But these are the results obtained by picked men under all conditions to insure the greatest accuracy. Results which agree to two or three in the fourth figure show an exceptionally good chemist, while a physicist must be careful indeed to obtain numbers concordant to the fifth figure.

For practical purposes, then, calculations in science may be divided into two classes. The great majority of experiments in physics, chemistry, biology, geodesy, mensuration, navigation, and crystallography are not to be trusted beyond the fourth or fifth figure. Hence a similar accuracy in calculation is all which is required. Some few experiments in each branch—such as the work of Kater, Regnault, Stas, some observations in astronomy, and a few reductions in sociology—may require six or eight figures to be accurately dealt with.

In pure mathematics, of course, numerical results may be pushed to any extent compatible with even the partial sanity of the calculator.

The following suggestions are intended to assist such of my readers as are not mathematicians in working sums of each class by the aid of tables.

Mechanical aids, such as slide rules, arithmometers, and the like, are purposely omitted, since they would require a paper to themselves. The objection to the larger and more powerful is that they are expensive and complicated; that they require a good deal of practice on the part of the operator to give accurate results; and that they are not readily adapted to work shorter sums than they are intended for. On the other hand, a slide rule is an almost indispensable servant when once one has learnt the use of it for dealing rapidly with comparatively small numbers; for large numbers it becomes very cumbrous.

The two cardinal points in approximate working are the short methods of multiplying and dividing decimals suggested by Oughtred in 1631, and strengthening the last figure retained when the first omitted is above 4. For greater accuracy it is well to mark all strengthened figures, and to allow for an excess or defect of them; as a further security one figure beyond what is required may be calculated.

Tables of the multiples from 1 to 9 of numbers which frequently occur are of great assistance especially when the calculator is tired. They are easily made by repeated additions or by the use of the convenient "automatic multiplier" of Mr. Sawyer, which is merely a modern adaptation of Napier's bones.

The due use of complements and reciprocals saves a good deal of time in subtraction and division.

Tables for general use and special purposes are very numerous. For our present purpose they fall naturally into three classes. Five kinds of tables should be in the hands of all calculators:—

1. Multiplication tables such as those of Crelle, by the aid of which three figures may be dealt with at once with greater certainty than is usually the case with one. Tables of primes and factors are not much required for scientific purposes.

2. Reciprocals, which reduce division to the short multiplication of decimals, render the addition of fractions easy, and assist chemists in percentage compositions.

3. Squares, cubes, square roots, cube roots. For most purposes in chemistry and physics a small table up to 100 is sufficient, especially when aided by the following convenient method of approximating to a cube root. If a^3 be the nearest exact cube to the given number N, $N = (a \pm b)^3 = a^3 \pm 3a^2b + 3ab^2 \pm b^3$, or if b be small, $\pm b = \frac{N - a^3}{3a^2}$. Thus to find $\sqrt[3]{28}$, $a = 3$, $b = \frac{28 - 27}{27} = \cdot 037$, $\therefore \sqrt[3]{28} = 3\cdot 037$ instead of $3\cdot 0366$.

De Morgan's edition of Barlow is very convenient, and suffices for all ordinary purposes.

4. Common logarithms to four and five figures. Four-figure tables are perhaps most convenient on one face of a card. Hoüel's reprint of Lalande, with some changes and many valuable additions, is cheap and most convenient in form; it quite suffices for all common work.

For the reasons already mentioned seven-figure tables are unnecessarily cumbrous and expensive for ordinary work. They should never be put into the hands of beginners, as is now the usual practice. Experience shows that boys learn the method of using and appreciate the value of logarithms far more readily than is generally supposed.

5. Gauss's sum and difference logarithms are valuable in dealing with certain trigonometrical formulæ and with questions of expansion.

In the second class may be placed those general tables which are less commonly required, such as:—

1. Powers of 2 and other numbers. Cohn tells us that some varieties of Bacterium multiply by fission every hour, hence by the end of a day one individual would increase to $2^{24} = 16,777,216$. We may therefore cease to wonder at the rapid spread of some forms of infection.

2. Factorials are required in solving permutations and combinations, and therefore in all questions relating to probabilities. Hatchett recommended that a systematic examination of all possible alloys of all the metals should be undertaken. He forgot to remind anyone who attempted to follow his advice that if only one proportion of each of thirty common metals were considered, the number of binary alloys would be 435, of ternary 4060, and of quaternary 27,405. If four multiples of the atomic weight of each of the thirty metals be taken, the binary compounds are 5655, ternary 247,660, quaternary 1,013,985.

3. The sums of arithmetical series are so readily obtained that they are rarely tabulated.

4. Geometrical series are required in certain social questions, such as the increase of population and the output of coal. Tables of the sums of these series when the ratio is nearly one are common, and of considerable use in some scientific problems.

5. For some purposes it is convenient to express numbers in a scale different from the common decimal one.

Thus (Clerk Maxwell, "Elementary Electricity," p. 180) a series of resistance coils are best arranged according to the powers of 2, since the smallest number of separate coils is required, and they are most readily tested. The same is true for a set of weights. Thus to express from 1 to 100 gm. 9 weights are ordinarily provided; 9 weights in the scale of 2 will express up to 511 gm., while 7 weights suffice for 100 gm., since 100 in the scale of 2 is expressed by 1100100.

6. The curious theory of trees due to Profs. Cayley and Sylvester (B. A. Report, 1875) seems to promise the possibility of computing the number of possible compounds formed by elements of given valency. Thus x atoms of tetravalent carbon will combine with monad hydrogen to form N compounds.

x	N	x	N	x	N.
1	1	6	18	11	1346
2	1	7	42	12	3326
3	2	8	96	13	8329
4	4	9	229		
5	9	10	549		

If of the first thirteen paraffin hydrocarbons alone there are 13,952 possible forms each with its own series of derivatives, there seems little chance of chemists having nothing to do for some time to come.

7. Natural logarithms are required by some formulæ, and are at times more convenient than common logarithms.

According to Haughton ("Animal Mechanics," p. 282), the study of the action of certain muscles requires the use of natural logarithms.

The ratio of the mean absolute pressure P to the initial absolute pressure p in a steam-cylinder at the given rate of expansion r is expressed by $\frac{P}{p} = \frac{1 + \text{nat. log } r}{r}$.

Weldon supposed (B. A. Report, 1881), that some power of

the atomic weight (X) of each of the first fourteen elements in Mendelejeff's classification is a simple multiple of the same power of the atomic weight of lithium. Or $X^x = m7^x$

$$l.\ X = 1{\cdot}9459 + \frac{l.\ m.}{x}$$

It is easy to see that since x may be any whole number, and m any small whole number, X may have any value whatever within the limits of errors of experiment; or the relation is fanciful rather than real.

S. Values of the definite integral $\frac{2}{\sqrt{\pi}}\int_0^x e^{-x^2}dx$ representing the probability curve, upon which the whole science of the adjustment and comparison of quantitative experiments is based.

Tables of the third class, which offer special facilities to those engaged in any one kind of work, are very numerous.

The physicist has Rankine's "Rules and Tables," Everett's "Units and Physical Constants," Hospitallier's "Formulaire de l'Electricien," and many others.

The chemist has Biedermann's "Kemiker Kalendar," the "Agenda du Chemiste," and various tables for analysis, such as those at the end of Fresenius.

The needs of both physicists and chemists are more or less supplied by Landolt and Bornstein's "Tabellen," the "Annuaire du Bureau des Longitudes," and my own more portable "Numerical Tables and Constants in Elementary Science."

I know of no such numerical compendium dealing with biology, but have often felt the want of one.

To sum up briefly the points which have been so far touched upon. The great majority of numerical problems which really occur in scientific work only require four figures to be accurately dealt with; hence a little ingenuity will generally bring them within the range of small tables. They should be worked out neatly, and as briefly as is consistent with the requisite accuracy; all useless figures should be rigorously excluded as misleading. Some few problems require the use of more powerful tables. Six-figure tables, such as those in Weale's series, and Collins's Logarithms for practical men, are little used, and inconvenient in practice. Seven-figure tables, such as Callet, Hutton, Babbage, Chambers, Schrön, Bremiker, Bruhns, Sang, so far as numbers go are nearly equally good; they differ chiefly in the trigonometrical ratios, which lie outside our present subject, and also considerably in price. SYDNEY LUPTON.

(*To be continued.*)

SOCIETIES AND ACADEMIES.

LONDON.

Zoological Society, December 20, 1887.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of November 1887.—Mr. Sclater read a letter from Dr. H. Burmeister containing a description of a supposed new Humming-bird from Tucuman. Mr. Sclater proposed to call this species, of which the type was in the National Museum of Buenos Ayres, *Chætocercus burmeisteri.*—The Secretary exhibited, on behalf of Major Yerbury, a pair of horns of the Oorial (*Ovis cycloceros*), which formerly belonged to the Royal Artillery Mess at Fort Attock, and were stated to have been originally obtained in the Chitta Pahar Range, a few miles south of Attock. These horns were apparently of the form lately described by Mr. A. O. Hume as *Ovis blanfordi.*—An extract was read from a letter received from Mr. H. M. Phipson, of the Bombay Natural History Society, offering some living Snakes for the Society's collection.—Mr. F. E. Beddard read a paper on Hooker's Sea-lion, *Otaria* (*Arctocephalus*) *hookeri*, based upon the specimens of this species recently received by the Society, one of which had lately died. The author called attention to the external features, visceral anatomy, and osteology of this Sea-lion, in comparison with the corresponding characters of other species of the group.—Mr. G. A. Boulenger read the description of a new genus of Lizards of the family Teiidæ, founded on a specimen presented to the British Museum by Mr. H. N. Ridley, who had obtained it in the forest of Iguarasse, Pernambuco. The author proposed to name this Lizard *Stenolepis ridleyi.*—A communication from the Rev. H. S. Gorham, entitled a "Revision of the Japanese species of *Endomychidæ*," was read. In this paper three new genera and thirteen new species were characterized and described. Additional observations were made upon the species previously known to inhabit Japan. The new species were based on specimens obtained by Mr. George Lewis during his last journey to the islands in 1880-81.—Mr. G. A. Boulenger gave an account of the fishes obtained by Surgeon-Major A. S. G. Jayakar at Muscat, east coast of Arabia, which had been presented by him to the British Museum. The collection contained specimens of 172 species, many of which were unrepresented in the national collection, and fifteen of which were apparently new to science.—Mr. H. Druce read a paper containing descriptions of some new species of Lepidoptera Heterocera, from Tropical Africa.

EDINBURGH.

Royal Society, December 19, 1887.—Sir Douglas Maclagan, Vice-President, in the chair.—Mr. John Murray communicated a paper on the height and volume of the dry land, and the depth and volume of the ocean. The mean height of the land above sea-level is 2250 feet. Only 2 per cent. of the ocean is included inside a depth of 500 fathoms. Seventy-seven per cent. lies between depths of 500 and 3000 fathoms. The mean depth of the ocean is 12,480 feet. If all the land were utilized to fill up hollows on the earth's surface, the sea would cover it to a uniform depth of 2 miles.—Sir W. Turner read a paper on the pineal gland in the walrus. The gland is excessively developed backwards, being visible from above without any dissection of the brain. The author contrasted it with the same gland in the lizard which is prolonged forwards and ends in the pineal eye. The cerebral lobes in the lizard are small, while those of all mammals are large. He suggested that the development of the lobes may have carried the gland backwards, and caused atrophy of the prolongation ending in the pineal eye. The atrophy, on the other hand, might have been caused by ossification extending over the aperture where the eye is situated.—Dr. Byron Bramwell described a method which he and Dr. Milne Murray had used successfully to record the exact time-relations of cardiac sounds and murmurs.—Prof. Crum Brown submitted a paper by Prof. Letts on the benzyl phosphines.—Dr. H. R. Mill read a criticism by Dr. Guppy on the theory of subsidence as explaining the origin of coral reefs.—Prof. Tait discussed the compressibility of water and of different solutions of common salt. Perkins proved sixty years ago that water becomes less compressible as the pressure is raised. At high pressures then it may be roughly assimilated to an extremely compressed gas. If the gas be regarded as consisting of hard spheres, the curve representing the relation between pressure and volume is approximately hyperbolic. The first asymptote of the hyperbola indicates what must be added to the external pressure to give the whole pressure to which the liquid is subject. The second indicates the ultimate volume to which it could be reduced by an infinite pressure. Applying this to the experimental results given to the Society in July last, the author showed that the pressure in water under ordinary circumstances is somewhere about thirty-two tons' weight per square inch; and the ultimate loss of volume under infinite pressure is about 25 per cent.

PARIS.

Academy of Sciences, December 26, 1887.—M. Janssen, President, in the chair.—Annual address, by M. Janssen. After brief reference to the losses sustained by the Academy during the year by the deaths of the illustrious *savants* MM. Paul Bert, Gosselin, Boussingault, and Vulpian, the President passed on to speak of recent scientific progress in France. Special mention was made of the magnificent Observatory just completed at Nice, for which the munificent founder, M. Bischoffsheim, receives the Arago Medal, now for the first time awarded. Allusion was also made to the isolation of fluorine effected by M. Moissan, and to the development of stellar photography, declared to be an "invention d'origine toute française." Nevertheless reference is made to the preliminary work of the English and American labourers in this field, Rutherfurd, Warren de la Rue, Bond, and Gould.—The Presidential allocution was followed by the announcement of the prizes for the year 1887, by the Secretary, M. J. Bertrand, who also read a paper on the life and work of the distinguished engineer, Stanislas Charles H. Laurent Dupuy de Lome.

Subjoined are the names of the successful competitors for the annual prizes. *Geometry:* Prix Francœur, M. Emile Barbier; Prix Poncelet, M. Appell. *Mechanics:* Extraordinary Prize of 6000 francs, divided between MM. Héraud, Dubois, Rouvier, and Moisson; Prix Montyon, M. Paul Vieille; Prix Plumey, M. Guyou. *Astronomy:* Prix Lalande, M. Dunér; Prix Valz, M. Perigaud; Prix Janssen, the late M. Kirchhoff. *Physics:* Grand Prize for the Mathematical Sciences, M. Willotte; Prix La Caze, MM. Paul and Prosper Henry. *Statistics:* Prix Montyon, MM. Victor Turquan, de Saint-Julien, and G. Bienaymé. *Chemistry:* Prix Jecker, MM. Arnaud and A. Haller; Prix La Caze, M. Moissan. *Geology:* Prix Delesse, M. Gorceix. *Botany:* Prix Barbier, MM. Edouard Heckel and M. Schlagdenhauffen; Prix Desmazières, MM. Ardissone and Dangeard; Prix Montagne, M. Boudier. *Anatomy and Zoology:* Grand Prize for the Physical Sciences, M. Raphael Dubois. *Medicine and Surgery:* Prix Montyon, Drs. Henri Leloir and E. Motais, and MM. Nocard and Mollereau; Prix Bréant, MM. Galtier, Chantemesse, and Widal; Prix Godard, M. Azarie Brodeur; Prix Chaussier, Dr. Jaccoud; Prix Serres, M. Alexandre Kowalevsky; Prix Lallemand, MM. Pitres, Vaillard, and Van Lair. *Physiology:* Prix Montyon, M. Ch. E. Quinquaud; Prix L. La Caze, Dr. Ch. Rouget. *Physical Geography:* Prix Gay, MM. Alfred Angot and Wilhelm Zeuker. *General Prizes:* the Arago Medal, M. Raphael Louis Bischoffsheim; Prix Montyon (Unhealthy Industries), Dr. Edouard Heckel; Prix Trémont, M. Jules Morin; Prix Gegner, M. Valson; Prix Petit d'Ormoy (Mathematical Sciences), the late M. Laguerre; Prix Petit d'Ormoy (Natural Sciences), M. Balbiani; Prix Laplace, M. Jules E. R. de Billy.—Honourable mention was made of the two English physiologists, Drs. Augustus D. Waller and E. Waymouth-Reid, for their memoir on the excised heart of mammals, published in the *Comptes rendus* for May 31, 1887. This study contains a number of new and highly interesting facts regarding the electric phenomena of the heart, the duration of the regular action of its four parts after excision, and the slowness acquired under certain circumstances by the wave of cardiac contraction.—Amongst the more important prizes offered for competition under the usual conditions during the years 1888 and 1889 are the following:—*Geometry:* Grand Prize for the Mathematical Sciences, to complete the theory of algebraic functions of two independent variables; Prix Bordin, to complete in some important particular the theory of the movement of a solid body. *Mechanics:* Prix Fourneyron, theoretic and practical essay on the progress of aërial navigation since 1880. *Astronomy:* Prix Damoiseau, to complete the theory of the irregularities occurring at long intervals in the motion of the moon caused by the planets. *Physics:* Grand Prize for the Mathematical Sciences, to complete in some important particular the theory of the application of electricity to the transmission of labour. *Agriculture:* Prix Vaillant for the best work on the diseases of cereals. *Anatomy and Zoology:* Grand Prize for the Physical Sciences, a complete study of the embryology and evolution of any animal, at the option of the candidate; Prix Bordin, comparative study of the auditory apparatus in warm-blooded Vertebrates, mammals and birds. *Physical Geography:* Prix Gay, to prepare monthly charts of the surface currents in the Atlantic, with a survey of the movement of drift ice in the waters about the Arctic regions; Prix Gay, to determine by a comparative study of their respective faunas and floras the relations formerly existing between the Polynesian Islands and the neighbouring lands.

Astronomical Society, November 9, 1887.—M. Flammarion, President, in the chair.—The President read a paper on some observations on the relative colours of stars, which he had made in 1875 by means of a specially constructed sextant in which the images of two stars wide apart could be brought into the same field.—M. Detaille read a paper on the photography of the solar spectrum with a direct-vision spectroscope, and stated that this subject was quite within the reach of amateurs, on a small scale of course, and presented many interesting points. He showed some negatives and positives obtained with a small instrument.

December 14.—M. Flammarion, President, in the chair.—The meeting was opened by the distribution of the calendar reform prizes, amounting, in medals and money, to the value of 5000 francs (an anonymous gift):—1st prize, 1500 francs, M. Gaston Armelin, of Paris; 2nd prize, 1200 francs, M. Hanin, of Auxerre; 3rd prize, 1000 francs, M. Francis de Roucy, of Compiègne; 4th prize, 800 francs, M. Barnout, of Paris; 5th prize, 250 francs, M. Remy Thouvenin, of Nancy; 6th prize, 250 francs, M. Blot, of Clermont (Oise).—M. Flammarion read a paper on some probable common proper movements of certain stars. In looking over the catalogue of the Paris Observatory, he had observed that several stars in Taurus—namely, Lalande 8178, 8209, 8237, 8256, 8297, 8404—had no motion in declination, and had all about the same proper motion in R.A. The same remarks apply to θ^1 and θ^2 Tauri. The two stars γ Leporis and Lalande 10931 seem also to be connected.—Colonel Laussedat, Director of the Conservatoire des Arts et Métiers, exhibited a curious binocular glass, constructed for Louis XIV. by Father Seraphin in 1681. This huge instrument comprises three rectangular bows which slide into each other. The length of the whole affair is no less than 3 metres 10 centimetres.—M. Neuville, in a letter, notices that the minimum of Algol seems longer than 6 minutes as given by several authors. He adopts 18 minutes, and gives a probable size of Algol's dark companion.—MM. Paul Henry and Detaille remark that Webb gives 18 minutes as the duration of Algol's minimum.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Birds of Wiltshire: Rev. A. C. Smith (Porter).—Arithmetic Papers: S. J. D. Shaw (Deighton, Bell, and Co.).—Major Lawrence, F.L.S., 3 vols.: Hon. E. Lawless (Murray).—Catalogue of the Fossil Mammalia in the British Museum; Natural History, Part v.: R. Lydekker (London).—Prodromus of the Zoology of Victoria, Decades 1–14: F. McCoy (Melbourne).—The Theory and Use of a Physical Balance: J. Walker (Clarendon Press).—Journal of Anatomy and Physiology, January (Williams and Norgate).

CONTENTS.

THURSDAY, JANUARY 12, 1888.

PHYSICAL CHEMISTRY.

Lehrbuch der Allgemeinen Chemie. Von Dr. Wilh. Ostwald. In Zwei Bänden. (Leipzig: W. Engelmann, 1885-87.)

THE larger text-books of chemistry have generally been devoted to describing and roughly classifying the facts which form the foundation of the science. These facts are so numerous, varied, and important, that when one has spent years in arranging, cataloguing, and reciting them, his chemical vision has generally acquired a fixed downward direction, and he is almost unable to lift his eyes from the foundation-stones to look on the buildings which other workers have been raising.

But, whether such a one will look at the building or not, the building is surely rising. The walls already are massive; there are adornments of conceits, perhaps sometimes too quaint; windows there are in plenty to admit light and air: the house will never be completed, because nature is inexhaustible, but even now there is promise of a goodly building. Nor shall the House Beautiful want fit interpreters, among whom an honourable place will be held by the Professor of Physical Chemistry at Leipzig.

It has generally been admitted that chemistry is a branch of physical science. Individual chemists by their researches have shown that the relation of chemistry to physics is that of the less to the greater; but most of the attempts to set forth this relationship in its entirety have failed. To treat chemistry as a branch of physics requires one who is almost as much a physicist as a chemist, but one whose physical training has waited on his chemical judgment. Some books on physical chemistry have been books on descriptive chemistry, with scraps of physical facts thrown in; others have been books on physics to which the use of chemical illustrations has given an ill-defined but not unpleasing chemical tone. Only of late years has it become possible to set forth the connections between the parent science and the greatest of her children in a fairly satisfactory manner; and this possibility has come through the recent advances made in the study of these connections.

It was therefore fitting that one of the men whose work forms no small part of all of first-class importance that has been done in recent years in the sphere of physical chemistry should be the man to write the first good text-book on general chemistry considered as a branch of physics. Ostwald prefers to call his work "Lehrbuch der Allgemeinen," rather than "physikalischen," "Chemie." The title very happily expresses the scope and character of the book; but the treatment of chemical principles in a general manner is made possible in this treatise by regarding chemistry as a special branch of physics. The book is intended for fairly advanced students who have already a tolerable knowledge both of descriptive chemistry and of physical principles. Some of the higher forms of mathematical analysis are freely employed. The form in which the author has chosen to present his treatise is the historical-critical; he justly remarks that the historical coincides with the logical development of many chemical ideas.

As the object of the work is to enable the student to gain a firm hold of the principles of chemistry, and more especially to teach him that very many of these principles have been reached by the application of physical methods to chemical phenomena, much care is taken to distinguish generalized statements of facts from hypotheses, to indicate the need of using hypotheses, to trace the merging of several hypotheses into one general theory, and to avoid mere speculation.

The first volume is devoted to stochiometry. The laws of chemical combination, which form the basis of the whole science, are laid down in a singularly clear and succinct manner; the atomic theory of Dalton is sketched; the chemical methods by which combining weights are determined are classified, and this is followed by a short critical exposition of the results obtained for each element. The second, third, and fourth books of the first volume are devoted to accounts of the properties of gaseous, liquid, and solid bodies, respectively. The relations between the volume, temperature, and pressure of gases, are considered; this leads to a statement of the law of Gay-Lussac, and a consideration of Avogadro's hypothesis; then follows an account of the kinetic theory of gases, the specific heats, and the optical properties of gases. The book on liquid bodies is devoted to a consideration of (1) the general properties of liquids; (2) the relations between the liquid and gaseous states; (3) the volume-relations of liquids; (4) solution; (5) optical properties of liquids; (6) capillarity, diffusion, and osmosis; (7) electrical conductivities and electrolysis of liquids; (8) specific heats of liquids. The book on the stochiometry of solid bodies includes the consideration of crystallography, especially in its chemical bearings, the optical and electrical properties of solids, &c. The first volume concludes with a sketch of the relations between atomic weights and chemical properties, a general account of the molecular theory as applied in chemistry, and a short but very suggestive chapter on theories of chemical composition and constitution.

The second volume deals with the vast and widely ramifying subject of chemical affinity. The first part, on chemical energy, comprises what is really a comprehensive treatise on thermo-chemistry, and also full critical accounts of photo-chemistry and electro-chemistry. The second part, dealing more distinctly with chemical affinity, begins with an historical sketch; this is followed by about 150 pages on chemical dynamics; and the whole concludes with an account of the various methods whereby measurements of the relative affinities of various bodies, especially acids and bases, have been obtained; the last chapter deals with the relations between the nature, composition, and constitution of bodies, and the values of their affinity-constants.

Ostwald has undertaken and brought to a conclusion a task of great difficulty. His book has removed the sting from the taunt so often cast at the chemist that chemistry is the pursuit of the mere fact-finder and formula-monger. If Ostwald's "Lehrbuch" had only made evident the fact that chemistry is one of the exact sciences it would have done much; but it has done more than this; it is a repository of the general and abstract truths of the

science arranged in logical sequence; it is a guide to the student and the investigator (for in chemistry these two are one); and it is full of suggestions alike to the physicist and the chemist.

That part of the second volume which deals with the recent developments of the study of chemical affinity will probably be found by many to be the most interesting portion of the book. Everyone knows how unsatisfactory is the treatment of this subject in the standard text-books. Who has not been perplexed as he attempted to gain clear conceptions about affinity? Affinity is one of those terms that escape one as soon as one tries to grasp it: it is protean, and each form which it assumes scarcely lasts long enough for one to distinguish it from the others.

The work of Guldberg and Waage, published twenty years ago, did not bring forth much fruit for some time; perhaps because these naturalists were obliged to go back sixty years to find in the writings of Berthollet the germs of a really exact treatment of the subject of affinity. But within recent years great advances have been made—and made, speaking broadly, on the lines laid down by the Norwegian professors. No one has had more part in these advances than Ostwald; to him we are indebted for several new experimental methods for finding values for the affinity-constants of acids and bases—indeed the proof of the existence of a measurable affinity-constant for each acid and base is, for the most part, due to him. It is one thing to know that memoirs are to be found in the journals wherein the subject of affinity is gradually advanced stage by stage, but it is quite another thing to have a clear, logically arranged, and condensed account of these memoirs in a text-book. It is one thing to be told that the modern development of affinity is the outcome of the views which Berthollet published, in 1803, in the *Essai de Statique Chimique;* it is quite another thing to have this historical and logical development set before one in detail in a masterly manner.

The subject of affinity is largely involved in the wider conception of chemical equilibrium. Ostwald gives a short account of the attempts which have been made to formulate the laws of chemical equilibrium. He then narrows the meaning of affinity, at least as applied to acids and bases; by doing this it becomes possible to extricate the notion of affinity from the mass of more or less connected facts which had threatened to swamp it, and to give it a quantitative meaning.

The affinity-constants of acids and bases are numbers which tell how much of a definite chemical action those bodies are capable of performing under definite conditions. The formulæ of the same acids and bases exhibit the composition of definite masses of these compounds, which masses are in many respects chemically comparable. The goal of chemistry has always been to trace definite connections between the composition of bodies and their chemical properties; but of all the chemical properties of a body the most important is its affinity-constant, inasmuch as we are apparently justified in saying that this value quantitatively conditions all the chemical reactions in which the body takes part: hence the importance of accurately tracing the connections between the changes of compositions of bodies, as represented by their formulæ, and the variations in the values of the affinity-constants of these bodies, must be very great. The data are as yet insufficient to allow of more than a beginning in this direction: such a beginning is made in the last chapter of Ostwald's book.

To everyone who hopes to make chemistry the business of his life I would say—get Ostwald's "Lehrbuch," read it, study it, become acquainted with it, use it; for by doing this you must become more fitted for doing your work as a chemist. M. M. PATTISON MUIR.

BRITISH AND IRISH SALMONIDÆ.

British and Irish Salmonidæ. By Francis Day. 12 Plates. (London and Edinburgh: Williams and Norgate, 1887.)

IN this work Mr. Day expounds in greater detail the views he made known in his "British and Irish Fishes," concerning the characters and affinities of the several British forms belonging to the genus Salmo. He also includes in the volume the consideration of many other important problems connected with the natural history of British Salmonoids. On p. 9 he gives a synopsis of the British genera of the family, viz. Salmo, Thymallus, Coregonus, Osmerus, and Argentina, and then proceeds to consider Genus 1, Salmo, while at p. 278, is the heading Genus 2, Thymallus, Cuvier. For the designation of species and varieties English names are generally used, but with each is given a copious list of the Latin Linnean synonyms, and references to the works where they occur. The species considered are as follows: the Salmon, Trout, British Char, American Char or *Salmo fontinalis*, and the Grayling. Thus Coregonus, Osmerus, and Argentina are left outside the scope of the book, notwithstanding its comprehensive title.

Very elaborate descriptions, including enumerations and dimensions, are detailed for each separate form, but concise diagnostic analysis is entirely wanting. In the synopsis of species of Salmo given in the earlier work, "British and Irish Fishes," we find that the only trustworthy specific character differentiating *Salmo salar* from *Salmo trutta* is the presence in the former of eleven rows of scales in an oblique row from the adipose fin to the lateral line, all forms of *Salmo trutta* having fourteen or more of such scales. In the work before us one has to wade through two pages and a half of description of the salmon before reaching a mention of this diagnostic feature.

The views here expressed concerning the forms of sea-trout are somewhat different from those published in the "British and Irish Fishes." In the latter work Mr. Day described *Salmo trutta* and two varieties, *S. albus* and *S. cambricus*. In the present he describes *Salmo albus* (with the same synonymy) as the immature stage or grilse of the northern sea race of trout, *S. cambricus* being the southern sea race. Here again the want of a short diagnosis of the two races is much felt by the reader. From the numerical formulæ of the two races, which are separated by several pages, it is seen that the range of variation in the number of pyloric cæca in the one race is different from that in the other. In the northern form it is 33–61, in the southern 33–52. But it is extremely difficult, by reading and comparing the two lengthy descriptions, to discover what is the exact amount of difference between the two races. However,

after the descriptions we reach a discussion of this very point, and we find that most of the differences on which emphasis has been placed by other authorities are not found to be constant when a large number of specimens are examined, that the two races pass by gradual transition one into the other, but that as a rule in the southern there are fewer pyloric cæca than in the northern, and that the Sewin usually loses the teeth on the body of the vomer, at an earlier age than the northern sea trout.

The following are the different forms of non-migratory fresh-water trout which have been distinguished as distinct species, and whose synonymy is given in the present work: Brook trout, Lochleven trout, Crasspuill trout, Estuary trout, Orkney trout, Cornish trout, Great Lake trout, Gillaroo trout, and Swaledale trout. Short descriptions of these are given in footnotes, excepting the brook trout and the Lochleven trout, which are discussed at length in the text. These descriptions, though brief, are not diagnostic, and it requires the most careful reading and comparison to find in what respects the varieties differ from one another. Mr. Day believes that there is no definite line to be drawn between anadromous sea trout and non-migratory fresh-water trout, intermediate forms being common; nor between the different varieties of fresh-water trout. But granting—for we are inclined to agree with Mr. Day's conclusions—that in all these forms we have but one species, it is surely worth while to give a more lucid and more definite account of the differences between them. The arguments which Mr. Day employs to prove that all forms of trout, whether anadromous or confined to fresh water, belong to one species, may be divided into three classes, and his book would have been much easier to read if he had kept them separate. The first class are those which show that the various forms graduate into one another, or that the peculiarities of one are included in the range of variation of another; the second, those which show that removal to a different environment causes the characteristics of one form to be transmuted into those of another; the third, those which show that the several forms breed freely when crossed.

All the species of char which have been distinguished in Britain are considered in this book as belonging to one variable species which is identical with the *Salmo salvelinus*, Linn., and *S. umbla*, Linn ,—that is, with the Continental char. A similar criticism may be passed on Mr. Day's discussion of char to that made of his account of trout.

In the account of the American char, *Salmo fontinalis*, we have again a minute description, with no specific diagnosis. In a footnote to this portion of the work, it is pointed out that in the article "Salmonidæ" of the present edition of the "Encyclopædia Britannica" the erroneous statement of Dr. Günther, that the *Salmo namaycush* of America is a true trout, is repeated, but no reference is given to any work where the correct description of *S. namaycush* as a char can be found.

We have up to this point been criticizing Mr. Day's work chiefly from a speciegraphical point of view; we must now say a few words about the treatment of other branches of the subject. At the beginning of the account of the genus Salmo is a short description of the anatomy of Salmonoid fishes, followed by a discussion of the eggs and their development, the latter especially in connection with pisciculture at Sir J. Maitland's establishment at Howietoun. The description in the text of the mode of packing eggs which has been perfected at Howietoun seems to be erroneous: it is stated that the main principle is to employ thin layers of well-picked and pressed moss in trays with perforated bottoms, the eggs being separated from the moss by muslin mosquito netting, swan's down, calico, or butter cloth; while in a quotation in a footnote the correct account is given—namely, that the ova rest in direct contact with the damp moss, and are covered by another layer of the same, the muslin being only used in order that the layer of moss may be lifted and moved. Reference is made in this part of the book to the subject of hybridization between different species of Salmo, and a review of the history of the subject is given, but the full treatment of the subject occurs in a chapter specially devoted to it. In this chapter details are recorded of definite experiments in hybridization made at Howietoun. This chapter on hybrids is one of the most interesting in the book, and another on monstrosities is also well worth study.

Scattered throughout the pages are examples of that originality in sentence-construction which is familiar to all who know Mr. Day's writings. Thus in the account of artificial fertilization we read: "This is gently stirred with the hand until the eggs harden, or 'frees' as it is termed, being a period from one to three quarters of an hour according to the temperature, taking longest in cold weather." In another place we find: "One modifying circumstance in the feeding of the salmon has been observed to be connected with a muddy state of the river, possibly interfering with respiration, consequent upon the amount of mud which had been swallowed." Another passage which is worth quoting is:—"As regards thirst it would seem either to be unknown to these creatures; or, living as they do in a watery medium, it may be quenched by means of endosmosis through the skin. Were this not the case, it would be difficult to conceive how such a longing could be satisfied while residing in salt water."

But in spite of its defects the book contains a mass of new and accurate information concerning the forms of Salmonidæ of which it treats. In bibliography it is unusually rich, the results of previous writers being freely quoted in footnotes, so that several of the pages contain 90 per cent. of notes and only 10 per cent. of text. Besides the woodcuts in the course of the work, there are twelve plates of illustrations at the end, ten of which represent different forms of Salmonidæ in beautifully coloured lithographic impressions. The excellence of these is very great, and testifies to great care and skill on the part of the draughtsman (*i.e.* the author himself), the colourist, and the lithographer.

THE ECHINOIDEA.

Die Japanischen Seeigel. Von Dr. L. Döderlein. Pp. 59, Pl. I.-XI., Th. I., Fam. Cidaridæ and Saleniidæ. (Stuttgart: E. Schweizerbart'sche Verlagshandlung, E. Koch, 1887.)

DR. DÖDERLEIN has produced the first part of a very philosophical study of the beautiful Echinoidea, which are in their paradise in the Japanese seas. Some

collections of considerable importance came to Dr. Döderlein from private sources, and one was the result of the collecting during the expedition of the Italian corvette *Vettor Pisani*. Descriptions of some of the species of Cidaridæ, Temnopleuridæ, Saleniidæ, and of a species of Hemipedina were published by this author in *Wieg. Archiv*, 1885, v. 51, pp. 73–112, and as some of the forms had an ancient facies they attracted attention. Dr. Döderlein seems to have been impressed with the importance of the fauna in reference to the past, and prepared the way for the present publication by studying the pre-Jurassic and Cretaceous species of Cidaridæ especially. The work now publishing in parts will evidently be worthy of a good naturalist who sees no vast biological breaks in the continuity of the Cidaridæ since the appearance of the Zechstein Eocidaris, which he shows to be inseparable from the modern family Cidaridæ. The author describes the new species, reconsiders the Cidaridæ already known, pays especial attention to the growth of the structures which are used in classification, and, after describing some peculiar structures which he discovered in the St. Cassian Cidarids, passes on to subdivide the great genus so as to identify groups of species according to sub-genera in the Secondary and existing times. The descriptions of species are accompanied by fair illustrations, but it would be as well if more of the denuded tests could be shown.

There are four new species of Cidaris, a new Porocidaris, and three species of Goniocidaris; the depths from which the specimens were derived were from 40 to 200 fathoms. *Goniocidaris mikado*, Död., is the most extraordinary of the species, and has an unusually small number of coronal plates, characteristic median groovings, and wonderful spines—outdoing any other, and that is saying a good deal. The spines are essentially according to Japanese art: the larger have umbrella-shaped disks at their top, and some another disk lower down; the disks are circular in their deeply incised or occasionally serrate outline. The commonest species of Cidaris certainly puts one in mind of the Mediterranean *C. histrix* and of the North Atlantic *papillata*, but these Japanese forms are considered to belong to a different sub-genus by Dr. Döderlein. He was impressed with the fact that some striking Cretaceous Cidaridæ have the primary tubercles of some of the coronal plates near the apical system, aborted or wanting, and that a similar condition occurs in the majority of the Japanese species. He would establish a better definition for Stereocidaris, Pomel, and thus link the Cretaceous and Japanese species together. There is something very candid and straightforward in Dr. Döderlein's method of writing, and he does not hesitate to indicate how, in a comparatively short time, he altered his opinion regarding the particular sub-genus under which his own and other species should go. A similar state of things is well illustrated in the instance of A. Agassiz, and his synonymy of the Cidaridæ shows, as in the case of Dr. Döderlein, how a mind desirous of truth has to suffer in the attempt to subdivide a good genus into groups which are not founded upon differences of structures of much physiological importance.

As a matter of fact, the tubercles of *Cidaris* (*Stereocidaris*) *grandis*, and of the species *japonica*, Död., are not much more deficient than in many specimens of the common *Cidaris* (*Dorocidaris*) *papillata* of the North Atlantic and Mediterranean; and the shape and ornamentation of the coronal plates with ill-developed or absent primary tubercles, in the well-known *Cidaris sceptifera* from the upper chalk, do not resemble those of the modern forms. The ornamentation shown on Pl. II., Fig. 4, is more like that of a Tertiary Cidaroid from Sind than of the tall-plated Cretaceous type. But there is a decided resemblance between Dr. Döderlein's *C. sceptriferoides* and the Cretaceous species.

If these unsatisfactory sub-genera were simply used to represent groups of species linked together by some unimportant but readily recognized structural peculiarities, there would be no objection to be made—indeed, the proceeding is very useful; but the groups are allowed to become of generic significance, and thus it will be noticed at the conclusion of Dr. Döderlein's work, that a list of twenty-two groups equal to genera is given; and bad sub-genera, good ones, and good genera are jumbled up together. Good old Cidaris has in fact fallen to pieces.

In considering the species which have not a Japanese habitat, Dr. Döderlein is in opposition to A. Agassiz and De Loriol in reference to the proper sub-genus under which some well-known species are grouped, and it appears to be the case that Dr. Döderlein will have to arrange the species of Cidaris proper on his own lines.

The particular structure to which Dr. Döderlein alludes in noticing the Triassic Cidaridæ, is a horizontal groove on the interradial side of each pair of pores; it seems to be very universal; moreover, there is a more decided overlap and ribbing of the coronal plates in these pre-Jurassic forms than in the Jurassic and subsequent.

The Salenia described by Dr. Döderlein is a very close neighbour of *S. hastigera*, A. Ag.

Few monographs relating to the recent Echinoidea have as much good matter and logical reasoning in them as this one of Dr. Döderlein's, and the second part of the work will be looked for with great interest.

P. MARTIN DUNCAN.

FRITSCH'S PALÆONTOLOGICAL RESEARCHES.

Fauna der Gaskohle und der Kalksteine der Perm-formation, Böhmens. Von Dr. Ant. Fritsch. Band II., Heft 1, pp. 32, Plates 49–60. (Prague: In Commission bei Fr. Rivac, 1885.)

THE first part of this admirable work was briefly reviewed in NATURE, vol. xxi. p. 31. It was then observed that the book was almost as interesting to the stratigraphical geologist as to the palæontologist, for the Gaskohle and its superincumbent Kalksteine rest upon Silurian rocks, and are usually not covered by other strata in vertical succession. The coals, clays, and ironstones have a Carboniferous facies, and the conformable limestones are believed to be true Permians. The palæontological evidence regarding the age of the beds is somewhat anomalous in the views of purely British fossilists, but it speaks very forcibly and in a most suggestive manner to the students of the Gondwana formations of Hindustan. The presence of *Sigillaria*, *Stigmaria*, *Calamites*, and *Lepidodendra*, in the Gaskohle,

in association with Permian species of ferns and a *Walchia*, seems however to place these Bohemian beds on a lower geological horizon than the Gondwana series, which have had their palæobotany studied by the same palæontologist, Feistmantel, who investigated the plant-remains of the Permo-Carboniferous of Eastern Europe.

The rich fauna of Labyrinthodontia of the Gaskohle, which, as was explained, Fritsch prefers to study under the more comprehensive group of Stegocephali, is associated with fish of the genera *Ceratodus*, *Orthacanthus*, *Pleuracanthus*, *Acanthodes*, and *Amblypterus*, and also with many species of *Palæoniscus*, found elsewhere in true Permian beds. Amongst the Invertebrates are Arachnoidea, Julidæ, Estheriæ, and Anthracosiæ.

The part of the work now under consideration is palæontological, and refers to some of the most interesting of the many sculptured-headed, folded-toothed Amphibia which preceded the Reptilia in time. Several classificatory alterations, especially in the grouping of the genera in families, are introduced, and apparently with good reason; and at the commencement it will be noticed that the Microsaurii, Dawson, suffer, and a new family, the Dendrerpetontidæ, is founded. Fritsch considers that the structure of the teeth of such Microsaurs as *Hylonomus* and *Hylerpeton*, prevents their being associated in the same family with *Dendrerpeton*, the species of which have teeth strongly grooved from the base, with simple irregular folds, the top being smooth: the new family has, like the Microsaurs, amphicœlian vertebræ. It is certainly remarkable how widely these forms were distributed geographically during that long period when so much of the present continental areas was land. Fritsch describes two new species, and also a third about the generic position of which there may be some doubt, and which has a wonderful arrangement of cranial bones behind the orbits.

The most interesting parts of the work are now reached and the author comes to the consideration of those extraordinary Stegocephali which have such curious double and multiple developments of the vertebral centra. The first of the families of these groups is the Diplovertebridæ, and the solitary form of it is carefully described. The characters of the family are the doubly segmented vertebral centra, at the caudal end of the column, and a very decided pitting of the surfaces of the bones of the extremities for vascular canals.

Fritsch avails himself of Cope's terminology; and the peculiar condition of the vertebral centra—the anterior of the two segments carrying the spinous processes and the ribs, the posterior not having any relics of arches, and being plain—necessitates the arrangement of the species with those whose vertebræ are "embolomeri." The illustrations of the species on Plates 50 and 52 are admirable, and their comprehension is assisted by the woodcut diagrams placed in the context.

Sparagmites lacertinus, Fr., is placed amongst the Archægosauridæ, and it will be observed (Plate 50, Figs. 15, 16) how the vertebral centra differ from those of the last family. The centra appear to be broken up, and each has two lateral and an inferior component, coming under the division "rachitomi" of Cope. Miall's family Chauliodontia is represented in the Gaskohle by a species, and the preserved remains show the dissimilar teeth with a semi-Labyrinthodont structure; the genus included is a familiar one to English palæontologists, and is *Loxomma*. The last family, described in the book, has genera with highly developed crania and a parietal foramen (which also occurs in all these forms from the Gaskohle), and the vertebræ are even more remarkable than in the other families. In the Melosauridæ the caudal portion has the centra embolomerous, whilst those of the fore-part of the column are rachitomous; the teeth are dissimilar, and simply and irregularly folded. The supra-occipital bones occasionally have strongly developed, backward projecting, curved processes (*Sehnenhöckern*). The genus *Chelydosaurus*, with a well-developed tarsus and a most singular growth of chest and body plates, belongs to the family. *Sphenosaurus*, H. von Meyer, comes in here, and the species *S. Sternbergii*, elsewhere a Muschelkalk form, is found in the red sandstone of the Bohemian Permian! The new genus *Cochleosaurus* has a species which shows the posterior hooks of the supra-occipital bones in perfection.

The book which contains all this interesting matter will be found of great value by students as well as by advanced palæontologists, and the beauty of the illustrations leaves little to be desired. The Geological Society presented Dr. A. Fritsch with the Lyell Medal and Fund, and the gift was mainly owing to the appreciation of his excellent work amongst these Upper Palæozoic, Permo-Carb. fossils. The work is a great addition to the natural history of the early Vertebrata.

P. M. D.

OUR BOOK SHELF.

The Flora of Howth. By H. C. Hart (Dublin: Hodges, Figgis, and Co., 1887.)

MR. HART enthusiastically describes the parish of Howth as one with many attractions. He thinks that as a sea bathing summer retreat "its equal cannot be found in Ireland"; and he points out that it is invested with archæological interest by a great dolmen in the demesne of Lord Howth, by the ruins of an early abbey in the village of Howth, by the earlier church or chantry of St. Fintan's on the Sutton side, with its holy well, and by the ancient castle, called Corr Castle, of the Barons of Howth. A little way from the shore is Ireland's Eye, with the remains of a church of the sixth or seventh century. For the ornithologist, the entomologist, and the marine zoologist, Howth, according to Mr. Hart, provides much material for study. These things, however, he notes only by the way; it is with the flora of Howth that he is especially concerned. For this he claims attention on two grounds: (1) because several of the species found are rare; (2) because it does not often happen that so many forms exist in so small a space. Mr. Hart has taken great pains to make his account of his subject complete and readily intelligible, and the book ought to be of considerable service to local botanists and tourists.

Mineralogy. By Frank Rutley. Third Edition. (London: Thomas Murby, 1887.)

WE are glad to welcome a third edition of this excellent manual, which forms one of Murby's "Science and Art Department" series of text-books. The materials of the little work are arranged with great clearness, and the descriptions of minerals are invariably simple and precise. Nearly the whole of the chapter on crystallography has been re-written, and other alterations have been made to fit the book for the present requirements of students. More than fifty fresh woodcuts have been added.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

"A Conspiracy of Silence."

MAY I ask your correspondents who have been good enough to read my article on "Darwin's Theory of Coral Islands," published in the September number of the *Nineteenth Century*, to begin addressing themselves to the merits of the scientific question there dealt with, and to cease wasting their own time and your space upon scolding me for a few words—perhaps exaggerated—respecting the wide-spread reluctance to question any theory advanced by Charles Darwin? I have already explained in your columns the sense in which I spoke, and, subject to that explanation, I have nothing to retract. I observe in Prof. Tait's notice of Dr. Balfour Stewart, published in your latest issue, a passage which shows that this very eminent man of science speaks in a tone very similar of certain "advanced" geologists who "ignore" views which "tend to dethrone" their own "pet theories." Moreover, since I last addressed you in explanation, I have observed the remarkable passage ("Darwin's Life," vol. ii. p. 186) in which my censor, Prof. Huxley, positively blasphemes against no less a distinguished body of scientific men than the French Institute for their conduct towards evolutionism. He speaks of the "ill-will of powerful members of that body producing for a long time the effect of a conspiracy of silence." This is the very same expression which I used, but without the offensive aggravations added by Prof. Huxley.

Inveraray, December 30, 1887. ARGYLL.

Mr. Seebohm on Physiological Selection.

FROM a footnote to page 23 of Mr. Seebohm's recently published and magnificent monograph on the Charadriidæ I learn that I owe him an apology for having inadvertently misrepresented his views upon a point of considerable importance in the philosophy of evolution. In his British Association paper (which he now re-publishes) he went even further than I had gone in recognizing the "swamping effects of intercrossing" upon incipient varieties, with the consequent importance of isolation in the differentiation of species. I therefore supposed that he likewise agreed with me in holding it improbable that new species arise as a result of many beneficial variations of the same kind arising at the same time and in the same place. I now find, however, that he is a strong advocate of the opposite opinion—apparently going further than Asa Gray, Nägeli, Mivart, the Duke of Argyll, or indeed any other evolutionist, in support of the doctrine of teleological variation in determinate lines. I therefore write to withdraw my previous misrepresentation of his views upon this matter, and to apologize for my inadvertency in making it.

At the same time, I may observe, it does not seem to me quite intelligible how Mr. Seebohm can reconcile his doctrine of teleological variation with his doctrine of the paramount importance of geographical isolation For it is evident that, in whatever measure geographical isolation is found to be of importance as a condition to the origin of species (*i.e.* by preventing free intercrossing), in that measure is the doctrine of teleological variation invalidated. Indeed, Mr. Seebohm himself puts Mr. Wallace on the horns of a dilemma with regard to a precisely parallel case. In order to meet me where I draw attention to the difficulty which free intercrossing imposes upon the theory of natural selection, Mr. Wallace argued in favour of collective variation, *i.e.* of the doctrine that a considerable percentage of identical and beneficial variations may arise simultaneously in the same community. Now, Mr. Seebohm very pertinently observes (p. 13):—"It seems to me that, by the admission of this fact, Mr. Wallace has dethroned his theory of natural selection from its proud position as the main factor in the origin of species." With this, of course, I fully agree; but does it not equally follow that by *his* admission of this same "fact" Mr. Seebohm is no less effectually dethroning his own theory of the paramount importance of isolation as one of the main factors in the origin of species?

In conclusion, I cannot understand why Mr. Seebohm should have ignored my answer to the criticisms which he now republishes. For, as I have pointed out in these columns before, the whole brunt of his criticism (like that of Mr. Wailace) was directed against a theory which never so much as occurred to me. Both my critics took it for granted that I supposed my "physiological complements" to arise only in pairs; and therefore they both had an easy case in showing how improbable it was that the two complements should chance to come together. But even in my original paper there were passages to show that I supposed these physiological variations to occur in large numbers, or "collectively," leading to what botanists now call "prepotency," and thus explaining why hybridization is so rare in Nature. Possibly in that paper I was not sufficiently explicit in guarding against a misconception which it never occurred to me could arise. But certainly in my reply to this misconception, no further doubt as to my meaning could possibly remain. I confess, therefore, to being not a little surprised at this reappearance of Mr. Seebohm's criticism, without allusion to my full repudiation of it a year ago. I should much like to learn his views upon the theory which I have published, but must protest against this absurd substitution being still attributed to me, after I have disclaimed it with all the emphasis of which the English language is capable. GEORGE J. ROMANES.

An Incorrect Footnote and its Consequences.

IN all the five editions of Baltzer's "Theorie und Anwendung der Determinanten" there stands at the foot of the first page an historical note, in which reference is made to a work entitled, "Demonstratio eliminationis Cramerianæ," by Mollweide (Leipzig, 1811). About a year ago it became necessary to examine this demonstration for the purpose of having it reported upon in an historical work. The University Libraries in Scotland were applied to in succession, but no copy could be heard of. Inquiries made at the more important libraries in Cambridge by friends resident there, or by letter, ended in the same unsatisfactory way. Letters, followed by an actual visit, to several libraries in London, brought no better result; and after every possible biographical scrap about Mollweide had been ferreted out in the British Museum, the suspicion began to form itself that some curious error had crept into Baltzer's footnote. In order to get to the bottom of the matter, the excellent mathematical library of Göttingen University was next applied to, and the library of Giessen University, where Baltzer was Professor; but in both cases in vain. A last effort was then made about a month ago in a letter to the University Library of Leipzig, where the reputed author Mollweide had taught, and where the "Demonstratio" (or *Demon*, as it had for more than one reason come to be called) had been published. Even here, at first, there was failure. But Prof. Virchl, who most kindly interested himself in the matter, was soon successful in his quest. What he found, however, was not a "Demonstratio" by Mollweide; the title was simply as follows: "Ad memoriam Kregelio-Sternbachianam in auditorio philosophorum die xviii. Julii, MDCCCXI. h. ix. celebrandam invitant ordinum Academiæ Lips. Decani seniores cæterique adsessores——'Demonstratio eliminationis Cramerianæ.'" Either, therefore, no author should have been mentioned by Baltzer, or an indication should have been given that Mollweide's name was an interpolation in the title. One or other of these courses would likewise have been less hurtful to Baltzer's reputation for accuracy; for, after all, Mollweide was not the author. In the Leipzig Library Catalogue the work is entered under the name of De Prasse, and Prof. Virchl had no doubt whatever, for perfectly conclusive reasons which he gave, that De Prasse was the author. The work extends to only 15 pages quarto, and is considered by the same authority to be very rare.

The point which we have now reached in the story might seem a not unfitting one to stop at; but the end is not yet. De Prasse's modesty requires explanation, and so likewise does the intrusion of Mollweide's name. Both are partly cleared up by the following facts supplied by Prof. Virchl. (1) The Kregel-Sternbach dissertation (which the "Demonstratio" was) falls to be delivered by the Dean of the Philosophical Faculty for the time being: the author's name was thus not an absolute necessity on the invitation title-page. (2) Mollweide was De Prasse's suc-

cessor, and came first to Leipzig in 1811, the very year we are concerned with; so that in that year both men may have held office, and consequently if an author's name had to be supplied Baltzer might easily have made a worse guess.

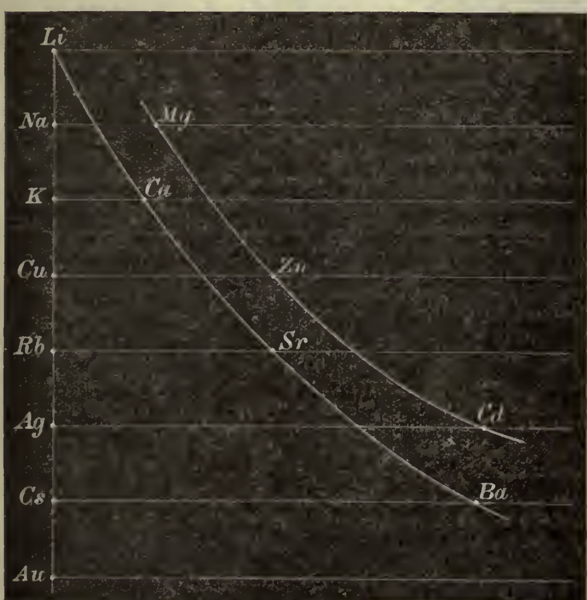
Both guess-work and circumstantial evidence, however, are quite unnecessary. After these facts were received from Leipzig, the library catalogue of University College, London, was turned up at De Prasse's name. No "Demonstratio," it is true, rewarded the searcher: but as a work with the miscellaneous-looking title, "Commentationes Mathematicæ," 4to, Lips. 1804–12, was found entered, the librarian was communicated with. In a day or two an obliging reply came to hand to the effect that the lair had indeed been found, the 15 quarto pages sought (or, at least, as many as are essential) being pp. 89–102 in the second fasciculus. The full title of the whole work is "Commentationes Mathematicæ, auctore Mauricio de Prasse, Math. prof. ord. in univers. liter. Lipsiensis." The first fasciculus contains 54 pages, and is dated 1804; the second contains 66 pages, viz. pp. 55–120, and is dated 1812. Of the eight separate "Commentationes" the "Demonstratio" is the seventh. Doubtless, copies of this collection of mathematical papers are to be found at several of the libraries above referred to. The work at any rate does not appear to be rare: the writer already possesses a copy, for which he paid the not extravagant sum of 2*s.* 8*d.*

The moral on the surface of this tale may be, "Verify your references"; it is not the only moral, however. Baltzer, in his first preface, felt called upon to direct attention to the many inaccuracies and even errors ("manche ungenauigkeiten und selbst unrichtigkeiten") of Spottiswoode's pioneer treatise; yet if the leaf following the said preface be turned over, a footnote of five lines is found containing five "ungenauigkeiten" (say), one of which—being that referred to in the narrative of the "Demon"—might well be put in a worse category. *Humanum est errare.* THOMAS MUIR.

Bothwell, Glasgow, December 26, 1887.

The Periodic Law.

IN none of the chemistry books or magazines to which I have access can I find any reference to a curious property of the chemical elements in connection with the Periodic Law. If instead of placing the elements as usual in seven vertical columns we arrange them at distances corresponding to the differences of their atomic weights, it will be found that they are disposed in curious curves. The following diagram will make my meaning



clearer. Arranging the monads in a vertical column, and taking it for a base line, place Ca at a distance from K corresponding to the difference of their atomic weights; also treat Sr and Ba in the same way in relation to Rb and Cs. It will then be found that they are arranged on a curve terminating in Li, which is known to unite in itself the properties of the metals of the alkalies and those of the alkaline earths. Mg, Zn, and Cd also range themselves on a curve when measured from Na, Cu, and Ag.

Ranging the tetrads vertically, we have O, S, Cr (Se?), and Mo, in almost a straight line, also P, V (As?), Nb and Sb. Many other curious relationships develop themselves if we plot off the elements vertically as well as horizontally. Is there any explanation of these curious curves? or is it simply accident? and if already known where can I find an account of them?

DONALD MURRAY.

Herald Office, Auckland, N.Z.

[Would not the position of Be (Beryllium) rather affect the apparent parallelism in these curves?—ED.]

The Leaps of *Lepus.*

WHILE rambling in the winter-time over the snow-covered plains in this region, I have recently interested myself in ascertaining how far, on a level surface, a hare or rabbit may leap at each spring, at a time when either of these animals is put to its best speed. Two species of *Lepus* are quite abundant in this vicinity, viz. the Mexican hare (*L. callotis callotis*), and the sage hare, which is really a medium-sized rabbit (*L. sylvaticus Nuttalli*), while the first-mentioned is a big hare. It is not uncommon to find here, in certain localities, a stretch of perfectly level prairie extending for a distance of 3 or 4 miles, and when this is covered by an even layer of 1 inch or more of snow, it offers an admirable surface on which to take account of the distance which may separate any two tracks of one of these animals, either one made by a hare or one made by one of the rabbits. On such a prairie as I have just referred to, I have on numerous occasions fired at these animals when they have been running, and at the same time beyond the range of my fowling-piece; such a shot almost invariably has the effect of so alarming the game as to make it run at its very best rate of speed, and, upon coming up with the tracks they have left on the snow at such times, I have been surprised at the distances they can clear at each individual leap. Under these conditions I once measured the spaces cleared by an old Mexican hare, and found the first two equalled 12 feet apiece, while the third effort was rather more than 13 feet, and I have never known this species to exceed this, although I have tested not a few of them. Of course the rabbit cannot compete with such magnificent gymnastics as this: it will, however, when thus frightened, make leaps of fully 6 feet; and on one occasion I measured one on the dead-level prairie, which was rather more than 7 feet. At their common rate of going, the hare rarely clears more than 4 feet at any single leap, while the rabbit is satisfied with rather more than 2 feet, and, when quietly feeding about the sage-brush, the tracks made by an individual of either species may actually overlap each other. R. W. SHUFELDT.

Fort Wingate, New Mexico, December 6, 1887.

A NEW MAGNETIC SURVEY OF FRANCE.[1]

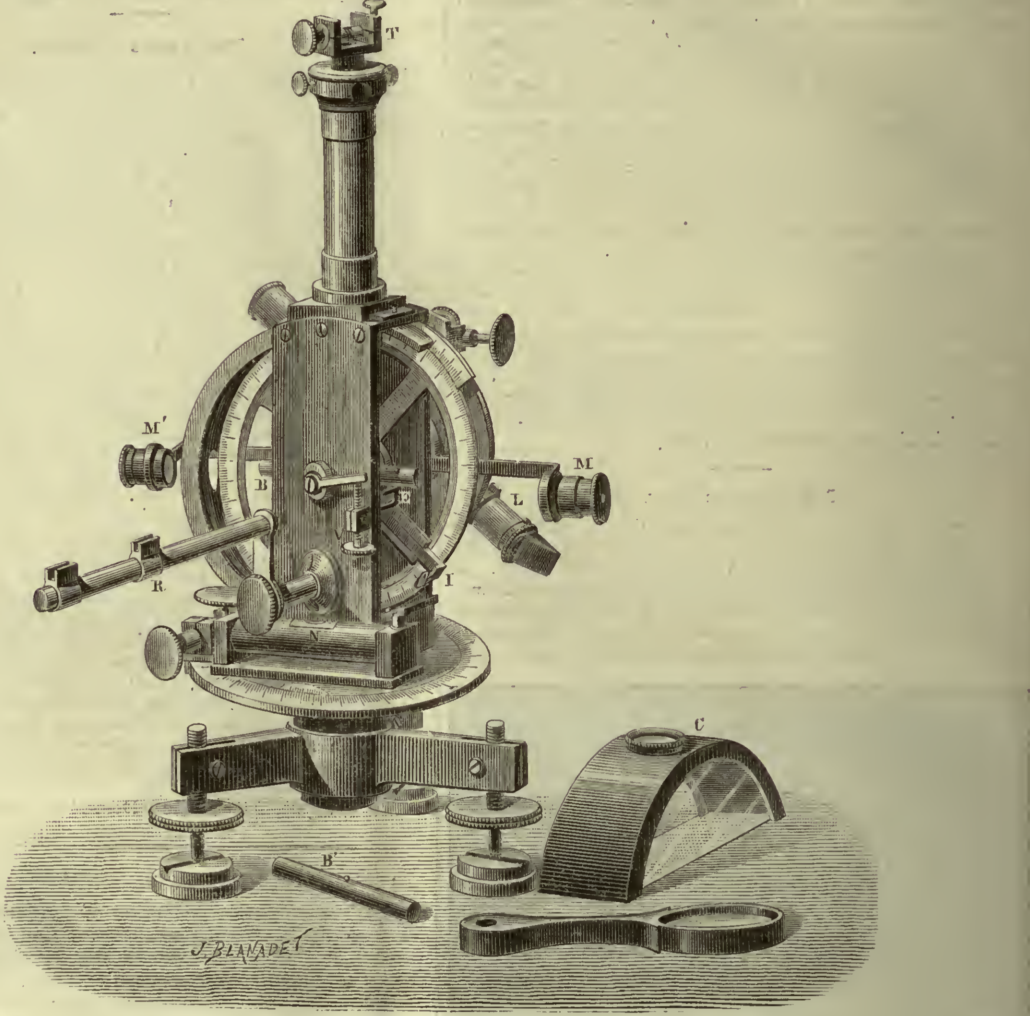
THE first systematic series of magnetic observations made in France was undertaken by Lamont, who in 1856 and 1857 determined the absolute value of the different elements at forty-four stations. The results are contained in his "Untersuchungen über die Richtung und Stärke der Erdmagnetismus an Verschiedenen Puncten des Südwestlichen Europa," and are reduced to three mean epochs: declination to March 1854; horizontal component to June 1848; and dip to the August of the same year. In 1868 and 1869 the Rev. Father Perry made a second series of observations of the intensity and direction of the earth's magnetic force at thirty-three stations in France (Phil. Trans., vols. clx. and clxii.). Determinations of declination have also been made at about twenty stations by MM. Marié-Davy and Descroix in 1875; and declination, dip, and intensity have been observed by M. de Bernardières at various points along

[1] "Détermination des Éléments Magnétiques en France." Ouvrage accompagné de nouvelles Cartes Magnétiques dressées pour le 1er Janvier. 1885 Par M. Th. Moureaux, Météorologiste-Adjoint au Bureau Central, Chargé du Service Magnétique à l'Observatoire du Parc Saint-Maur. (Paris: Gauthier-Villars, 1886.)

the Mediterranean littoral. These observations comprised all that was known respecting the distribution of the magnetic elements and rate of secular change in France prior to the appearance of the important work which forms the subject of this notice.

The observations of M. Moureaux were undertaken at the instigation of M. Mascart, the Director of the Meteorological Observatory of the Parc Saint Maur, and were made during the years 1884 and 1885. A few observations made in 1882 by M. Mascart and M. Moureaux in the neighbourhood of the Pyrenees are also included. A description of the instruments employed, of the methods of observation, together with a detailed account of the results obtained from about eighty stations, fairly well distributed over France, constitute the subject-matter of this memoir.

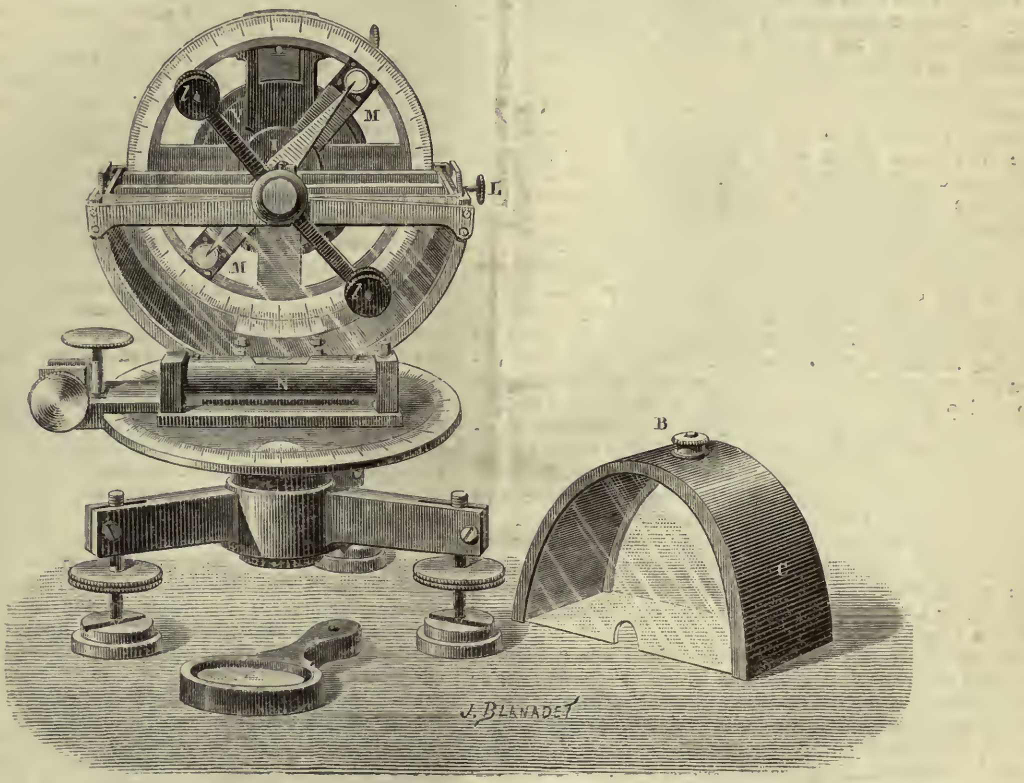
As the instruments employed by M. Moureaux differ in some important particulars from those which are ordinarily employed for field-work by us, it may be desirable to point out their peculiarities. The instruments which are mainly made use of in this country, and which have been employed by English observers who have made magnetic surveys in other parts of the world during the last quarter of a century, are of what is known as the Kew pattern,



Portable Magnetometer. B, magnet; E, apparatus for steadying magnet; N, level; M M', reading microscopes; L, telescope; T, torsion head; R, bar for deflection experiments.

and embody the results of the experience of such practical magneticians as Lloyd, Sabine, Airy, Welsh, Balfour Stewart, Whipple, and others. Indeed it may be said that almost every observer who has made any extensive series of measurements of terrestrial magnetism has influenced the construction of the Kew magnetometer, and there is no question that this instrument, although not absolutely perfect, has now reached a very high degree of excellence. In some respects, however, the magnetometer employed by M. Moureaux possesses advantages over the Kew pattern, and these are especially evident in surveys over rough and difficult country, and where the means of transport are limited. In the matter of weight alone there is a considerable difference. A Kew magnetometer, in its box complete, and exclusive of the deflection bar, which is now usually carried in a hollow leg of the tripod, weighs nearly 50 pounds, whereas that of the French observers weighs only about 9 pounds. A further advantage possessed by the French model is that it is also an alt-azimuth instrument, and hence the observer is less dependent upon the knowledge of true time, afforded by his chronometer, in determining the geographical meridian in a declination observation than he is with the English instrument. In the magnetic survey of Scotland made

by Welsh in 1857-58 it was necessary to make use of a special altazimuth instrument, or of a sextant and artificial horizon, in order to determine the sun's altitude at the time of observation, and a similar method was employed by the Rev. Father Perry in the course of the magnetic survey of France to which reference has already been made. Thanks to the admirable arrangement of our Post Office by which signals giving Greenwich mean time are sent to all the postal telegraph stations in the United Kingdom, it is possible for an observer engaged in magnetic work in the British Isles to determine the error and rate of his chronometer with an accuracy sufficient to enable him to dispense with the labour and trouble involved in the use of an altazimuth instrument. But unfortunately Greenwich mean time is not yet flashed all over the world, and a surveyor making use of the Kew magnetometer in distant countries would be under the necessity of making independent observations for solar altitude, and hence of adding to his *impedimenta* some such arrangement as those used in former surveys. Nor does this diminution in weight of the French instrument materially influence the accuracy of the observations, at all events so far as declination is concerned. It is hardly possible with the English instrument, even under favourable conditions, to obtain a declination observation which shall be accurate to within 2'. And yet, so far as an analysis of the data given by M. Moureaux enables us to judge, his instrument, of which the circle is only 0·08 m. in diameter, gives results which are in at least as close accordance with the truth. The method of observation which M. Moureaux adopts in determining the magnetic meridian allows him to read the position of both ends of the magnet both when erect and inverted in its stirrup. The magnets are solid and cylindrical in form, 6·5 cm. in length and 0·4 cm. in diameter, and weigh about 7½ grammes, and are suspended by a single thread



Iclinometer. M M, reading microscopes; L, lifting apparatus for needle; I, dipping needle; C, cover; N, level.

of silk 0·11 m. in length. The ends of the magnets are made slightly concave, and are polished so as to reflect the cross-wire placed in each of the microscopes, through which the readings for position are made. Each determination of geographical meridian is the mean result of from four to six independent observations, which rarely differ among themselves by more than 1' of arc.

The same magnet which serves for the observation of declination is used as in the Kew instrument for the determination of the horizontal component, which is done, as with us, by finding the relation $\frac{H}{M}$ by Gauss's method of deflections, and the product HM by the method of vibrations, whence H can be deduced. For this particular determination it seems to us that the Kew model is distinctly to be preferred. Indeed, in the observation of deflections the Kew instrument leaves very little to be desired, provided that care is taken to avoid sudden alterations of temperature, say by exposure to sunshine. The main error in the estimation of the period of vibration of the magnet also arises from the uncertainty of its temperature when observing in the field. But in the French instrument no special pains are taken either to ascertain or to correct for temperature. M. Moureaux indeed is of opinion that under the conditions of observation, the error committed by neglecting the correction is not greater than that which results from the difficulty of knowing whether the temperature of the magnet is represented by that of the outside thermometer. This is no doubt true of the instrument employed by the French observer, but in the Kew pattern special attention is paid to this point, and, although the arrangement leaves something to be desired, there is no doubt that with care the temperature may be determined with a fairly close

approximation to truth. Moreover, the method of determining the time of vibration of the magnet as generally practised by English observers also appears to us to be preferable to that adopted by M. Moureaux, although this has the advantage of occupying little time and therefore of minimizing the effect of any alteration in temperature during these observations and those of the deflections.

As regards inclination, there can, we think, be little doubt that the Kew pattern of dip circle, as made by Dover, is distinctly preferable to that used in the French survey. Indeed in the latter instrument it would seem to be difficult to avoid draughts and dust, the two great enemies to accuracy in field work. Only one needle of 0·065 m. in length was used by M. Moureaux, and the memoir gives no direct evidence of the degree of accuracy of which it was capable. Still M. Moureaux's instrument has the merit of portability, since, when packed in its box, it weighs less than 2 kilos.

As regards the plan of operations, we cannot speak too highly. Every care seems to have been taken, by a preliminary study of the ground, to select stations which should be as free as possible from any local disturbance, such as the proximity of railway-lines, manufactories, &c. It would, however, have added to the completeness of M. Moureaux's work if to the description of the stations there had been given some account of their general geological character and of that of the districts in the immediate neighbourhood, since, as is well known, the presence of igneous rocks or of rocks containing magnetic oxide of iron is the chief cause of local disturbance.

M. Moureaux began operations at each station, as a rule, at the commendably early hour of 7 a.m., so as to secure the determination of the magnetic meridian when the diurnal variation in declination was at about its morning minimum and nearly stationary. The observations for the geographical meridian were made between 8.30 and 9 a.m.; that is, at about the best period for the observation. The determination of the horizontal component was next made, a set of swings being taken before and after the deflection observations, all of which were completed by about 10.30 a.m. Between this time and noon was occupied in the dip observations. When the circumstances of travel or of weather made a departure from this plan necessary, the observations of declination were made either at the time of maximum of diurnal variation or at about the time of evening minimum—say between 5 and 6 p.m.

The results of the various observations are presented with that elegance and clearness which is characteristic of the publications of the Bureau Central Météorologique. They are all referred to the Parc Saint-Maur as a base station, by direct comparisons with the photographic curves of the registering apparatus at work in the magnetic observatory; and are reduced to the mean epoch January 1, 1885, by adding the difference between the values obtained at the different stations and Parc Saint-Maur at the time of observation to the corresponding values at Parc Saint-Maur on January 1, 1885, obtained from the mean of the observations made there in December 1884 and January 1885. This method presupposes that the diurnal variation is of the same order throughout the whole of France, which is not strictly true, but the error resulting from this mode of treatment is probably not greater than the errors of the observations themselves.

The final values are then tabulated and compared with the values obtained for the same places as deduced from the curves given by Lamont, and in this way a measure of the secular change is obtained. The results are finally plotted in the form of maps on Mercator's projection, giving lines of equal declination, force (horizontal component), and dip, and there is, lastly, a map of magnetic meridians. As to the methods employed in the construction of these maps there are unfortunately no details. It would seem that the lines are simply free-hand curves, so drawn as to best represent the observational results. There is at least no evidence that the results have been combined, as is the practice among English magneticians, so as to obtain the most probable direction of the lines by calculation, and therefore independently of bias on the part of the map-maker. M. Moureaux moreover offers us no direct means of comparing the values as taken out from his curves with the actual values obtained at the various stations. The maps, however, show certain points of interest which may be thus briefly summarized:—

(1) In the north of France the declination varies about 30′ for each degree of longitude; this proportion decreases in the south. The difference in declination between two points at a given distance apart on the same parallel increases with the latitude, and the isogonal lines are closer together in the north than in the south. The most remarkable feature in the declination map is the form of the curves in Brittany and more especially in the neighbourhood of Rennes. Their regularity is broken in such manner as to suggest that they are modified by the particular trend of the coast-line. Throughout the whole of the north-west portion of France the declination is less than would be expected from the direction and character of the lines over the rest of the Continent. A comparison with Lamont's map for 1854 shows that the declination has diminished during the thirty years by about 3° 58′ in the north, and by about 3° 19′ in the south of France. The mean annual decrease in declination seems to increase pretty regularly from south-south-east to north-north-west, or in a direction approximating to that of the magnetic north; hence the curves of equal declination have not been displaced, by time, parallel to themselves, but have gradually approached to the direction of the geographical meridian.

(2) The map of lines of equal horizontal component shows that the minimum, 0·18460 (C.G.S. units), is observed at Dunkirk, and the maximum, 0·22124, at Perpignan, or a difference of 0·03664 for the interval of 8° of latitude which separates the two points. The maximum rate of decrease of the horizontal component takes place in a direction approximating to that of the magnetic meridian. The decrease is more rapid in the south than in the north, and the interval between two consecutive curves increases pretty regularly with the latitude. The direction of these lines, like those of declination, seems to be modified towards the north-west of France, in such manner that the line corresponding to 0·190 is nearly straight and does not bend to the south as do the others. At places in the extreme north-west of France the value of the horizontal component is therefore greater than the general direction of the other lines would indicate should be the case. A comparison with Lamont's map for 1848 shows that the horizontal component has increased from about 0·008 to 0·010 in absolute value during the thirty-six years. The lines of equal horizontal component have not been displaced parallel to themselves, but are more inclined towards the east, so as to approach the direction of the geographical parallels. The secular change is at its maximum in the west, and diminishes slightly towards the east.

(3) The map of isoclinals shows that these lines have sensibly the same orientation as the lines of equal horizontal component; *i.e.* these are very nearly normal to the direction of the magnetic needle. Whilst the inclination diminishes in general towards the south, the interval between two consecutive curves decreases pretty regularly with the latitude. The direction of the lines corresponding to 66° and 67° seems to be slightly modified as they cross the north-west part of France, as are the lines of equal horizontal component. During the thirty-six years which have intervened since the date of Lamont's map, the dip has decreased by about 1° 35′ in

the north and by about 2° in the south, and, like the lines of horizontal component, the isoclinals have not been displaced parallel to themselves, but in a direction approximating to that of the parallels of latitude. The secular change is least in the north east and gradually increases towards the south, and attains its maximum along the Pyrenees and towards the Gulf of Genoa.

M. Moureaux is to be congratulated on the results of his work, for his countrymen have hitherto scarcely contributed their fair share to our knowledge of terrestrial magnetism. Even the surveys of their own country have been made for them by Germans and Englishmen. Now that Frenchmen themselves have made a beginning, it is to be hoped that the continuity of the work will not be interrupted, for it is only by systematic survey work of the kind so successfully accomplished by M. Moureaux that our knowledge of the magnetic state of the earth and of the laws which regulate its changes can be elucidated.

T. E. THORPE.

TIMBER, AND SOME OF ITS DISEASES.[1]

IV.

BEFORE proceeding further it will be of advantage to describe another tree-killing fungus, which has long been well known to mycologists as one of the commonest of our toadstools growing from rotten stumps, and decaying wood-work such as old water-pipes, bridges, &c. This is *Agaricus melleus* (Fig. 15), a tawny yellow toadstool with

FIG. 15.—A small group of *Agaricus* (*Armillaria*) *melleus*. The toad-stool is tawny-yellow, and produces white spores; the gills are decurrent, and the stem bears a ring. The fine hair-like appendages on the pileus should be bolder.

a ring round its stem, and its gills running down on the stem and bearing white spores, and which springs in tufts from the base of dead and dying trees during September and October. It is very common in this country, and I have often found it on beeches and other trees in Surrey, but it has been regarded as simply springing from the dead rotten wood, &c., at the base of the tree. As a matter of fact, however, this toadstool is traced to a series of dark shining strings, looking almost like the purple-black leaf-stalks of the maidenhair fern, and these strings branch and meander in the wood of the tree, and in the soil, and may attain even great lengths—several feet, for instance. The interest of all this is enhanced when we know that until the last few years these long black cords were supposed to be a peculiar form of fungus, and were known as *Rhizomorpha*. They are, however, the subterranean vegetative parts (mycelium) of the Agaric we are concerned with, and they can be traced without break of continuity from the base of the toadstool into the soil and tree (Fig. 16). I have several times followed these dark mycelial cords into the timber of old beeches and spruce-fir stumps, but they are also to be found in oaks, plums, various Conifers, and probably may occur in most of our timber-trees if opportunity offers.

The most important point in this connection is that *Agaricus melleus* becomes in these cases a true parasite,

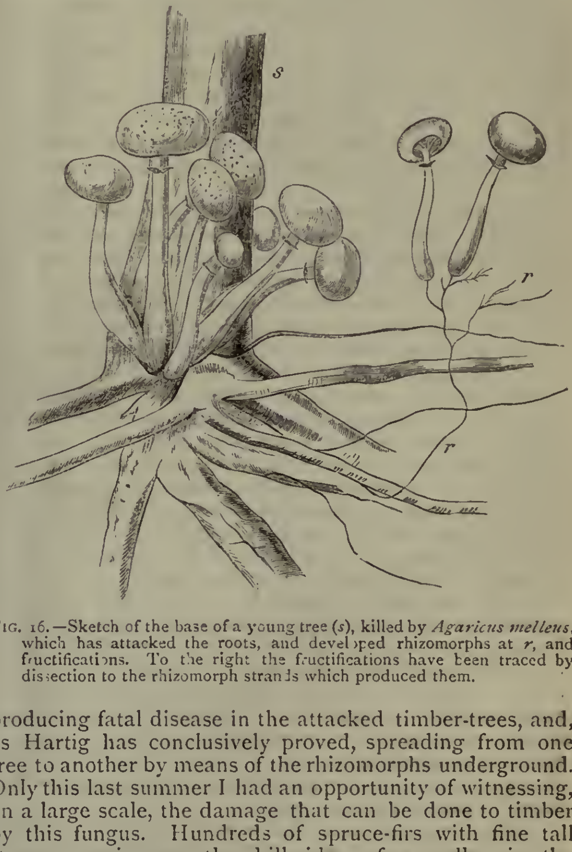


FIG. 16.—Sketch of the base of a young tree (*s*), killed by *Agaricus melleus*, which has attacked the roots, and developed rhizomorphs at *r*, and fructifications. To the right the fructifications have been traced by dissection to the rhizomorph strands which produced them.

producing fatal disease in the attacked timber-trees, and, as Hartig has conclusively proved, spreading from one tree to another by means of the rhizomorphs underground. Only this last summer I had an opportunity of witnessing, on a large scale, the damage that can be done to timber by this fungus. Hundreds of spruce-firs with fine tall stems, growing on the hill sides of a valley in the Bavarian Alps, were shown to me as "victims to a kind of rot." In most cases the trees (which at first sight appeared only slightly unhealthy) gave a hollow sound when struck, and the foresters told me that nearly every tree was rotten at the core. I had found the mycelium of *Agaricus melleus* in the rotting stumps of previously felled trees all up and down the same valley, but it was not satisfactory to simply assume that the "rot" was the same in both cases, though the foresters assured me it was so.

By the kindness of the forest manager I was allowed to fell one of these trees. It was chosen at hazard, after the men had struck a large number, to show me how easily the hollow trees could be detected by the sound.

[1] Continued from p. 229.

The tree was felled by sawing close to the roots: the interior was hollow for several feet up the stem, and two of the main roots were hollow as far as we could poke canes, and no doubt further. The dark-coloured rotting mass around the hollow was wet and spongy, and consisted of disintegrated wood held together by a mesh-work of the rhizomorphs. Further outwards the wood was yellow, with white patches scattered in the yellow matrix, and, again, the rhizomorph-strands were seen running in all directions through the mass.

Not to follow this particular case further—since we are concerned with the general features of the diseases of timber—I may pass to the consideration of the diagnosis of this disease caused by *Agaricus melleus*, as contrasted with that due to *Trametes radiciperda*.

Of course no botanist would confound the fructification of the *Trametes* with that of the *Agaricus;* but the fructifications of such fungi only appear at certain seasons, and that of *Trametes radiciperda* may be underground, and it is important to be able to distinguish such forms in the absence of the fructifications.

The external symptoms of the disease, where young trees are concerned, are similar in both cases. In a plantation at Freising, in Bavaria, Prof. Hartig showed me young Weymouth pines (*P. Strobus*) attacked and killed by *Agaricus melleus*. The leaves turn pale and yellow, and the lower part of the stem—the so-called "collar"—begins to die and rot, the cortex above still looking healthy. So far the symptoms might be those due to the destructive action of other forms of tree-killing fungi.

On uprooting a young pine, killed or badly attacked by the Agaric, the roots are found to be matted together with a ball of earth permeated by the resin which has flowed out: this is very pronounced in the case of some pines, less so in others. On lifting up the scales of the bark, there will be found, not the silky, white, delicate mycelium of the *Trametes*, but probably the dark cord-like rhizomorphs: there may also be flat white rhizomorphs in the young stages, but they are easily distinguished. These dark rhizomorphs may also be found spreading around into the soil from the roots, and they look so much like thin roots indeed that we can at once understand their name—rhizomorph. The presence of the rhizomorphs and (in the case of the resinous pines) the outflow of resin and sticking together of soil and roots are good distinctive features. No less evident are the differences to be found on examining the diseased timber, as exemplified by Prof. Hartig's magnificent specimens. The wood attacked assumes brown and bright yellow colours, and is marked by sharp brown or nearly black lines, bounding areas of one colour and separating them from areas of another colour. In some cases the yellow colour is quite bright—canary yellow, or nearly so. The white areas scattered in this yellow matrix have no black specks in them, and can thus be distinguished from those due to the *Trametes*. In advanced stages the purple-black rhizomorphs will be found in the soft, spongy wood.

The great danger of *Agaricus melleus* is its power of extending itself beneath the soil by means of the spreading rhizomorphs: these are known to reach lengths of several feet, and to pass from root to root, keeping a more or less horizontal course at a depth of 6 or 8 inches or so in the ground. On reaching the root of another tree, the tips of the branched rhizomorph penetrate the living cortex, and grow forward in the plane of the cambium, sending off smaller ramifications into the medullary rays and (in the case of the pines, &c.) into the resin passages. The hyphæ of the ultimate twigs enter the tracheides, vessels, &c., of the wood, and delignify them, with changes of colour and substance as described. Reference must be made to Prof. Hartig's publications for the details which serve to distinguish histologically between timber attacked by *Agaricus melleus* and by *Trametes* or other fungi. Enough has been said to show that diagnosis is possible, and indeed, to an expert, not difficult.

It is at least clear from the above sketch that we can distinguish these two kinds of diseases of timber, and it will be seen on reflection that this depends on knowledge of the structure and functions of the timber and cambium on the one hand, and proper acquaintance with the biology of the fungi on the other. It is the victory of the fungus over the timber in the struggle for existence which brings about the disease; and one who is ignorant of these points will be apt to go astray in any reasoning which concerns the whole question. Anyone knowing the facts and understanding their bearings, on the contrary, possesses the key to a reasonable treatment of the timber; and this is important, because the two diseases referred to can be eradicated from young plantations and the areas of their ravages limited in older forests.

Suppose, for example, a plantation presents the following case. A tree is found to turn sickly and die, with the symptoms described, and trees immediately surrounding it are turning yellow. The first tree is at once cut down, and its roots and timber examined, and the diagnosis shows the presence of *Agaricus melleus* or of *Trametes radiciperda*, as the case may be. Knowing this, the expert also knows more. If the timber is being destroyed by the *Trametes*, he knows that the ravaging agent can travel from tree to tree by means of roots in contact, and he at once cuts a ditch around the diseased area, taking care to include the recently-infected and neighbouring trees. Then the diseased timber is cut, because it will get worse the longer it stands, and the diseased parts burnt. If *Agaricus melleus* is the destroying agent, a similar procedure is necessary; but regard must be had to the much more extensive wanderings of the rhizomorphs in the soil, and it may be imperative to cut the moat round more of the neighbouring trees. Nevertheless, it has also to be remembered that the rhizomorphs run not far below the surface. However, my purpose here is not to treat this subject in detail, but to indicate the lines along which practical application of the truths of botanical science may be looked for. The reader who wishes to go further into the subject may consult special works. Of course the spores are a source of danger, but need be by no means so much so where knowledge is intelligently applied in removing young fructifications.

I will now pass on to a few remarks on a class of disease-producing timber fungi which present certain peculiarities in their biology. The two fungi which have been described are true parasites, attacking the roots of living trees, and causing disease in the timber by travelling up the cambium, &c., into the stem: the fungi I am about to refer to are termed wound-parasites, because they attack the timber of trees at the surfaces of wounds, such as cut branches, torn bark, frost-cracks, &c., and spread from thence into the sound timber. When we are reminded how many sources of danger are here open in the shape of wounds, there is no room for wonder that such fungi as these are so widely spread. Squirrels, rats, cattle, &c., nibble or rub off bark; snow and dew break branches; insects bore into stems; wind, hail, &c., injure young parts of trees; and in fact small wounds are formed in such quantities that if the fructifications of such fungi as those referred to are permitted to ripen indiscriminately, the wonder is not that access to the timber is gained, but rather that a tree of any considerable age escapes at all.

One of the commonest of these is *Polyporus sulphureus*, which does great injury to all kinds of standing timber, especially the oak, poplar, willow, hazel, pear, larch, and others. It is probably well known to all foresters, as its fructification projects horizontally from the diseased trunks as tiers of bracket-shaped bodies of a cheese-like

consistency; bright yellow below, where the numerous minute pores are, and orange or somewhat vermilion above, giving the substance a coral-like appearance. I have often seen it in the neighbourhood of Englefield Green and Windsor, and it is very common in England generally.

If the spore of this *Polyporus* lodges on a wound which exposes the cambium and young wood, the filaments grow into the medullary rays and the vessels, and soon spread in all directions in the timber, especially longitudinally, causing the latter to assume a warm brown colour and to undergo decay. In the infested timber are to be observed radial and other crevices filled with the dense felt-like mycelium formed by the common growth of the innumerable branched filaments. In bad cases it is possible to strip sheets of this yellowish white felt-work out of the cracks, and on looking at the timber more closely (of the oak, for instance) the vessels are found to be filled with the fungus filaments, and look like long white streaks in longitudinal sections of the wood—showing as white dots in transverse sections.

It is not necessary to dwell on the details of the histology of the diseased timber: the ultimate filaments of the fungus penetrate the walls of all the cells and vessels, dissolve and destroy the starch in the medullary rays, and convert the lignified walls of the wood elements back again into cellulose. This evidently occurs by some solvent action, and is due to a ferment excreted from the fungus filaments, and the destroyed timber becomes reduced to a brown mass of powder.

I cannot leave this subject without referring to a remarkably interesting museum specimen which Prof. Hartig showed and explained to me this summer. This is a block of wood containing an enormous irregularly spheroidal mass of the white felted mycelium of this fungus, *Polyporus sulphureus*. The mass had been cut clean across, and the section exposed a number of thin brown ovoid bodies embedded in the closely-woven felt: these bodies were of the size and shape of acorns, but were simply hollow shells filled with the same felt-like mycelium as that in which they were embedded. They were cut in all directions, and so appeared as circles in some cases. These bodies are, in fact, the outer shells of so many acorns, embedded in and hollowed out by the mycelium of *Polyporus sulphureus*. Hartig's ingenious explanation of their presence speaks for itself. A squirrel had stored up the acorns in a hollow in the timber, and had not returned to them—what tragedy intervenes must be left to the imagination. The *Polyporus* had then invaded the hollow, and the acorns, and had dissolved and destroyed the cellular and starchy contents of the latter, leaving only the cuticularized and corky shells, looking exactly like fossil eggs in the matrix. I hardly think geology can beat this for a true story.

The three diseases so far described serve very well as types of a number of others known to be due to the invasion of timber and the dissolution of the walls of its cells, fibres, and vessels by Hymenomycetous fungi, *i.e.* by fungi allied to the toadstools and polypores. They all "rot" the timber by destroying its structure and substance, starting from the cambium and medullary rays.

To mention one or two additional forms, *Trametes Pini* is common on pines, but, unlike its truly parasitic ally, *Tr. radiciperda*, which attacks sound roots, it is a wound-parasite, and seems able to gain access to the timber only if the spores germinate on exposed surfaces. The disease it produces is very like that caused by its ally: probably none but an expert could distinguish between them, though the differences are clear when the histology is understood.

Polyporus fulvus is remarkable because its hyphæ destroy the middle-lamella, and thus isolate the tracheides in the timber of firs; *Polyporus borealis* also produces disease in the timber of standing Conifers; *Polyporus igniarius* is one of the commonest parasites on trees such as the oak, &c., and produces in them a disease not unlike that due to the last form mentioned; *Polyporus dryadeus* also destroys oaks, and is again remarkable because its hyphæ destroy the middle-lamella.

With reference to the two fungi last mentioned I cannot avoid describing a specimen in the Museum of Forest Botany in München, since it seems to have a possible bearing on a very important question of biology, viz. the action of soluble ferments.

It has already been stated that some of these tree-killing fungi excrete ferments which attack and dissolve starch-grains, and it is well known that starch-grains are stored up in the cells of the medullary rays found in timber. Now, *Polyporus dryadeus* and *P. igniarius* are such fungi; their hyphæ excrete a ferment which completely destroys the starch-grains in the cells of the medullary rays of the oak, a tree very apt to be attacked by these two parasites, though *P. igniarius*, at any rate, attacks many other dicotyledonous trees as well. It occasionally happens that an oak is attacked by both of these Polyporei, and their mycelia become intermingled in the timber: when this is the case the *starch-grains remain intact in those cells which are invaded simultaneously by the hyphæ of both fungi*. Prof. Hartig lately showed me longitudinal radial sections of oak-timber thus attacked, and the medullary rays showed up as glistening white plates. These plates consist of nearly pure starch: the hyphæ have destroyed the cell-walls, but left the starch intact. It is easy to suggest that the two ferments acting together exert (with respect to the starch), a sort of inhibitory action one on the other; but it is also obvious that this is not the ultimate explanation, and one feels that the matter deserves investigation.

It now becomes a question—What other types of timber-diseases shall be described? Of course the limits of a popular article are too narrow for anything approaching an exhaustive treatment of such a subject, and nothing has as yet been said of several other diseases due to crust-like fungi often found on decaying stems, or of others due to certain minute fungi which attack healthy roots. Then there is a class of diseases which commence in the bark or cortex of trees, and extend thence into the cambium and timber: some of these "cankers," as they are often called, are proved to be due to the ravages of fungi, though there is another series of apparently similar "cankers" which are caused by variations in the environment—the atmosphere and weather generally.

It would need a long article to place the reader *au courant* with the chief results of what is known of these diseases, and I must be content here with the bare statement that these "cankers" are in the main due to local injury or destruction of the cambium. If the normal cylindrical sheet of cambium is locally irritated or destroyed, no one can wonder that the thickening layers of wood are not continued normally at the locality in question: the uninjured cells are also influenced, and abnormal cushions of tissue formed which vary in different cases. Now, in "cankers" this is—put shortly—what happens: it may be, and often is, due to the local action of a parasitic fungus; or it may be—and, again, often is—owing to injuries produced by the weather, in the broad sense, and saprophytic organisms may subsequently invade the wounds.

The details as to how the injury thus set up is propagated to other parts—how the "canker" spreads into the bark and wood around—*are* details, and would require considerable space for their description: the chief point here is again the destructive action of mycelia of various fungi, which by means of their powers of pervading the cells and vessels of the wood, and of secreting soluble ferments which break down the structure of the timber, render the latter diseased and unfit for use. The only too well known larch-disease is a case in point; but, since

this is a subject which needs a chapter to itself, I may pass on to more general remarks on what we have learnt so far.

It will be noticed that, whereas such fungi as *Trametes radiciperda* and *Agaricus melleus* are true parasites which can attack the living roots of trees, the other fungi referred to can only reach the interior of the timber from the exposed surfaces of wounds. It has been pointed out along what lines the special treatment of the former diseases must be followed, and it only remains to say of the latter: take care of the cortex and cambium of the tree, and the timber will take care of itself. It is unquestionably true that the diseases due to wound-parasites can be avoided if no open wounds are allowed to exist. Many a fine oak and beech perishes before its time, or its timber becomes diseased and a high wind blows the tree down, because the spores of one of these fungi alight on the cut or torn surface of a pruned or broken branch. Of course it is not always possible to carry out the surgical operations, so to speak, which are necessary to protect a tree which has lost a limb, and in other cases no doubt those responsible have to discuss whether it costs more to perform the operations on a large scale than to risk the timber. With these matters I have nothing to do here, but the fact remains that by properly closing over open wounds, and allowing the surrounding cambium to cover them up, as it will naturally do, the term of life of many a valuable tree can be prolonged, and its timber not only prevented from becoming diseased and deteriorating, but actually increased in value.

There is no need probably for me to repeat that, although the present essay deals with certain diseases of timber due to fungi, there are other diseases brought about entirely by inorganic agencies. Some of these were touched upon in the last article, and I have already put before the readers of NATURE some remarks as to how trees and their timber may suffer from the roots being in an unsuitable medium.

In the next paper it is proposed to deal with the so-called "dry-rot" in timber which has been felled and cut up—a disease which has produced much distress at various times and in various countries.

H. MARSHALL WARD.

(*To be continued.*)

PERPETUAL MOTION.[1]

IF we study the past in order to trace the development of machines, we cannot help being astonished at the long centuries during which man was content to employ only his own muscular effort and that of animals, instead of utilizing the other forces of Nature to do his work; for it is a striking fact that it is during little more than the last quarter of a century that the power of the steam-engine has in the aggregate become twice as great as that of the whole working population of the world.

Although the early history of the subject is shrouded in obscurity, there is little doubt that the power of water was the first to be employed. We can easily imagine that, in those early days when the laws of Nature were so little understood, the idea would arise that, if some machine could be contrived which would not get tired like man or animal, as machines appeared to do when left to themselves, and, moreover, one which did not depend upon a capricious and variable supply of water, such a machine would go on for ever—in short, would have perpetual motion. As a matter of fact, Geiger, the German philologist, has adduced strong grounds for believing the Buddhist praying-wheels—on which the prayers of the worshippers were fastened, and which were turned by water power—to be probably the first kind of water motor; and at the same time the first record of a proposal for a perpetual motion machine appears to be in the "Siddhânta Ciromani," a Sanskrit text-book on astronomy, in which a wheel for this purpose is suggested, having a number of closed equidistant holes half filled with mercury upon a zigzag line round its rim. No doubt other suggestions of this kind were made from time to time, but writers and literary men did not condescend to notice them, or even the progress of the really practical and useful machines. We are thus brought from that distant date down to the thirteenth century, when we find in the sketch-book of an architect, Wilars de Honecort (the original being now in the École des Chartres, at Paris), a drawing of a proposed perpetual motion machine, with the statement which, translated, runs:—"Many a time have skilful workmen tried to contrive a wheel that shall turn of itself: here is a way to make such by means of an uneven number of mallets or by quicksilver." The engraving shows four mallets upon what is evidently meant to be the descending side of the wheel, and three upon the ascending side, the former therefore overbalancing the latter. To get the mallets into this desirable position the top one on the descending side has evidently been made to fall over before its time; but independently of this there is to the ordinary mind a strong suggestion of speedy dissolution in any structure a greater number of whose parts are going in one direction than in the other, but this little difficulty M. de Honecort does not allude to or discuss. The unevenly weighted wheel in which the action of gravity is to be cheated in some way or the other has appeared in a great variety of forms since, and, from the words "many a time," probably before, and is by far the most important type of proposed contrivance for perpetual motion.

About two centuries after De Honecort, the famous Leonardi da Vinci gives sketches of six designs, either due to his own fertile brain or taken from other sources, and since then there has been an incessant flow of proposals of this type of machine, a large number of which are given in the work of Dr. Henry Dirks, "Perpetuum Mobile," and several in vol. xii. of the *Mechanical World*.

The next class of proposed machines we may consider are those in which gravity was to be made use of in one direction and evaded in the opposite, by the agency of falling water, amongst these being the devices of Schott, Scheiner, Böckler, and others. The idea in all these was that a quantity of water might be kept circulating between two tanks, one above, and one below; being raised to the upper one by means of pumps driven by a water-wheel which derived its motion from the selfsame water in falling the same distance, there being a balance to the good in the form of extra work to be done by the wheel.

A third class of proposals suggests the application of capillary action to raise the water instead of employing pumps, one of the earliest being that of a Professor of of Philosophy in Glasgow about 200 years ago. In this case and others the drawings show (in anticipation) the water thus raised flowing out at the top in a good substantial stream, as, for instance, in the scheme of Branca about the date of the Professor's production.

The fourth and last class, which partook more of a philosophic nature, proposed to employ magnets, the attraction of which is to be effective in one position, and masked in another. There are many proposed ways of effecting this, all equally futile, although one contrived by a shoemaker of Linlithgow actually deceived for a time Sir David Brewster, who communicated an account of it to the *Annales de Chimie*. In the simplest a ball is to fall through a certain distance, so as to come into a position where it can be raised up an inclined plane by magnetic attraction. The first part is carried out in strict

[1] Abstract of a Lecture delivered by Prof. Hele Shaw, University College, on December 21, 1887, in St. George's Hall, Liverpool.

accordance with the programme, but the ball refuses to go through the second part without coercion.

Now most of these schemes had a very definite object in view, which was to obtain motive power, and not at all the innocent philosophic notion of delighting future ages by the sight of a machine which, like the sacred flame Mark Twain tells of, had been going for so many centuries; in short, it was not to benefit posterity but themselves that perpetual motion seekers worked and patented their inventions; and thus the question naturally arises, Did any of their inventions appear to work? Well, they did; and here we may divide these machines into two classes, those which did not succeed, and those which did. The former are in a strong majority, but the latter are important; and I will briefly give an account of one case, perhaps the most celebrated, of the latter. About the year 1712 a great stir was made on the Continent by the appearance of a wonderful machine contrived by a German Pole, by name Jean Ernst Elie-Bessler, who apparently (not perhaps having enough names) had assumed the additional surname Orffyreus. This Orffyreus had, it was said, contrived upwards of 300 perpetual motion machines, and at last had got one that worked. Kings, princes, landgraves, not to say professors and learned men, were all convinced of the absolute certainty of the action of the machine, and Baron Fischer writes to the celebrated Dr. Desaguliers as seriously as Prof. s'Gravesande did to Sir Isaac Newton about it as follows, concerning a visit paid to this machine in the castle of Wissenstein, in Cassel:—"The wheel turns with astonishing rapidity. Having tied a cord to the axle, to turn an Archimedean screw to raise water, the wheel then made twenty turns a minute. This I noted several times by my watch, and I always found the same regularity. An attempt to stop it suddenly would raise a man from the ground. Having stopped it in this manner it remained stationary (and here is the greatest proof of a perpetual motion). I commenced the movements very gently to see if it would of itself regain its former rapidity, which I doubted; but to my great astonishment I observed that the rapidity of the wheel augmented little by little until it made two turns, and then it regained its former speed. This experiment, showing the rapidity of the wheel augmented from the very slow movement that I gave it to an extraordinary rapid one, convinces me more than if I had only seen the wheel moving a whole year, which would not have persuaded me that it was perpetual motion, because it might have diminished little by little until it ceased altogether; but to gain speed instead of losing it, and to increase that speed to a certain degree in spite of the resistance of the air and the friction of the axles, I do not see how any one can doubt the truth of this action." The inventor himself wrote various pamphlets—with dedications 60 pages in length in German—entitled, "Das Triumphirende Perpetuum Mobile Orffyreanum," and in Latin, "Triumphans Perpetuum Mobile Orffyreanum." This machine worked hard, raising and lowering stones or water as required, being locked in a room; the people outside could see the work done by means of a rope which passed through an opening in the wall, and this ought to have satisfied them. Still, there were disbelievers, and amongst others we find a M. Crousaz writing as follows:—"First, Orffyreus is a fool; second, it is impossible that a fool can have discovered what such a number of clever people have searched for without success; third, I do not believe in impossibilities; . . . fifth, the servant who ran away from his house for fear of being strangled, has in her possession, in writing, the terrible oath that Orffyreus made her swear; sixth, he only had to have asked in order to have had this girl imprisoned, until he had time to finish this machine; . . . eighth, it is true that there is a machine at his house, to which they give the name of perpetual motion, but that is a small one and cannot be removed." These are serious charges even if not in logical sequence, and before we conclude the history of this invention we will examine a machine which has been made at University College, which has certainly surprising properties, although very simple. It is now locked, for we may say of it what was said of a machine about twenty years ago by the *Boston Journal*:—"It will not, nay cannot, stop without a brake, as it is so fixed by means of balls and arms that the descending side of the wheel is perpetually farther from the centre of motion than the opposition ascending." That is just our machine, which, started, behaves exactly as Baron Fischer describes, and raises a weight or does other work. This machine is so constructed as to enable complete examination to be made, and all possibility of unfair play apparently detected, and yet it is a fraud,[1] as was that of Mr. Orffyreus, which was afterwards exposed.

The conclusion we arrive at is, that it would have been well for a great number of folks if the saying due to Lucretius nearly 2000 years ago, "Ex nihilo nihil fit," [2] had been appreciated and believed in by them. Thus the waste of many lives of fruitless work might have been avoided not only in the past but even in the present day, for it is an astonishing fact that during the last twenty years more than 100 English and French patents for perpetual motion machines have been obtained; in one case a gentleman not very far from Liverpool having spent a very large sum on this profitable subject. The lecturer stated that the other day he had a visit *in propria persona* from an inventor of, and of course believer in, such a machine, and after having for an hour and a half discussed the question with this gentleman as calmly as was possible under the circumstances, he had grounds for feeling that his lecture would be utterly incomplete if he left the subject content with raising a laugh at the whole matter: not so very long ago it was easy enough to do this at the expense of railways and ocean steamers. He would therefore briefly and simply, but he hoped conclusively, state the general nature of the problem of perpetual motion. Firstly, all machines such as we have seen projected for creating power are as impossible as the idea of creating matter. Secondly, many machines have been projected for using sources of energy, such as heat, as proposed by Desaguliers, and many others since, in which known sources of power were to be rendered available. Such machines continue to work only while the supply of energy lasts, therefore have not perpetual motion. Thirdly, since, just as energy cannot be created, so it cannot be destroyed, but can only take another form, the question arises, Cannot the causes retarding a body's motion be removed and the body go on moving for ever? In order to answer this reasonable question, he proposed for a few moments to search for perpetual motion. He then proceeded to illustrate, by means of a variety of machines, what efforts had been made to reduce frictional resistance. In one case, an inventor working on the principle that in a wheel of half the size the friction was reduced in the same proportion proposed to employ two in this ratio; no doubt with the same idea as the man who, seeing a stove advertised to save half the usual quantity of coal, bought two with the idea of saving it all. Many people thought that, theoretically, friction was entirely removed by means of rolling contact—illustrated by roller and ball-bearings—but it was only because the theory was imperfect, and the true nature of rolling not understood; and, by means of lantern illustrations, the action of rolling surfaces was experimentally examined. The irresistible conclusion must be arrived at that friction is as universal in its action

[1] Being driven by concealed cords passing down the hollow legs and actuated by a youth beneath the platform.

[2] Propounded, indeed, in a different form by Democritus 400 years before that.

as gravitation, and to avoid it on the earth is impossible; and with this conclusion vanishes all hope of a perpetual motion machine. If we are inclined to regret this fact, a little reflection on what would occur if friction ceased to act may not be uninstructive, for the whole face of Nature would be at once changed, and much of the dry land, and, even more rapidly, most of our buildings, would disappear beneath the sea. Such inhabitants as remained for a short time alive would not only be unable to provide themselves with fire or warmth, but would find their very clothes falling back to the original fibre from which they were made; and if not destroyed in one of the many possible ways—such as by falling meteors, no longer dissipated by friction through the air, or by falling masses of water, no longer retarded by the atmosphere and descending as rain—would be unable to obtain food, from inability to move themselves by any ordinary method of locomotion, or, what would be equally serious, having once started into motion, from being unable to stop except when they came into collision with other unhappy beings or moving bodies. Before long they, with all heavier substances, would disappear for ever beneath the waters which would now cover the face of a lifeless world.

We turn to the motion of planetary bodies—is that perpetual? At first, everything seems to show that it is. The earth with its mass of 3000 trillion tons turns with a speed which enables a student to go bare-headed a good many miles without catching cold in the act of saluting a Professor, for a long time defied all attempts to detect in it loss of speed; but with the friction of the tides continually at work such loss must take place, and now it is pretty certain from the calculations of Adams, the astronomer, that the earth loses about an hour in 16,000 years, and is coming to rest, though it must be admitted rather leisurely. So, also, the hurrying up of the comets as they go round the sun is possibly accounted for by a retarding action in space which makes it necessary for them to try and make up, as it were, for lost time; and in fact the general arguments in the present day are in favour of what Sir Isaac Newton believed—that the motions of all bodies in space are suffering retardation, and that their velocity is becoming less and will ultimately cease.

Perpetual motion, then, is impossible. By no means. We have duly considered motion of matter in its visible and mechanical form, and if the foregoing remarks are true, then in this form assuredly it is; but there is, as we have seen, the great fact of indestructibility of energy, and the greatest generalization of the present century is that which accounts for the disappearance of energy in the form of mechanical and visible motion by showing that an exactly equal amount appears in the form of molecular and invisible motion. To this all outward motion tends, and friction is the agency by which the change is effected. Down to a certain point the change can be effected in either direction, and the heat-engine converts molecular motion into mechanical, again to be reconverted into molecular motion in all its working parts, as well as in connection with the useful work it does. This stage reached, there is no process known to us by which the cycle can be continued, and the term "degradation," in the sense of having gone down a step, but nevertheless a step which can never be reclaimed, is applied to the tendency of energy to assume molecular form by dissipation over a larger mass of matter, so that its effect is less intense, though equal numerically in amount. To this all Nature tends, and beyond this point we cannot go. Here, at any rate, the motion is perpetual, but it is motion that tends to approach a state unsatisfactory to the instinct of the human mind. Great intellects, such as Rankine and Siemens, have striven to conjecture ways at present unknown to us by which the energy now spreading itself over the vast expanse of space may be gathered again and regenerated, so that we may look forward not to the lowest but to the highest form of motion as that which, passing through all its cycles, shall last for ever.

THE CHAIR OF DARWINISM IN PARIS.

ONE of the most interesting evidences of the differing results of municipal organization in foreign countries, as compared with those resulting from such organization in our own, is the news that the Municipal Council of Paris intends to found (in connection with the Sorbonne, or the Jardin des Plantes, or the Collège de France, we do not know which) a Chair of Philosophical Zoology, with a special view to the propagation of the doctrine of evolution as elaborated by Darwin. It appears that the official naturalists in France—those holding the leading professorships and museum appointments—have not hitherto been very friendly to Darwinian doctrine. The Municipal Council of Paris has recognized the fact that there is an undesirable hostility to Darwin's views amongst the official group, and actually proposes to remedy the evil results of this hostility by establishing a new Chair, destined to give fair play and a full hearing to the new philosophy. It is as though the Corporation of London should propose to build and endow a laboratory of physiological experiment or of bacteriology. The imagination recoils before the task of picturing Mr. Alderman Greenfat expounding to his colleagues the importance to the community of scientific research, and carrying with him a large majority in favour of a scientific enterprise hitherto neglected and even penalized by middle-class authority.

There is very little doubt as to who is the fittest man in France at this moment to hold such a Chair as that which is now to be created. M. Giard, for many years Professor of Zoology at Lille, and only this year called to a similar Chair in Paris, has not only been the first in France to teach from an official position the doctrine of evolution in zoology, but has made many most valuable researches himself, and has created a school amongst whom are the ablest of the younger French zoologists. Every embryologist knows the works not only of Alfred Giard, but those of his pupils Barrois, Halley, Monnet, and others. Alfred Giard had to submit to some painful remonstrances, and to imperil his official career as a Professor of Zoology in France, when he determined to break with the traditions of his eminent master, Henri de Lacaze Duthiers, and to boldly accept Darwinism and the methods of the modern English and German school. It is therefore only right that his name should be the first to be considered in relation to the new Chair in Paris, and we have no hesitation in saying that, should he be appointed, a man will have been secured as the first occupant of a difficult position whose qualifications render it certain that he will not only do credit to himself, but will justify, by his successful teaching, the enlightened, patriotic, and high-minded initiative of the Municipality of Paris.

E. R. L.

NOTES.

ON the 3rd of this month there passed away a Scottish parish minister, who though not himself a scientific man has come in contact with three successive generations of men of science whom the love of travel or of geology has led to the picturesque island of Skye. The Rev. Dr. Donald Mackinnon was the third of his family who have been ministers of the parish of Strath. His grandfather was appointed to the incumbency in 1777, and held it for forty-nine years. His father took the office in 1826, and held it for thirty years, until he himself succeeded to it in 1856. The parish has thus been presided over by the same family for the long period of 110 years. Unfortunately none of

the numerous family of the deceased clergyman have entered the Church, so that the interesting ecclesiastical connection of the family with the parish now comes to an end. Dr. Mackinnon was a noble type of the true old Highland gentleman, dignified, courteous, kindly, and always the same, whether conversing with crofter or countess. He was delighted to tell his reminiscences of the old geologists. It was his uncle who put into visible expression by his famous but unspeakable "device of the pots" (as Barbour has it) the universal indignation of Skye at the account of the island and its inhabitants given by the geologist Macculloch, in his book on the Highlands and Western Islands. It was in his father's house that Sedgwick and Murchison were entertained when they passed through the north-west Highlands in 1827, and he had some amusing stories about the impression made on himself and his brothers by the doings of these two great brethren of the hammer. In later years geologists and other students of science, as well as artists and distinguished men of many kinds, have enjoyed the hospitality of his home at Kilbride under the shadow of the great mountain, and in sight of the gleaming Atlantic. Only a few months ago he had an opportunity of renewing his early love for mineralogy and geology, and while riding on his favourite quiet cob, looking after his farm-servants as they harvested between the showers of a Skye September, he would stop now and again to point out geological features that had been familiar and interesting to him from boyhood. He belonged to a type of Scottish clergyman that is slowly disappearing, and carries with him the affectionate regrets of everyone who was privileged to enjoy his friendship.

The Annual General Meeting of the Royal Meteorological Society will be held at 25 Great George Street, Westminster, on Wednesday, the 18th instant, at 8 p.m., when the Report of the Council will be read, the election of officers and Council for the ensuing year will take place, and Mr. W. Ellis, the President, will deliver his address.

On Tuesday next (January 17), Mr. George J. Romanes will begin at the Royal Institution a course of ten lectures, being the first part of a course on "Before and After Darwin;" Mr. Hubert Herkomer will on Thursday (January 19), begin a course of three lectures on (1) "The Walker School," (2) "My Visits to America," and (3) "Art Education"; and Lord Rayleigh will on Saturday (January 21) begin a course of seven lectures on "Experimental Optics." The Friday evening meetings will begin on January 20, when Lord Rayleigh will give a discourse on "Diffraction of Sound."

The following are the arrangements for the Penny Science Lectures at the Royal Victoria Hall for the present month:—January 10, "The Great Sea-Serpent," by Arthur Stradling; January 17, "Caves and Cave-Men," by F. W. Rudler; January 24, "The Oldest Monuments in Brittany and Britain," by Prof. Bonney, F.R.S.; January 31, "Speech made Visible, or Picture-Writing as it was, and as it is now," by Prof. Ramsay.

Lectures will be delivered in Gresham College, Basinghall Street, E.C., on January 17, 18, 19, and 20, at 6 p.m., by Dr. E. Symes Thompson, on "Sleep, Sleeplessness, and Pain."

In reference to the review in these columns last week (p. 218) of the second series of collected papers on Indo-China, we observe from the last Annual Report of the Council of the Straits Branch, Royal Asiatic Society, that there is at present no intention of proceeding further with the publication of selected papers on the East Indian Archipelago. The Council, however, expresses a hope that, at some future time, an effort will be made by the Society to translate and publish selected papers which have appeared in the Journals of Societies in Holland and Java, written by learned Dutch Orientalists. The Report adds that the new map of the peninsula, to which we have several times referred, was finished in 1886, but before it could be sent to England the Siamese Government gave further geographical information concerning the northern part of the peninsula, and the map will not be published till this new information is incorporated in it.

In the November Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, attention was drawn to the subject of fruit-growing in British colonies, and an admirable report on the fruits of Canada was given. The treatment of the subject is continued in the January Bulletin, which contains full reports sent by the Governments of Victoria, South Australia, Western Australia, Tasmania, New Zealand, Cape Colony, and Mauritius. Prominence is given to the quantity of fruit actually available for export in each colony. To this the writers add the months during which the fruit is in season, and the prices usually paid for it locally. It was intended to publish the reports from the Australian colonies, Tasmania, New Zealand, and the Cape of Good Hope in one series, so as to present a general review of the fruit industries of the Southern Hemisphere; and this was to have been followed by reports dealing exclusively with the fruits of tropical colonies. So far, however, reports from New South Wales and Queensland have not been received.

An interesting paper, by Mr. Daniel Morris, on the use of certain plants as alexipharmics, or snake-bite antidotes, has just been issued. Mr. Morris explains that his enumeration of the plants reputed to possess alexipharmic properties is offered without any expression of opinion as to their value. It is intended chiefly as an attempt to bring together for the first time a summary of information about these plants, in order that inquiry may be made to confirm or refute the popular opinion respecting them. "Opportunities," says Mr. Morris, "to test the action of these plants on a person actually bitten by a well-known poisonous snake are seldom offered to a competent investigator. But as material is being brought together which can be carefully tested by chemical and therapeutical investigations, the most prominent of these plants, such as species of *Aristolochia* and *Mikania*, deserve very careful attention."

A valuable paper, by Prof. Marshall Ward, on the tubercular swellings on the roots of *Vicia Faba*, has just been printed in the Philosophical Transactions of the Royal Society.

Messrs. Swan Sonnenschein and Co. will publish in a few days a new work by Mr. Theodore Wood, entitled "The Farmer's Friends and Foes." The book describes in considerable detail the nature and habits of those animals, birds, and insects which exercise a good or evil influence upon the products of British agriculture, and is profusely illustrated.

We have received the "Annuaire," for 1888, of L'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. It contains full information as to the organization, rules, and work of the Academy; and there are several rather elaborate memoirs of late members. Each of these memoirs is accompanied by a carefully-engraved portrait.

Capt. M. Rykatschew, the Assistant Director of the Central Physical Observatory at St. Petersburg, has published in the *Repertorium für Meteorologie* (Bd. xi. No. 2) a discussion of the winds and pressure of the Caspian Sea. The title is misleading, as the observations are made on land, ten stations being on the shores of the Caspian Sea, and nine in neighbouring districts. And the mean winds are deduced by Lambert's formula, which deals with the number of observations without reference to the force of the wind. Nevertheless the work is a valuable and elaborate discussion, based on trustworthy observations extending from three to forty-four years.

The mean wind-frequency and pressure are given for every month, for seasons, and for the year, and charts are drawn for the seasons and for the year. The work is a continuation of previous similar discussions of the winds of the Baltic, the White and Black Seas, and the Sea of Azov.

IN the *Archives des Sciences Physiques et Naturelles* for December 15 last, M. P. Plantamour publishes the results of observations of the periodic movements of the ground from October 1, 1886, to September 30, 1887, as shown by spirit-levels fixed in the exterior and central partition walls of his house at Sécheron, near Geneva. The oscillations are illustrated by curves, from which it is seen that the movements exhibited by the two levels are not always parallel, but vary in a regular manner, and that both curves follow the variations of temperature throughout the year. Experiments have been carried on for nine years, but a longer series is necessary to arrive at definite conclusions.

M. A. F. SUNDELL publishes in vol. xvi. of the Proceedings of the Scientific Society of Finland, the results of comparisons of the standard barometers at the principal Observatories of Europe, with the view of showing what corrections are to be applied to reduce the readings of different countries to absolute uniformity. The comparison shows the existence of considerable differences between the various standards. But as the experiments were made with an instrument which was filled with mercury at each comparison and afterwards emptied, it is a question whether the results obtained may be considered perfectly trustworthy.

A NEW compound of arsenic, containing that somewhat remarkable substance, hexiodide of sulphur, has recently been prepared by Dr. Schneider, of Berlin (*Journ. für prakt. Chemie*, No. 22). Hexiodide of sulphur, SI_6, was prepared some time ago by Landolt by evaporation of a solution of iodine and sulphur in carbon bisulphide at a low temperature: it forms pyramidal crystals, shown by Von Rath to belong to the rhombic system, and, curiously, is isomorphous with iodine itself. It is a compound of considerable theoretical interest, inasmuch as it is the only known instance in which the supposed six combining bonds or affinities of sulphur are satisfied by monad atoms. One would naturally imagine that such a compound would be eminently saturated, and it has never hitherto been known to effect any further combinations; but Dr. Schneider now shows that it is capable of forming a crystalline double compound with arsenious iodide, of the composition $2AsI_3 . SI_6$. This new compound was first incidentally obtained during a lengthy research upon the relations between arsenious sulphide and iodine, and its discovery forms another example of the happy manner in which important results are often most unexpectedly attained by following the by-paths which so frequently lead off from the high-way of systematic research. It may, however, be synthetically prepared by gently warming a mixture of SI_6 and AsI_3 in the proportion of one molecule of the former to two of the latter: the two substances melt together to a deep-brown liquid, which, on cooling, solidifies to a dark gray crystalline mass. The crystals are homogeneous, tolerably hard and brittle, yielding a reddish-brown powder on pulverization; they cannot be preserved in the air, as they lose all their iodine in twenty-four hours, but in sealed tubes may be kept any length of time. The double compound itself, however, is nothing near so interesting as the important theoretical questions which it suggests. We may well ask, Is it possible that the atoms of sulphur are still endowed with a certain amount of combining energy after their six "affinities" are satisfied? or do the iodine atoms act in this case in one of their higher capacities?

AT the last meeting of the Geographical Society of St. Petersburg, M. Kuesenoff gave an account of an interesting nomad tribe in the Ural Mountains, calling themselves Vagueles. In the winter they dwell in wooden huts, and in the summer wander among the mountains, living in tents. At the former season their clothing consists of deerskin, and at the latter of linen garments. They worship the sun and some of the stars, and have a superstitious dread of certain forests, which they deem sacred. Women hold a very inferior position, being treated as slaves. During the last few years contact with more civilized tribes has had a good influence on the Vagueles, and some of them have begun to settle down as tillers of the soil. The tribe is said to be of Finnish origin.

ON December 10, about 6 p.m., a meteor was seen at Hönefos, in Norway. It went in a north-easterly direction, emitting a brilliant bluish-white light, and lasted a few seconds.

ON December 18, about 8 p.m., a magnificent meteor was seen in several parts of the province of Stockholm, going in a direction north-west to south-east. It shone with a bluish light. It left a broad trail in the sky, and eventually burst into tiny fragments, but without any report.

WHAT is believed to be a meteorite has just been dug out of the ferry harbour of Nökjobing, in Denmark. The stone, which weighs about half a ton, was found in soft mud, and no other stones were near it. It is very dark in colour, contains iron, and is of unusual weight for its size, the work of moving it being very laborious. It has now been blasted to pieces, which will be examined scientifically.

LAST year a Phanerogam hitherto never met with in Scandinavia (*Juncus tenuis*, Willd.) was found near Vexio, in Central Sweden. In Europe this plant is found only in a few localities in Germany, Holland, and Scotland.

DURING last autumn, in October and November, ornithologists in the province of Tromsö, in the extreme north of Norway, were interested by the sudden appearance of large flocks of the so called "Nut-crow" (*Nucifraga caryocatactes*), a bird hitherto never seen in Northern Norway, and which is scarce even in the southern part of the country. Several specimens were shot and forwarded to the Tromsö Museum. It is surmised that the birds were driven thus far north, during migration, by stormy weather.

OWING to unfavourable weather, the cultivation of oysters on the coast of Norway was not so successful last year as in previous years.

LAST autumn an attempt was made to bring live cod from Iceland to Norway on board smacks, and 6000 fish were brought over to Bergen successfully. Here, however, many of them died, on account of the basin in which they were kept until the sale could be effected being too small. This year fresh attempts will be made.

THE temptation of French architects seems to be to attend to the decorative rather than the useful parts of the buildings they design. The architect who designed the new Medical School in Paris took so little pains about the distribution of the water-pipes, that in very cold weather the laboratories (chemistry, physiology, bacteriology, experimental pathology, &c.) are wholly deprived of water. Last week the water in all the pipes was frozen, so that not a drop of water was available in a single laboratory. Of course, everyone connected with the school complains that work under such conditions is nearly impossible.

THE new Sorbonne will be a handsome building, but, unfortunately, the work is soon to be stopped owing to lack of money. The ornamental part of the building is finished, but the useful part has not yet been begun.

THE *Ceylon Observer*, writing on the great trigonometrical survey of that island, states that its connection with the con-

tinent of India by a network of triangles is now an accomplished fact, Mr. More, District Surveyor, having in November last finished his series of observations with the large theodolite. Nothing now remains but to reduce the observations, a work which it is anticipated will take about six months. Mr. More had enormous difficulties to overcome in his survey. The north of the island is so much covered with forests that he was compelled to erect lofty stages for his theodolite, at a height of from 40 to 70 feet above the ground; and the observed signals were in many cases 140 feet from the earth. All these stages had to be made on the spot, the appliances at hand being of the poorest description, and it was with the greatest difficulty that the structures thus made were kept at the necessary rigidity. The climate is so uncertain that the surveyors often watched for days without seeing a flash from the heliostat, and at other times every member of the working parties was prostrated by fever. As the observers approached the coast, stone towers were put up instead of timber stages, and these towers will serve not only as permanent survey stations, but as landmarks for those navigating the neighbouring waters. In all, eleven stone towers were erected, and very many wooden stages. Ceylon, by the completion of this trigonometrical survey, is now free from the reproach which it has lain under since the Indian surveyors finished their portion of the work. There is now a complete chain of triangles from Asiatic Russia to the south of Ceylon. The *Observer* adds that it is curious to note that exactly one hundred years ago (1787) a complete triangular connection was formed between Great Britain and France across the Channel under the superintendence of General Roy, R.E.

THE additions to the Zoological Society's Gardens during the past week include a Burrowing Owl (*Speotybo cunicularia*) from South America, presented by the Rev. Basil Wilberforce; a Vulpine Phalanger (*Phalangista vulpina* ♀) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

O'GYALLA SPECTROSCOPIC CATALOGUE.—The systematic survey with the spectroscope, undertaken for the northern heavens several years ago, by Prof. Vogel and Dr. Dunér, the former examining the region from Decl. 1° S. to Decl. 40° N., and the latter that from Decl. 40° N. up to the Pole, has been now carried some considerable distance into the southern hemisphere by Dr. N. de Konkoly and his assistant, Dr. Kövesligethy; and the second part of the eighth volume of the O'Gyalla observations, which has recently appeared, contains a spectroscopic catalogue of the stars down to mag. 7·5, lying between Decl. 15° S. and the equator. The work was commenced in August 1883, and was completed in August 1886, 2797 spectra having been observed on ninety nights. A number of these were observed on more than one night, so that the resulting catalogue contains only 2022 stars. Vogel's arrangement of types was followed, so that the present catalogue is on the same lines as those of Vogel and Dunér. The annexed table gives the number of stars ranged under each type.

I.*a.*	I.*b.*	I *b*?	I.*c*?	II *a.*	II.*b.*	III.*a*	III.*b.*
990	4	12	1	865	2	87	3

Continuous.	Monochromatic.	?
41	3	14

The three monochromatic spectra indicate the presence of minute planetary nebulæ. There was only one star spectrum suspected of showing a bright line, a star of mag. 6·5 about 50′ n of ζ Orionis. This latter star, together with β, δ, and ϵ of the same constellation, Dr. Konkoly finds to be variable as to its spectrum. It is to be hoped that the details of the observations upon which so important a statement is based will be published. And it is also to be desired that the work which has been carried so far may now be taken up by some southern observer, and the remaining portion of the heavens surveyed. It is to such works as the present, and the similar labours of Vogel and Dunér, that we must look for evidence of such physical changes amongst the stars as Dr. Konkoly would seem to predicate of the principal stars of Orion.

ASTRONOMICAL PRIZES OF THE PARIS ACADEMY OF SCIENCES.—The Lalande Prize of the Academy has been decreed to M. Dunér for his micrometric measures of double stars, and for his researches on spectra of the third type. M. Périgaud, of the Observatory of Paris, receives the Valz Prize for his important astronomical labours. Amongst those specially mentioned are his determinations of the division errors of four of the circles, and of the absolute flexure of the two principal meridian instruments of the Paris Observatory. The Janssen Prize for important progress in physical astronomy—in the recent sense of the term—awarded this year for the first time, was most appropriately assigned to the late Prof. Kirchhoff. Amongst the general prizes of the Academy should be noted the Arago Medal decreed to M. Bischoffsheim for his great and generous aid to science, and especially for his magnificent foundation of the Nice Observatory. This prize also is now given for the first time. The La Caze Physical Prize is given to MM. Paul and Prosper Henry, chiefly for their great achievements in astronomical photography.

The subject for the Damoiseau Prize for 1888 is proposed in the following question: To perfect the theory of inequalities of long period caused by the planets in the movement of the moon; to see if they exist sensibly beyond those already known.

NEW OBSERVATORY IN VIENNA.—The observatory of Herr M. von Kuffner, the erection of which was commenced in the summer of 1884, has been practically completed. The building is cruciform in shape, and is 82 feet from east to west, and 61 from north to south. The meridian instrument is by Repsold, and has an aperture of 4·9 inches, and a focal length of 5 feet; the eye-piece and object-glass are interchangeable; the circle is 21·6 inches in diameter, and is divided to 2′ and read by four microscopes. The principal equatorial is by the same maker, and has an aperture of 10·6 inches, and focal length of 12 feet 6 inches, with a finder of 2·6 inches aperture, and 26 inches focal length. The co-ordinates of the observatory are provisionally given as long. = 1h. 5m. 11·1s. east of Greenwich, and lat. = 48° 12′ 47″·2 N.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JANUARY* 15–21.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 15

Sun rises, 8h. 2m.; souths, 12h. 9m. 34·3s.; sets, 16h. 17m.: right asc. on meridian, 19h. 47·1m.; decl. 21° 10′ S. Sidereal Time at Sunset, 23h. 55m.

Moon (at First Quarter on January 21, 5h.) rises, 9h. 18m.; souths, 14h. 1m.; sets, 18h. 51m.: right asc. on meridian, 21h. 39·3m.; decl. 15° 14′ S.

Planet.	Rises. h. m.		Souths. h. m.		Sets. h. m.		Right asc. and declination on meridian. h. m.		° ′
Mercury..	8 9	...	12 2	..	15 55	...	19 39·4	...	23 26 S.
Venus ...	4 52	...	9 7	...	13 22	...	16 44·5	...	20 0 S.
Mars ...	0 0	...	5 36	...	11 12	...	13 12·8	...	5 17 S.
Jupiter ...	3 56	...	8 16	...	12 36	...	15 52·5	...	19 19 S.
Saturn ...	16 58*	...	0 49	...	8 40	...	8 24·9	...	19 47 N.
Uranus ...	23 56*	...	5 28	...	11 0	...	13 4·5	...	6 9 S.
Neptune..	12 23	...	20 3	...	3 43*	...	3 42·1	...	17 55 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Jan	h.	
18 ...	20 ...	Mercury in superior conjunction with the Sun.

Meteor-Showers.

	R.A.	Decl.	
	°	°	
Near π_1 Orionis... ...	72 ...	5 N. ...	January 15–20.
From Canes Venatici.	180 ...	35 N. ...	Swift; streaks.
Near θ Aurigæ	295 ...	53 N. ...	January 14–17.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
T Cassiopeiæ ...	0 17·2 ...	55 10 N. ...	Jan. 19,		*M*
U Cephei	0 52·4 ...	81 16 N. ...	,, 15,	22 1	*m*
			,, 20,	21 41	*m*
Algol	3 0·9 ...	40 31 N. ...	,, 18,	2 59	*m*
			,, 20,	23 48	*m*
V Tauri	4 45·6 ...	17 21 N. ...	,, 15,		*M*
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	,, 19,	7 0	*m*
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,, 19,	21 45	*m*
			,, 21,	1 1	*m*
T Canis Minoris...	7 27·8 ...	11 59 N. ...	,, 19,		*M*
W Virginis	13 20·3 ...	2 48 S. ...	,, 17,	3 0	*M*
δ Libræ	14 55·0 ...	8 4 S. .	,, 18,	4 8	*m*
W Scorpii	16 5·2 ...	19 51 S. ...	,, 17,		*M*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 15,	17 47	*m*
		and at intervals of		20 8	
β Lyræ...	18 46·0 ...	33 14 N. ...	Jan. 16,	6 0	*M*
R Lyræ	18 51·9 ...	43 48 N. ...	,, 16,		*M*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	,, 19,	21 0	*M*
Y Cygni	20 47·6 ...	34 14 N. ...	,, 16,	20 54	*m*
			,, 19,	20 47	*m*
δ Cephei	22 25·1 ...	57 51 N. ...	,, 17,	6 0	*m*
			,, 18,	21 0	*M*

M signifies maximum; *m* minimum.

DUNÉR ON STARS WITH SPECTRA OF CLASS III.[1]

II.

A SERIES of observations such as ours ought to add at least a little to our knowledge of the development by which the spectra of stars pass from the second class to one of the two sections of the third, especially if these observations are combined with those made of the stars of the two first classes generally, and of our sun in particular; we might even draw conclusions as to the successive development of stars after they have already reached this class. He who sees trees in a forest in different stages of development, some old, some young, some decaying, can at once form an idea of the different stages undergone by each: it is just the same with the observer of the different classes of stellar spectra.

The spectra of the first class are characterized by the almost total absence of all metallic lines excepting those of hydrogen. In spite of that, we cannot doubt for a moment the presence of metallic gases in their atmospheres, for even in the spectrum of Vega we can faintly distinguish the principal rays of sodium, magnesium, and iron. But these gases are probably at such a high temperature that their power of absorption is very slight. But as the star cools and the spectrum approaches the second class, the metallic lines become stronger and more numerous, whilst, strange to say, the lines of hydrogen diminish. Thus the spectrum becomes more and more like that of our sun in its actual state, and at length, as the metallic lines increase, it resembles that of Arcturus.

Up to this stage of development it is unnecessary to consider the two divisions of the third class separately, but after this it becomes indispensable.

In those spectra which at length become III.*a*, the change seems to operate as follows. On account, probably, of the progressive cooling, the metallic lines, especially those of iron, magnesium, calcium, and sodium, become larger, and, besides these, numerous weak narrow lines are seen grouped together, generally in the neighbourhood of the stronger lines. At this stage it is often difficult, if not impossible, to decide, with spectroscopes of small dispersion, whether one sees broad lines or real bands (or flutings). This happens in the spectrum of Aldebaran. The faint lines go on accumulating, until they cannot be separated from one another and occupy broader spaces, and now the spectrum is easily seen to belong to Class III.*a*. At first the bands in the red and orange are the only ones distinctly visible; but later the bands in the green-blue and in the blue become very strong and broad.

While the development of the stars III.*a* was very well known before my researches, former observers have known no star with a spectrum intermediate between II.*a* and III.*b*. Thus, M. Pechülé declares the hypothesis of the co-ordination of the III.*a* and III.*b* classes to be inadmissible. On the other hand, he seems disposed to think that the spectra III.*b* represent a phase, perhaps the last before its total extinction, in the development of each star, and that the passage from type III.*a* to III.*b* takes place suddenly or by a catastrophe, during which the bright lines appear ("Expedition Danoise," pp. 22-25). M. Pechülé seems, however, to consider this hypothesis doubtful, and at length declares that the physical *rôle* of the stars III.*b* is still quite a mystery.

A very simple explanation clears up at least part of this mystery. If the hypothesis which I, in full agreement with M. Vogel, have suggested be correct, the stars intermediate between the second and third classes must necessarily be comparatively rare, considering that this is only a transitory phase of their existence. The general spectroscopic observations of M. Vogel affirm this fact, for amongst the numerous stars examined by him there are only forty-eight whose spectra are denoted by II.*a* ! ! ! II.*a* ! ! or II.*a* ! But as the lines must be very distinctly visible in the spectra of the stars which are on the point of passing from the second class to Class III.*a*, we are obliged to acknowledge that almost all the stars of this category within the zone examined by M. Vogel are among these forty-eight objects. At first sight one might be disposed to seek these stars among those whose spectra are designated by M. Vogel by II.*a* (III.*a*), II.*a*? III.*a*, and III.*a* (II.*a*); but a closer examination shows that although it is not impossible that these spectra may be among these objects, they must be so rare that that is of no essential consequence as regards the question which occupies us.

Amongst these stars there are none which attain the magnitude 4·5, and only fourteen which surpass the magnitude 6·4. All the others are faint objects, and the ambiguous symbols show the difficulty M. Vogel found in recognizing with certainty the details in the spectra, and not that he could not decide with certainty to which of the two contiguous classes a spectrum of which he could easily perceive the details belongs. The correctness of this supposition is, however, proved by the circumstance that certain spectra are designated by III.*a* (III.*b*), or III.*a*? III.*b*. And none will believe that M. Vogel meant to imply that these spectra were in the act of passing from one section of the third class to the other. Besides, one of these stars is R Serpentis, whose spectrum when the star is at the maximum is one of the most strongly marked of III.*a*, according to M. Vogel's earlier researches, and according to mine. But in his general spectroscopic review M. Vogel examined it when its magnitude was only 9·0, and therefore it was easy to doubt, on account of the excessive width of the bands, whether the spectrum might not be III.*b* instead of III.*a*.

Consequently, although I think I am right in admitting that most of these stars belong to the pure type II.*a* or III.*a*, I will nevertheless suppose that a third of them really have spectra intermediate between II.*a* and III.*a*. Their number in M. Vogel's catalogue is 120, and the third is 40, so we should have therefore between the Pole and $-25°$ declination 160 spectra intermediate between II.*a* and III.*a*. I found also by special observations that among the spectra designated by II.*a* ! ! ! II.*a* ! ! and II.*a* ! a fourth part really belong to the intermediate type. Thus there would be in all 200 such spectra, a number evidently much too great. Then, the spectra III.*b* being about fifty times rarer, we should have at most four spectra intermediate between II.*a* and III.*b*, and if only stars of a higher magnitude than 6·0 are reckoned, there would scarcely be one.

But, if we consider the differences between the spectra III.*a* and III.*b*, we shall find that in reality we can scarcely expect to find any spectrum intermediate between II.*a* and III.*b*. As we have seen above, the spectra III.*a* are formed by the exaggeration of the essential characteristics of the spectra II.*a*. There must then be a phase, especially if the star is not very bright, in which one cannot decide to which of the two classes the spectrum belongs. Thus in the spectra III.*b* there are undoubtedly well-marked Fraunhöfer lines—for instance, D, and the narrow band 8, which is probably nothing but the collection of strong lines in the neighbourhood of E, and the very narrow band 5 ($\lambda = 576\mu$) which is almost like a broad line; but all these details are only secondary. The essential characteristics are the three nebulous, very broad flutings, which owe their origin to some carbon compound. If these bands are visible, the spectrum is called III.*b*; if they are not, it is called II.*a*. The only forms intermediate between the spectra of the type of Aldebaran and the normal

[1] Continued from p. 236.

type III.*b* are those in which the bands are more or less faint, or even scarcely perceptible. In fact, I have proved not only that there are spectra in which the principal bands, and especially band 6, are weak on account of the brightness of the stars, but I have found a spectrum which is scarcely a spectrum III.*b* yet, but in which the characteristics of this class are undoubtedly present.

This star is DM. + 38° 3957 = 541 Birm. In its spectrum (Planché, Fig. 6) I have seen a rather broad and well-marked band, whose approximate wave-length is 519μ, and the spectrum terminates abruptly at 475μ. These wave-lengths are, within the limits of probable errors, the same as those of the less refrangible ends of bands 9 and 10 in the spectra III.*b*. Once I thought I perceived a very faint trace of light beyond 475μ, and in the best atmospheric conditions I caught a glimpse of faint traces of the bands 4 and 6. Unfortunately the star is only of the eighth magnitude, so that only few details of its spectrum can be seen with a telescope like ours. Nevertheless, what I did see seems to me of some importance in explaining the development of a spectrum II.*a* into III.*b*.

If this spectrum be compared with those of other stars of the same or even of a lower magnitude, such as 145 Schj. DM. + 34° 56, DM. + 36° 3168, it is at once seen that in the former the principal bands are still in a very low stage of development, and if the bands had only been a little paler nothing unusual would have been seen in the spectrum under ordinary atmospheric conditions. The aspect of this star seems to prove what I said above, that there is, properly speaking, no intermediate state between the spectra II.*a* and III.*b*, but that the passage from one to the other is already accomplished before the first traces have been perceived.

But there is still one more circumstance deserving of attention, which may perhaps lead to the knowledge of other spectra which are still nearer to the critical point; that is, the very strong absorption of the more refrangible rays, which makes the whole spectrum very short, and gives to the star itself its bright orange colour. We know that there are many stars of a deep colour and with short spectra, but otherwise not striking; they ought to be examined from time to time with very powerful microscopes, for amongst these will be found, I believe, the new spectra III.*b*.

There are other spectra, which, although they undoubtedly belong to Class III.*b*, have not, it appears, reached their full development. The least faint of these stars is that known as 7 Schj. Before my researches, nothing had been published regarding this spectrum except this short remark of D'Arrest, "Irregular spectrum, probably type IV." (*Vierteljahrschrift der Astr. Ges.* ix. Jahrg. p. 255). This spectrum presents the characteristics of III.*b* very pronounced; only band 5 is invisible, and band 6 is so faint that at first sight the spectrum has not the aspect characteristic of well-developed spectra of this class. It is for this reason that D'Arrest would say nothing positive regarding this star. If the spectrum of 541 Birm. represents the first step in the passage of a star to Class III.*b*, this star doubtless represents the second step. Band 6 is the least developed of the three principal ones. Although the spectrum of this star is pretty bright, band 5 is not visible, whilst band 4 is well visible, and is also perceived in the spectrum of 541 Birm.

In the spectrum of 19 Piscium (Fig. 4 on the map), which is one of the most magnificent in other respects, band 6 is still considerably fainter than the other two principal bands, whilst in that of 152 Schj. (Fig. 3) it is quite as pronounced as band 10, and almost as pronounced as band 9. This last spectrum is in an advanced stage of development; but in spite of that, band 4 is not stronger than in the spectrum of 7 Schj., and rather fainter than that of 19 Piscium. The same relation is repeated in other spectra of this class, so that sometimes band 4 is very visible in an otherwise less developed spectrum, but invisible in more strongly marked spectra, and in the spectra of brighter stars of this class there are in the same way very faint bands, 7 and 8. But band 4 is in itself very pale; it is the deep sodium line which makes it remarkable, and the bands 7 and 8 are probably only groups of Fraunhöfer lines.

It is therefore very probable that the more or less easy visibility of these bands is no indication as to the phase of development in which the star is. There is, on the contrary, reason to believe that the strengthening of these lines, and also of the other principal lines of the spectrum (except those of hydrogen, which grow fainter during the passage of a star to Class III.) is a process of relatively small importance which goes on whilst the star still undoubtedly belongs to Class II.*a*; and even when this is accomplished there is still nothing to show whether the star will become III.*a* or III.*b*, unless perhaps in those which tend towards Class III.*a*, the line, or rather group of lines, with wave-length 616, is very well marked, which seems not to take place in the spectra III.*b*. But in the stars which tend towards the latter class the violet rays are already very much absorbed, and the stars are therefore of a deep orange.

If we pass on to consider the ulterior development of the star, it is evident that as it cools further it at length reaches a temperature at which the carbon which must be present in abundance, either in its atmosphere or under some form in its photosphere, can combine with hydrogen or some other element to give the so-called hydrocarbon spectrum. After that, the spectrum appears cut by a broad faint band with the wave-length 516μ, and by another still paler at 473μ, and the parts of the spectrum beyond this are very faint. But gradually these two bands increase in intensity, and at the same time the band 563μ is perceived, at first very faintly, and gradually becoming stronger. At this stage the narrow band 576μ is developed, and finally the three principal bands are nearly of equal intensity, and the spectrum shows all the characteristic details. It would be useless to attempt to discuss the moment at which the secondary bands in the red and orange make their appearance, as no facts on the subject are known.

It is doubtless very remarkable that in the spectra III.*b* no trace of the carbon band with the wave-length 618·7μ is seen, which is so brilliant in Plücker's tubes containing hydrocarbon. This is, however, in perfect analogy with what is seen in the spectra of comets, which owe their appearance to the same carbon compound as the stellar spectra III.*b*, and there are analogies also for the other bands. Thus the band 563μ is often very weak even in the bright comets, and the band in the green is always the strongest both in comets and stars. The band in the blue is sometimes pretty faint in cometary spectra, whilst in the stars it is only a little fainter than the band in the green; but we must remember that it is situated in a very faint part in the spectra of the stars. It is therefore very possible that a little dimness should render the remaining light entirely imperceptible. In this perhaps there is no diversity between comets and these stars. The violet bands are very faint in Plücker's tubes, but strong in the flame of alcohol. A trace of them has been seen in the spectra of the brightest comets. In very brilliant, not too red stars III.*b*, there is also a violet zone, terminating at the wave-length 430μ, of which there is a band at the position of the first and the second of these bands in the spectra of these stars.

We will now pass on to consider the changes which take place in stars of Class III. after their spectra have completely developed. As the cooling goes on, they necessarily grow dimmer and dimmer, and at length become extinct. Either the bands in their spectra must increase in width until at last the shining intervals disappear, or else, the bands keeping their same width, the whole spectrum grows fainter. Certainly we see that there are stars whose bands are enormously broad, but none the breadth of whose bands surpasses that of the bright zones.

I think, therefore, we can hardly accept the first hypothesis, but there are reasons which give very valuable support to the second. We know that the weakness of the light in the solar spots is, in the first place, caused by a general obscuration of the spectrum, and that the enlargement of the Fraunhöfer lines has very little to do with it. Besides, I have examined, on different occasions, between the maximum and the minimum, the spectra of several variable stars of Class III., and found that there was no widening of the bands sufficient to explain the weakening of the stars. There is no doubt a remarkable analogy between the spectra of the sunspots and those of the stars of Class III., and one which we have no cause to be surprised at. For, on account of the relatively low temperature of these stars, it is very probable that their surfaces are in great part covered with formations similar to our sunspots, and the absorption-bands found in their spectra are no argument against this analogy. They prove only that chemical compounds may be formed and maintained in the atmospheres of these stars, which is not possible in our sun, not even in the masses of relatively low temperature of which the spots consist.

Before laying down my pen I must remark that the induction by which I arrived at these conclusions does not prove that the spectrum of each star commences with Class I. and finishes with

Class III. The development might just as well be in inverse order, though we have important reasons for believing it is not so.

The astronomy of the future must decide between these two alternatives. My object in undertaking this work was to facilitate this decision by giving as exact descriptions as possible of the spectra presented by the different stars of Class III. in the year 1880.

THE ART OF COMPUTATION FOR THE PURPOSES OF SCIENCE.[1]

II.

SOME few problems in astronomy and certain theories in pure mathematics require more than seven figures to be calculated. In these cases a large arithmometer is generally the most convenient. Ten-figure tables of logarithms may be obtained second-hand; or the required logarithms must be calculated.

The tables of Vlacq, re-edited by Vega in 1749, 1794, and 1797 are somewhat difficult to obtain and cumbrous to use. The logarithms of numbers up to 101,000 are given to ten figures with first and second differences. Thus to find log 10 542 482 375, from the table directly

$$
\begin{array}{llll}
\log 10\,542 = & \cdot0229\,230\,119 & \Delta_1 = & 411\,946 \\
 & \quad\;\; 198\,712\,3 & & 482\,375 \\
 & \qquad\qquad 5 & & \\
 & & & 1\,647\,784 \\
\text{log required} & \cdot0229\,428\,836\,3 & & 329\,557\,+ \\
 & & & 8\,239\,+ \\
\text{The true log of } 10\,542\,482\,375 \text{ is} & & & 1\,236\,+ \\
\quad\cdot022\,942\,883\,626\,562. & & & 288\,+ \\
 & & & 20\,- \\
 & & & 198\,712\,3\,1 \text{ subtracted.}
\end{array}
$$

$$\Delta_2 = 40$$

$$\frac{\cdot48(\cdot48 - 1)(-40)}{2} = 4\cdot992.$$

In default of Vega, or if more places are required, the logarithm must be calculated, and this is by no means such a serious affair as one is led to think by the ordinary books on algebra. I am much indebted in what follows to the article by Mr. J. W. L. Glaisher on logarithms in the new edition of the "Encyclopædia Britannica," to which I refer my readers for further particulars in theory, restricting myself to practical details.

The easiest way to calculate a table of logarithms absolutely *de novo* would be by the method of differences, with some mechanical assistance, such as the difference-engine of Babbage or of Scheutz. It seems unlikely that larger tables will be calculated than those already in existence, since the cost increases with great rapidity. Mr. Sang has, however, recently calculated independently the logarithms of numbers from 100000 to 200000, where the ordinary tables are weakest.

Briggs used at least two methods for the calculation of logarithms which depended upon the extraction of a succession of roots. For instance, by taking the square root of 10 fifty-four times he found log $1\cdot(0)^{15}1\ 278\ 191\ 493$ to be $\cdot(0)^{15}0\ 555\ 111\ 512$. Whence assuming that very small numbers vary as their logarithms, log $1\cdot(0)^{15}1 = 555\ 111\ 512/1\ 278\ 191\ 493$, or log $1\cdot(0)^{15}1 = 0\cdot43\ 429\ 448 = \text{M}$, the modulus. And if x be small, log $1\cdot(0)^{15}x = x \times 0\cdot43\ 429\ 448$. To find log 2 he extracted the square root of the tenth power, 1024/1000 forty-seven times, and found $1\cdot(0)^{15}1\ 685\ 160\ 570$, which multiplied by M gave $\cdot(0)^{15}0\ 731\ 855\ 936$. This multiplied by 2^{47} gave log $1\cdot024$; adding 3 and dividing by 10 gives log 2. Another more simple method was to find a series of geometrical means between two numbers, such as 10 and 1, the logarithms of which are known. After taking 22 of these roots, log 5 is found to be 0·69897.

It was soon found that logarithms could be more easily calculated by the summation of various series, and many great mathematicians, such as Newton, Gregory, Halley, Cotes, exercised their ingenuity in discovering those most suitable for the purpose.

Though for practical purposes the use of series has been almost superseded, three very simple ones are still occasionally useful:—

$$\log(1 \pm x) = \text{M}\left(\pm x - \frac{x^2}{2} \pm \frac{x^3}{3} - \frac{x^4}{4} \pm \frac{x^5}{5} - \right)$$

which converges rapidly if x be small. M is a number depending upon the system of logarithms adopted, and constant for each system. If M be 1, the system is called the Naperian, or natural one; and if M = 0·434 &c., the system is the common one. Unless otherwise stated M will be assumed to be 1, or the logarithms will be natural ones.

Thus to calculate $\log 1\cdot1 = 1 + \frac{1}{10}$, omitting M:—

$$\log 1\cdot1 = \frac{1}{10} - \frac{1}{200} + \frac{1}{3000} - \frac{1}{40000} + \frac{1}{500000} -, \text{\&c.},$$
$$= 0\cdot1003\ 3534 - 0\cdot0050\ 2517 = 0\cdot0953\ 1017.$$

Suppose x be small, $\log(1 \pm x) = \pm \text{M}x$ nearly. Thus if $\log 1\cdot(0)^{10}9$ be required to twenty decimals, it is

$$\cdot(0)^{10}9 - \frac{1}{2}(\cdot9 \times 10^{-10})^2,$$

or the error caused by omitting this and all subsequent terms is only 4 in the twenty-first decimal place. Using common logarithms the multiplication by M reduces the error by one-half. This result is of great importance in calculating logarithms by Flower's method, since the factors which have to be dealt with are only half the number of decimal places in the required logarithm.

Writing $\frac{1}{x}$ for x in the above series, we obtain—

$$\log(1 + x) - \log x = \text{M}\left(\frac{1}{x} - \frac{1}{2x^2} + \frac{1}{3x^3} - \frac{1}{4x^4} + \frac{1}{5x^5} - \right)$$

which converges rapidly when x is large. Various artifices may be used to render x large, even when the number the logarithm of which is required is small. Thus, Prof. J. C. Adams has calculated (NATURE, vol. xxxv. p. 381) log 2, log 3, log 5, log 7, $\frac{1}{\text{M}}$ and M to 270 places of decimals.

Another very valuable series is—

$$\log(a \pm x) = \log a \pm 2\text{M}\left\{\frac{x}{2a+x} + \frac{1}{3}\left(\frac{x}{2a+x}\right)^3 + \frac{1}{5}\left(\frac{x}{2a+x}\right)^5 + \text{\&c.}\right\}$$

Thus, supposing log 219 known, to calculate log 2198:—

$$\log 2198 = 7\cdot6916\ 5682\ 2810 + 2\left\{\frac{4}{2194} + \frac{1}{3}\left(\frac{4}{2194}\right)^3 + \text{\&c.}\right\}$$

$$
\begin{array}{ll}
\quad\cdot0036\ 4630\ 8113 & \dfrac{4}{2194} = \cdot0018\ 2315\ 40565 \\
\qquad\qquad\quad 4039 & \\
\log 2198 = 7\cdot6593\ 0313\ 4962 & \dfrac{2}{3}\left(\dfrac{1823}{10^6}\right)^3 = \cdot0^8 4039
\end{array}
$$

Using common logarithms, the third term of the series is $< \frac{1}{27\cdot6}\left(\frac{x}{a}\right)^3$, that is less than 5 in the ninth place when $\frac{x}{a} < \frac{1}{200}$. Hence, with a table giving the logarithms of 100–1000 to eight figures the third term may be neglected, or the required difference is $\pm \frac{2\text{M}x}{2a + x}$, or, writing $\log(a + x) - \log a = y$, $x = \frac{2ay}{2\text{M} - y}$.

The given numbers may also be broken up into factors by the aid of such a table as Burkhard's, which gives the factors of all numbers up to 3,036,000. The logarithms of the factors may then be found from tables and added together. Of all tables for this purpose, that of Wolfram is the most valuable; it gives the natural logarithms to forty-eight places of *all* numbers up to 2200, and of all which are not easily divisible up to 10,009.

The multiplication by M to convert into common logarithms is tedious, and it is frequently better to dispense with it in heavy calculations. If necessary, a table of the first ninety-nine multiples of M should be prepared, and Oughtred's short method of multiplication used.

If any of my readers desire to test themselves and their tables

[1] Continued from p. 239.

by a long but easy calculation, the amount of £1 laid up at 5 per cent. compound interest for a thousand years will be found not to differ very much from £1,546,318,920,731,927,238,982. An answer of this sort is of course of no practical utility whatever, but it brings vividly before us an important point in political economy—the accretion of wealth in the hands of corporations. It was computed that just before the Revolution more than half the soil of France was owned by the Church. Looking at this array of figures, and remembering that since the Church could never alienate its property all surplus income must be regarded as at compound interest, we can only wonder that it was the half and not the whole.

The first table for facilitating the computation of logarithms was one given by Long (Phil. Trans., 1724) of the decimal powers of 10 to nine figures. Thus, to find the number the logarithm of which is

$$·30103 = 10^{·3} \times 10^{·001} \times 10^{·00003} = 1·99526231 \times 1·00230523 \times 1·00006908 = 1·99999997, \text{ or } 2.$$

This method is cumbrous, but it is perhaps one of the most simple for explaining the calculation of logarithms to beginners.

A much more convenient method has been well worked out by M. Namur, but, unfortunately, only his twelve-figure table seems to be still in print. The table contains the logarithms of numbers from 433300 to 434300 to twelve figures, and the numbers corresponding to logarithms from 637780 to 638860. By the aid of certain factors which are tabulated with their complementary logarithms, any number or logarithm can be reduced between these limits.

Thus, to find log π—

	314 159 265 359	× 1·3
	94 247 779 607 7	
	408 407 044 956 7	× 1·063
	24 504 422 698 0	
	1 225 221 134 9	
	434 136 688 799 6	
log from table	637 625 800 474	Δ = 1·000364
	206 4	
	41 3	
	2 4	
	637 626 489 524	
	973 466 735 477	complementary logs of 1·3 and 1·063
	886 056 647 693	
	497 149 872 694	= log π.

The last method I shall mention is generally known by the name of Weddle; it was probably used by Briggs, and published by Flower in 1771. It consists in multiplying the given number by a series of factors of the form $1 \pm \frac{x}{10^n}$ until it is reduced to one. The complement of the sum of the logarithms of the factors is the required logarithm. The logarithms of the factors are easily calculated by the first series; they have been tabulated to about thirty places.

Thus to find log 3550·26:—

355026 × 2	30103	
710052 × 1·3	11394 3	
2130156	3342 4	
9230676 × 1·08	130 1	
738454	3 9	
9969130 × 1·003	44973 7	
29907	55026	complement.
9999037 × 1·00009		

Hence log 3550·26 = 3·55026, or we have a number which is expressed by the same figures as its logarithm.

It is the present fashion, while depreciating our own country men, to extol all Germans in matters connected with education, and especially to award them the palm for patient plodding. It will be some time before a German rivals Prof. Adams, and even then there is a height beyond. Of all monuments of calculation the value of π, or the number of times the circumference is longer than the diameter of a circle, is most astounding. Archimedes found it to be $\frac{22}{7}$, Wolf calculated it to 16 places, Van Ceulen to 35, Machin to 100, Beerens de Haan to 250, Richter to 500. But in 1853 Mr. Shanks threw all these results into the shade, and excited the admiration even of De Morgan by calculating π to 530 places, "throwing aside as an unnoticed chip the 219th power of 9"! Two printers' errors were pointed out by Mr. John Morgan, which Mr. Shanks corrected from his manuscript, and in 1873 gave a new result to 707 places. Hence the value of π is known to within $\frac{1}{3 \times 10^{707}}$, an exactness which is useless from the inability of the human mind to comprehend the figures which express it.

Clerk Maxwell proposed, possibly in irony, to take the wave-length of a certain light as the universal unit of length. Choosing for this purpose about the middle of the violet, a mile would be expressed by 60000 × 63360 = $3·8 \times 10^9$ units nearly. Suppose that Sirius, the brightest star in our firmament, has an annual parallax of $\frac{1}{5}''$, a quantity perceptible, but barely measurable, by our best telescopes, the distance of the sun from Sirius is about 5 × 206,265 × 92,300,000 miles, or $3·5 \times 10^{23}$ units. Assume again that Kant's fanciful conjecture is correct, and that the sun revolves round Sirius in a circle the length of which is expressed by $7 \times 10^{23} \times \pi$ units. Make the still greater assumption that all our measures are correct, and our arithmetic as it ought to be, so that the only possible error would be in the evaluation of π. The greatest possible error according to Mr. Shanks's determination would be $\frac{7 \times 10^{23}}{3 \times 10^{707}}$ or $\frac{1}{4·3 \times 10^{683}}$ of a wave-length of violet light. Whatever metaphysicians may say, I think we have here reached, if not surpassed, the limits of the human understanding.

SYDNEY LUPTON.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, January 2.—M. Janssen, President, in the chair.—On an objection made to the employment of electro-magnetic regulators in a system of synchronous timepieces, by M. A. Cornu. This is a reply to M. Wolf's recent communication, in which several objections were urged against the apparatus in question. It is shown (1) that such a regulator does not necessarily tend to stop the system to which it is applied; (2) that in any case the stoppage may be prevented without complication or expense; and (3) that in a public time-distributing service the stoppage should not only not be prevented, but efforts should be made to bring it about whenever the synchronizing system gets out of order. The paper was followed by some further remarks on the part of M. Wolf, who reiterated his objections, and treated M. Cornu's third point as somewhat paradoxical.—Remarks on Père Dechevrens's letter regarding the artificial reproduction of whirlwinds, by M. H. Faye. The author complains that, like other partisans of the prevailing ideas on the subject of tornadoes, typhoons, and cyclones, M. Dechevrens endeavours to suit the facts to the exploded theory of an ascending motion in the artificial reproduction of these aërial phenomena.—On the meteorite which fell at Phû-Long, Cochin China, on September 22, 1887, by M. Daubrée. In supplement to M. Delauney's communication of December 19, the author adds that this meteorite was an oligosiderite of somewhat ordinary type, closely resembling those of Tabor (Bohemia), July 3, 1753; Weston (Connecticut), December 14, 1807; Limerick, September 10, 1813; and Ohaba (Transylvania), October 10, 1817.—Remarks in connection with the presentation of the "Annuaire du Bureau des Longitudes" for 1888, the "Connaissance des Temps" and the "Extrait de la Connaissance des Temps" for 1889, by M. Faye. Amongst the fresh matter added to the "Annuaire" this year are papers by M. Janssen on the age of the stars, by Admiral Mouchez on the progress of stellar photography, and by M. d'Abbadie on his recent expedition to the East in order to determine the elements of terrestrial magnetism in Egypt, Palestine, and Syria.—Observations of Olbers' comet made at the Observatory of Nice (Gautier's 0·38 m. equatorial), by M. Charlois. These observations are for December 25, 26, and 27, after the comet was discovered on December 23, when the nucleus was of the tenth magnitude, surrounded by a bright nebulosity, and with tail from 20′ to 25′ in length.—On the total eclipse of the sun

observed on August 19, 1887, at Petrovsk, Government of Jaroslav, by M. G. M. Stanoiewitch. Owing to the extremely unfavourable atmospheric conditions the observer was unable to carry out any important part of his programme. A chief result of his observations was the conclusion that the gloom prevailing during eclipses is all the deeper the less clouded is the sky and the flatter the ground, especially on the horizon. The sky being on this occasion almost completely overcast, he was able to read the title of a pamphlet printed on a red cover at a distance of 2 metres.—On the variations of temperature of gases and vapours which preserve the same quantity of heat under different tensions, by M. Ch. Antoine. A simple means is proposed for avoiding the laborious calculations required to determine the values Θ and Θ^1 in the formula $y = 25\sqrt[3]{\Theta - \Theta^1}$ deduced from V. Regnault's experiments on atmospheric air.—On the energy needed to create a magnetic field and to magnetize iron, by M. Aimé Witz. The researches here described serve to verify Lamont's statement that the effect produced by a magnetic field on a magnet is greater when the force acts to diminish than it is when the force acts to increase the magnetizing power.—On the rapidity of transformation of metaphosphoric acid, by M. Paul Sabatier. Solutions of metaphosphoric acid are transformed spontaneously with greater or less rapidity. Berzelius and Thomsen suppose that there is at first production of pyrophosphoric acid, which is afterwards changed to orthophosphoric acid. Others, with Graham, think that there is immediate formation of tribasic orthophosphoric acid, and the author's researches tend to show that this is normally the case It is also established that the rapidity of transformation is at each instant proportional to the mass of transformable substance present in the system.—On an alloy of titanium, silicium, and aluminium, by M. Lucien Lévy. Wöhler indicated two alloys of these metals without giving their composition. The author here determines a similar alloy differing in some of its properties from those of Wöhler. He has also determined its composition, as apparently a mixture of two isomorphous bodies crystallized together with formula $TiAl_4$ and $SiAl_4$. The same preparation with zinc or magnesium substituted for aluminium yielded no results.—On some derivatives of cinchonine, by MM. E. Jungfleisch and E. Léger. The authors were able some time ago to announce that the sulphate of cinchonine being heated to 120° C. for forty-eight hours with a mixture in equal parts of sulphuric acid and water, the alkaloid changes to diverse bases, of which they have isolated the six most abundant. Here they explain the process by which they have succeeded in separating the alkalies.—On the presence of diaphragms in the aëriferous ducts of roots, by M. C. Sauvageau. The transverse diaphragms intersecting the aëriferous ducts of vascular plants have hitherto been supposed to be confined to the middle region of the bark of their various members. But the author has now determined their presence also in the root of at least one such aquatic plant, the *Hydrocharis morsus-ranæ*.

Berlin.

Physiological Society, December 16, 1887.—Prof. du Bois Reymond, President, in the chair.—Herr Meyer, from Hamburg, discussed the nature of ventriloquism, and combated the opinion, so widely spread among physiologists, that it consists in speaking while inspiring, and without the cavity of the mouth acting in any way as a resonator; on the contrary, ventriloquists speak while expiring, and do move their mouths. An extended series of laryngoscopic observations on the speaker, who has practised ventriloquism for many years, has shown that in ventriloquizing the vocal opening of the larynx is shortened as it is when producing the falsetto, and that the soft palate is pressed back and that the uvula becomes invisible. Everybody who naturally possesses a high voice can easily learn to ventriloquize. One most important factor in the deception of the listeners is the contrast between the loud, full and metallic tone in which the question is asked and the answer which immediately follows in a high and gentle falsetto. Sibilants and the high I should be as far as possible avoided. The speaker then gave a series of extremely successful examples of ventriloquism, which did not presuppose any particular training, and showed that it is never accompanied by any special action of the abdominal muscles. Prof. Gad has made some experiments on Herr Meyer, and by graphically recording the variations in pressure of the air, has shown that the curve obtained when a certain sentence is spoken in the ordinary way is in all respects identical with the one which is described when the same sentence is spoken ventriloquially. In the latter case the volume of air expired was considerably less than during normal speech; in one particular case it amounted to only 900 c.c., whereas during normal speech the volume expired was 1300 c.c. Dr. Benda expressed his idea that when ventriloquizing the Eustachian tubes are open and the cavity of the tympanum, together with the tympanic membrane, are set into simultaneous vibration. He had not been able to detect any resonance of the tympanic membrane in Herr Meyer; but he believes that this explanation of the curiously veiled tones emitted is not thereby invalidated, since they closely resemble the tones produced by speaking while yawning, in which case the Eustachian tubes are certainly open and the tympanic cavity acts as a resonator.—Dr. Benda gave a further account of his researches on the development of spermatozoa, and referred to several works which have been recently published and do not agree with the results obtained by himself. For his own part he could only confirm his earlier opinions by his later researches. In Marsupials he finds some resemblance to that which holds good in Sauropsida. In general it may be said that the very varying relationships observed in Mammalia between the parent-cell and the spermatozoa-cells which are connected with this may be looked at from one common point of view; it is only necessary to adopt for animals the differentiation of the cells of pollen-grains, observed by botanists, into vegetative or nutritive, and into generative, from which the spermatozoa then arise. These vegetative and generative cells can be made out both in the functioning and not yet active testes of embryos, the cells having extremely varying relations each to the other.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Course of Elementary Instruction in Practical Biology: T. H. Huxley and H. N. Martin; Revised Edition, extended and edited by Profs. Howes and Scott (Macmillan).—Early Christian Art in Ireland: Margaret Stokes (Chapman and Hall).—Diseases of the Dog: J. H. Steel (Longmans).—Papers of Fleeming Jenkin, 2 vols. (Longmans).—Practical Guide to Photographic and Photo-mechanical Printing: W. K. Burton (Marion).—United States Commission of Fish and Fisheries, Part 13, Report of the Commissioner for 1885 (Washington).—Mechanics and Experimental Science—Mechanics: E. Aveling (Longmans).—Astronomy for Amateurs: J. A. W. Oliver (Longmans).—Modern Theories of Chemistry: Dr. L. Meyer, translated by Profs. Bedson and Williams (Longmans).—Calendar of the University College of Wales, Aberystwith, 1887–88 (Cornish, Manchester).—The Children: How to Study Them: Dr. F. Warner (Hodgson).

CONTENTS.

THURSDAY, JANUARY 19, 1888.

THE TEACHING OF ELEMENTARY CHEMISTRY.

Elementary Chemistry. By M. M. Pattison Muir, M.A., Fellow and Prælector in Chemistry of Gonville and Caius College, and Charles Slater, M.A., M.B., formerly Scholar of St. John's College, Cambridge.

Practical Chemistry: a Course of Laboratory Work. By M. M. Pattison Muir, M.A., and Douglas Carnegie, B.A., Demonstrator of Chemistry, and formerly Scholar of Gonville and Caius College. (Cambridge, at the University Press, 1887.)

DURING the past few years numerous expressions of dissatisfaction have been more or less openly uttered by members of the younger generation of English chemical teachers, and the opinion is gaining ground that instruction in the elements of the science can no longer be imparted entirely on the stereotyped lines of practice devised to suit the requirements of a bygone generation—of a time when a science of chemistry was but beginning to exist, and the conviction had not yet been acquired that the subject *must* ultimately be reckoned as a necessary element of a liberal education. Several of the objectors have advanced their criticisms to the constructive stage, thereby rendering great service to the cause; nevertheless we believe it is the general opinion that, although each contains numerous good points, all the schemes hitherto advanced are in the main failures, and that it is impossible to accept any one as it stands. The senior author of the works now under notice has been one of the most active objectors to the good old-fashioned style of teaching, and has told us in terms somewhat vague and general it is true, but none the less plainly, what we ought to do. Even chemists recognize, however, how comparatively easy it is to preach and yet how difficult to practice, and we have therefore patiently awaited the publication of details to guide us on the tortuous and narrow path to success. These details are now before us in the two books of which the titles are given at the head of this article; "they are intended to be used together," say the authors, and "their object is to teach the elements of chemical science." What will be the verdict of, say, a jury of schoolmasters—by far the most competent judges on such a question—as regards the merits of the scheme put forward by Messrs. Muir, Carnegie and Slater? We venture to predict, and we trust, that it will be, "Impossible." In order to justify this statement we shall proceed to specify our objections to the scheme, trusting that, by so doing, some service may be rendered to a cause in which so many are now deeply interested, and which is undoubtedly of the highest importance to the community on account of the inestimable advantages to be derived from the teaching of the elements of experimental science, and especially of chemistry, in schools in a logical and systematic manner.

The issue of two companion volumes has many advantages: indeed we believe that in the future it will be thought essential to separate the instructions to a student stating what is to be done from any description or discussion of observations or inferences to be deduced from results, in order, as far as possible, to induce the habit of observing and of reasoning from observation; in no other way probably is it possible to force the student to become an independent observer and thinker, and to prevent the teaching of science from degenerating into mere cram, as is too frequently the case in schools. It appears to us, however, that in the earlier part of the "Practical Chemistry" Messrs. Muir and Carnegie do not sufficiently bear in mind their own intention, and that much of the matter would find a more fitting place in the companion volume.

In the "Practical Chemistry," we learn from the preface, "the aim has been to arrange a progressive course in which, as the experiments become more difficult, the reasoning becomes more close and accurate." But surely, in a scientific work, the reasoning should throughout be "close and accurate:" authors who make such a statement almost invite suspicion, and it is to be feared that in this case such suspicion is unfortunately not entirely unwarranted; the reasoning is indeed but rarely close, and not infrequently conspicuously absent. As a typical case, and as an illustration of the manner in which the experiments are usually set forth, Experiment 1, Chapter VI., p. 22, may be quoted:—

"Place a small piece of sodium in a little cage of wire-gauze attached to a glass rod. Fill a large test-tube with water and invert it in a small basin of water; hold the tube with one hand, and with the other bring the wire cage containing the sodium under the water, so that the gas, which at once begins to bubble through the water, passes into the tube and collects there. When the tube is full of gas, cover the mouth with the thumb, invert the tube, and bring a lighted taper to the mouth; the gas takes fire, and burns with a pale, almost non-luminous flame—the gas is hydrogen. Evaporate the water in the basin to dryness; the white solid which remains is a compound of sodium, hydrogen, and oxygen; it is called sodium hydroxide, or caustic soda. (The composition of this compound cannot be proved at present.) By the interaction of sodium and water, hydrogen and a compound of sodium with hydrogen and oxygen have been formed. Sodium is an element: *if this is taken as proved*, it follows that the hydrogen evolved as gas in the foregoing experiment, and also the hydrogen and oxygen which combined with the sodium, must have formed part of the water at the beginning of the experiment. (*Here we assume* that the material of the vessel was not chemically changed during the process.) Water therefore is a compound of hydrogen and oxygen."

What can be the educational value of an experiment thus described and discussed? That water *therefore* is a compound of hydrogen and oxygen only follows when a variety of assumptions are made. The tendency of such teaching is entirely in the wrong direction: the habit of *assuming* that such and such is the case is one which it is all-important to counteract by experimental teaching, and practical chemistry will never be of value as a rigid mental discipline unless the student be led from the beginning to demand and obtain proof of each successive link in a chain of arguments.

Again, the directions for Experiment 3, Chapter II., p. 7, are to heat copper in dried air, and to weigh the tube containing it before and after heating; the weight is found to increase, whence it follows that the metal has combined

with some other kind of matter, the most likely source of which is the air. We then read:—

"We must now *make two assumptions* which *can be, and have been, proved* by accurate experiments. We shall *assume* (1) that the air is a mixture of at least two gases called oxygen and nitrogen; (2) that water is a compound of two gases, hydrogen and oxygen. If then hydrogen is brought into contact with a heated solid substance and water is produced, it follows that oxygen must have been taken away from the heated solid by the hydrogen."

The student is therefore directed to heat the copper oxide previously obtained in a current of hydrogen, and finally to weigh the tube. The weight is the same as at the beginning of the series of experiments.

"You have therefore proved, *on the basis of certain assumptions*, that when copper is heated in air it combines with oxygen in the air to produce a new kind of matter called copper oxide; and that the weight of the copper oxide thus produced is greater than that of the copper from which it has been produced. *By experiments too difficult to be performed at present it can be proved* that the difference between these weights is the weight of the oxygen which has combined with the copper."

The effect of such teaching must be that the mind of a student with inborn intelligence, instead of having logic infused into it, will have become filled with profound contempt of chemical experiments; it is impossible that it should lead to the acquisition of precision of thought or judgment. In a properly chosen series of experiments everything should be proved; no assumption should be necessary.

"The arrangement of the course and the selection of the experiments are the outcome of the experience gained in teaching chemistry for many years" (preface). Having in mind the manifestos issued at various times by one of the authors, we naturally are led by this paragraph to expect an entirely original treatment of the subject. But, alas! we fear we may safely say that "what is true is not new, and what is new is not true"! Thus, in Chapter IV., which bears the imposing heading, "Conservation of Mass of Matter," we no longer meet with the classical candle experiment, and we confess that we little regret its banishment; but what have we in its place? An experiment in which zinc is dissolved in diluted sulphuric acid, the hydrogen being retained in a tube; and a second, in which marble is dissolved in acid, the carbon dioxide being prevented from escaping by potash solution. We venture to think that neither experiment is calculated to impress the beginner, and that the only proper demonstration in this case is by some form of combustion experiment in which there is an apparent destruction of matter; but we hold that it is far better simply to lead the student to observe that in every case of apparent disappearance a new form or forms of matter are produced, and to postpone any attempt to teach the law of the "conservation of matter" until a time when the results of the gigantic labours of men like Stas can be appreciated. Again, is a *blue crystalline solid* obtained (Experiment 3, Chapter III.), on dissolving copper in sulphuric acid and evaporating the liquid nearly, but not quite, to dryness—we presume in a water-bath, as directions have previously been given (p. 3) *always* to use a water-bath, unless otherwise directed. In Experiment 8, Chapter V., the student is directed to electrolyze water containing a little sulphuric acid, and the accompanying cut represents a basin in which tubes are inserted over electrodes connected with *two* bunsen cells; in the figure the bunsens are 7/16 of an inch in diameter, the basin is 1 inch across at the base, and the liquid column 3/16 of an inch deep. Assuming the bunsens used to be 4 inches in diameter, the basin would be 7 inches across at the base, and the liquid $1\frac{5}{16}$ inches deep; there would consequently be a fairly respectable quantity of water to electrolyze. Yet, at p. 7 of the "Elementary Chemistry" we read: "If the process is continued, the water will at last entirely disappear, and in place of it we shall have two colourless gases. This result of "experience gained in teaching chemistry for many years" is indeed remarkable; the store of energy in two bunsen cells is truly marvellous, and we had not previously realized how great is the capacity of tubes such as are figured. At p. 3 the direction is given to add sodium to water in a basin, and, when the sodium is all gone, to place the basin on a *water-bath* and evaporate until the water is wholly removed. A white hard lustreless solid called caustic soda is said to be obtained. Here, again, the authors' experience is probably extraordinary. We are also under the impression that the student would be disappointed with the result of the experiment figured on p. 30 of the "Elementary Chemistry."

Next, as to the arrangement of the course. What strikes us most, and what we are least prepared to excuse, in the "Practical Chemistry," is the entire absence of anything approaching to a *systematic* arrangement. Part I. consists of 102 pages, and the chapters bear the following headings: I. Chemical and physical change; II. Elements and not-elements; III. Not-elements divided into mixtures and compounds; IV. Conservation of mass of matter; V. Methods of bringing about chemical changes; VI. Chemical properties of water; VII. Classification of oxides; VIII. Acids and salts; IX. Classification of salts; X. Alkalis, and alkaline hydroxides; XI. Reactions between acids and salts; XII.–XV. Classification of elements; XVI. Conditions which modify chemical change; XVII. Oxidations and reductions; XVIII. Strong and weak acids. In Part II. (78 pages) the chapters are headed: I. Laws of chemical combination; II. Equivalent and combining weights; III. Molecular and atomic weights; IV. Dissociation; V. Reacting weights of compounds determined by chemical methods; VI. Chemical change; VII. Chemical classification. At the outset the authors are strictly conservative, and in the most orthodox manner possible in the first three chapters instruct the student to dabble with a variety of substances never heard of for the most part in ordinary life, and to this we most strenuously object. We are convinced that the only way of beginning to teach chemistry, if the object be to cultivate the faculties of experimenting, observing and reasoning, is to deal with familiar objects and phenomena; and that at the very outset, after as far as possible determining the properties of familiar objects by means of ordinary appliances, we ought to set our students to analyze. We hold that air and the phenomena of combustion should be first studied: the composition of air should be determined, and oxygen should be *discovered* by the student. This we

believe to be both historically and scientifically the correct method. The composition of water should next be qualitatively ascertained. It is a *sine quâ non* that the experiments made with the object of solving such problems be throughout logically interrelated; each experiment should be suggested by the experiment or experiments previously made, and should be made with the object either of verifying or extending the information previously gained. When a student is told to perform experiments selected by the teacher for no apparent reason and merely with the object of demonstrating some particular point, their value as a logical exercise is practically *nil*. In solving such problems as the composition of air and water, &c., the student insensibly realizes the distinctions which are to be drawn between mixtures, compounds and elements, and soon learns to appreciate the characteristic difference between chemical and so-called physical change; but we hold it to be a positive advantage not to insist too strongly on the presumed difference now that it is becoming probable that many phenomena hitherto regarded as physical essentially depend on a change in molecular composition.

These remarks apply also to Chapter IV., already referred to, and to Chapter V.; in this latter, the slain of previous chapters are rekilled. Chapter VI. is headed "Chemical Properties of Water." Experiment 1 was quoted above and appears to be intended to serve as proof of the composition of water. Experiments 2–7 have nothing whatever to do with water, but relate to the preparation and properties of hydrogen and oxygen. Experiment 8 involves the examination of the residues from the preparation of hydrogen and oxygen. Then follows the oracular sentence: "Water is a compound of hydrogen and oxygen; let us examine a few of its properties." Experiment 9 therefore directs the student to add powdered copper sulphate crystals, potassium nitrate and tartar emetic to separate portions of water, and to take note that water acts on these as a solvent, inasmuch as their composition is not changed by it. Experiment 10 consists in adding anhydrous copper sulphate, and also solid sulphur trioxide to water; in both cases, it is found that the water not only dissolves but acts upon the substances. Here the chapter ends: we question whether the most conscientious performance of the experiments will lead the student to acquire any clear conception of the "chemical properties of water."

Thus far we have confined our remarks to the opening chapters, it being our opinion that these are all-important in a work which purports to teach the elements of chemistry. But there is much in the arrangement of the remainder of the book to which we venture altogether to take exception. Thus a fatal error of judgment has led the authors to postpone the experimental discussion of the laws of chemical combination and of equivalent and combining weights, as well as of molecular and atomic weights, to Part II., placing in advance of these all-important subjects a variety of matters—among others a discussion of the properties of the various elements classified in groups in accordance with the periodic law—which cannot properly be considered without a fairly complete knowledge of the laws of chemical combination. It is obvious that the authors to some extent recognize their mistake, as the order is different in the *companion* volume, the laws of chemical combination, and symbols and formulæ being discussed in Chapters V. and VI.

The "Elementary Chemistry" contains a third part dealing with subjects which are only touched on in the companion volume; this part is to be used in conjunction with portions of the "Principles of Chemistry," by one of the authors. Chapter I. of this part should have been included in Part I.; the remaining chapters ought never to have been introduced into an "Elementary Chemistry," and are obviously only included because of the senior author's well-known tendency to worship physical constants. Thus Chapter II. is headed Dissociation, and directions are given for the performance of Lemoine's experiments on the dissociation of hydrogen iodide, and of Horstmann's on ammonium carbamate: the authors evidently to some extent foresee the probable result of making such experiments, as, in summing up those on hydrogen iodide, they say: "The results of your experiments *ought* to show" that such and such is the case. How often would they? Chapter III. bears the title, "Relative Affinities of Acids," and in it experiments are described illustrating Thomsen's and Ostwald's methods; the same subject is briefly referred to in Chapter XVIII., Part I. The main objection to this chapter is that students of elementary chemistry are incapable of performing such experiments with sufficient accuracy. Moreover, it cannot yet be admitted that the conception introduced by Thomsen is warranted: until the part which the water plays is determined, neither Thomsen's nor Ostwald's results can be accepted as furnishing estimates of the relative affinities of acids for a given base. A similar remark applies to Menschutkin's etherification experiments, the repetition of which is directed in Chapter IV.: the complete interpretation of these is yet to be given.

Nothing is farther from our intention than the desire to disparage the study of so-called physical properties—on the contrary, we hold it to be of primary importance that a *proportionate* amount of attention should be devoted by students of chemistry to the physical side of their science; but let them learn before all things to regard the phenomena from the true chemist's point of view. Chemistry is to a large extent an art: a large number of relationships and peculiarities which are obvious to the skilled chemist will probably always elude mathematical treatment; it appears, indeed, to be as impossible to give formal expression to them by means of physical constants as it would be to define the work of a great painter after spectroscopic analysis in terms of wave-lengths. Especially have we felt this to be the case on reading through Ostwald's invaluable work: it has frequently struck us that he has perhaps unduly forgotten his art as chemist in the exercise of his great technical skill in determining and setting forth physical constants, the result being a picture which fails to satisfy. But it is not to be denied that chemists as a class have not yet acquired that belief in the power of physicists to help them forward which, with or without reason, is demanded of them; and this is not difficult to understand. The establishment of the doctrine of structure—the great achievement of modern chemistry—is the outcome solely of chemists' labours; in this particular case, the study of physical properties has served to confirm the conclusions of chemists, but there is nothing to

show that it could ever have led to them. And all recent attempts to directly apply the results of physical determinations have proved most unfortunately barren of results: a striking example of this is afforded by the complete failure which appears to have attended Thomsen's attempt to deal with the vast mass of thermal data accumulated by his unwearied study of carbon compounds. Chemists have not as yet received much assistance from physicists: the determination of physical constants has served to give precision to chemical statements, but little else; and it is not probable that it will ever be otherwise. In fact, the attitude of the two classes of observers towards natural objects is different, and appears to be somewhat as follows. The physicists are much like a party engaged in the investigation of a strange nation: they walk through the streets of its towns and most carefully observe how the houses are externally constructed and arranged, and study the traffic in the streets, but they do not enter the houses or take note of the mental peculiarities of the people. The chemists, however, enter the houses: they observe their internal structure, they determine the influence of this internal structure on the character and occupations of the inhabitants, of whose mental peculiarities they also endeavour to gain clear conceptions. Those chemists who are satisfied to merely cross the thresholds without continuing their studies and researches, and who therefore have much to learn before they can appreciate the labours of their more active and curious brethren, have no right to take upon themselves the functions of law-givers.

Lastly, a few words regarding the illustrations. It will no doubt be said that these are only diagrammatic; that students are to perform the experiments themselves and therefore will become acquainted with the actual apparatus. But even diagrams should be drawn to scale: Figs. 37, 38, and 43, are illustrations which show how frequently this is not the case: if such very wide-mouthed flasks were always used as are pictured in most of the diagrams a small fortune would be expended in corks. An elementary work should be properly illustrated by drawings which fairly represent the actual apparatus, as such a book will necessarily fall into the hands of those who have no knowledge of apparatus, and therefore need guidance.

From our remarks it will be gathered that we entirely disapprove of the "Practical Chemistry" as a book for beginners: we do not recommend it even to more advanced students. Teachers will no doubt be able to cull a few useful hints from it, although there is a striking absence of originality or novelty in all practical details.

We have little to say of the "Elementary Chemistry." It is an infinitely better book than the companion volume, and a fairly advanced student will find in it much information of interest and value not to be met with in any other current work of the kind. But it is not an elementary chemistry in any proper sense of the term, and, as in the companion volume, the attempt is made to crowd far too much matter into the space at disposal.

In expressing our opinion thus plainly, we have been guided by the desire to do something to stem the ever-flowing tide of so-called elementary text-books of chemistry; these are mainly the outcome of the existence in this country of a vast amount of pseudo-chemistry, and of little true chemistry, and the very existence of such books is doing an infinity of mischief in helping to perpetuate the evil. We believe that it would be of great advantage to chemical science to form an Association to prevent the further publication of elementary works other than such as had been carefully revised and approved of by a Publication Committee of the Association. The harm done by unsystematic and illogical teaching, and by vague experimenting, can never be repaired, and it is incumbent on an author to ponder the meaning and effect of every word, line, and sentence of an elementary text-book.

The authors of the "Elementary Chemistry" say that the book does not profess to be a descriptive catalogue of chemical facts regarding the properties of the individual elements and compounds. But until a satisfactory practical elementary chemistry shall have been written, it is far better that students should gain simply an exact knowledge of chemical facts, and that in their practical work they should be guided by books which we all acknowledge to be sound, though we may think that they are far too restricted in range. Let each school purchase as many copies as possible of a grand old standard work such as Miller's large "Inorganic Chemistry," full of honest common-sense and all but free from fads, and let this serve as the book of reference. A fair understanding of the broad principles which underlie the science may be gained from books such as Cooke's "New Chemistry," and Wurtz's "Atomic Theory," both master-works in their way.

H. E. A.

CHINESE CIVILIZATION.

China: its Social, Political, and Religious Life. From the French of G. Eug. Sîmon. (London: Sampson Low and Co., 1887.)

THE reader who takes up this volume, expecting to find it an ordinary popular sketch of Chinese life and manners, similar to dozens of others which have gone by and dozens which are doubtless yet to come, will be totally mistaken. For in place of a colourless account of China—if any account of that wonderful country with its marvellous civilization could be written wholly devoid of colour,—and a jejune outline of the peculiarities of the Chinese, the reader will find here one of the most closely reasoned, original, and powerful defences of the Chinese social and political system that have ever been published in Europe. Writers of eminence, indeed, there have been who have selected some special peculiarity of Chinese religion, society, or politics, and have held it up to the West as worthy of imitation, and as a mark of profound wisdom; but M. Sîmon defends Chinese polity and civilization all along the line. He lived in China as a French official in the critical years succeeding the war of 1861-62; he travelled widely, and he observed keenly. This volume was not written in the first flush of pleasure and surprise at the strange and wonderful things he saw about him; he returned home, and has had ample time to correct first impressions, to review conclusions formed on the spot by the light of subsequent experience and knowledge, and years afterwards he is able to tell to the West that, as of old, the wise men still come from the East, and that the highest product of the human mind is to be found in the civilization of China. The most civilized

State is that "in which on a given area the largest possible number of human beings are able to procure and distribute most equally amongst themselves the most well-being, liberty, justice, and security." Measured by this standard, China is pronounced to be the most highly civilized country in the world, and the Chinese have this peculiarity—that, while modern nations are only the collateral successors of those of antiquity, China is the direct heir of the generations which created it. "Its history shows the phenomena of heredity in regular succession, neither modified nor obstructed by change of medium, with the evolution of events and ideas —an evolution as regular as that of living beings, freely proceeding unshaken and untroubled by any exterior influence, by which its direction might have been altered or its development retarded; and it is here, I repeat, that we find the deep and original interest of China, and perhaps also the secret of her extraordinary longevity." The book is a study of the progress and organization, in short of the civilization, attained by humanity under such conditions of liberty and development. The student in this case is full of love of his subject, and this no doubt is a great advantage, although it has its disadvantages also. M. Sîmon tells us of a land flowing with milk and honey, moral as well as material. Nothing that he has seen is inharmonious or out of place; everything is for the best, and has had the best effects. Chinese civilization is not a dead, rotten branch, as it is usually represented to be, but a living active power for good; in fact, "nowhere in the world is there such proof of force and vitality" as in the Chinese character and in Chinese civilization.

The book is divided into five parts: (1) the family; (2) labour; (3) the State; (4) the Government; (5) the Ouang-ming-tse family, in which he gives the history of the life, labours, and pleasures of a family with which he got acquainted in his travels, besides illustrating by a concrete instance how Chinese polity and administration work out in an individual case. With regard to the family, he says that it is at the hearth that the government of the country is carried on. The family has the power of passing judgment on any of its members for an offence, and can sentence the delinquent to whipping, exile, and excommunication. From the decision of the domestic tribunal an appeal is permitted to the ordinary courts of justice, but it is unusual for such an appeal to be made. Such is the respect paid by the Chinese to their traditions that there are few who do not submit at once to the sentence passed on them by their family. No punishment inflicted on a Chinaman can be more terrible than exclusion from the family. Socially he becomes an outcast, and, driven from the shelter of his ancestral home, and the protection of the spirits of his ancestors, he wanders in search of employment over the world, and it is the thousands of these abandoned ones who flood the American labour-markets. In the family, ancestral worship is cultivated, and is one of the strongest incentives to labour and progress: each member looks on himself as the guardian of posterity, toiling for their benefit, and satisfying the ancestors who watch over the family home. Each family religiously preserves the records of its ancestors, their lives and acts; and to the assembled members these records are read by the head of the house at regular intervals. At each meeting one biography is read, then the next, and so on in order, till the last of the series is finished, when a commencement is again made with the first. With regard to these family records, M. Sîmon sees no more noble sign of the honesty and independence of the Chinese than the fact that, when any question is in dispute, an entry in one of these sacred family books referring to the dispute is looked on by the authorities as decisive. To be able to make the entries in this book, and to read it to his family, should he ever become its head, every Chinaman is taught to read and write; of this, in connection with education, we shall speak later on. Property is collective and individual; and the living holders look on themselves as the trustees of posterity. The fee-simple belongs to the community, except in a few fast-diminishing cases, where small portions of land are owned by each family, and are considered inalienable; and he who dares to introduce a stranger into this patrimonial land commits sacrilege, and becomes an outcast. China has been described as a despotic monarchy, but there is perfect liberty to all. Religions of all kinds are tolerated and are never interfered with except for political purposes. All public meetings and expressions of public opinion are freely permitted. To prove this, M. Sîmon says that in 1863 he made in one province a collection of proclamations of great virulence, denouncing the Emperor for agreeing to the treaty with the Europeans after the sack of the Summer Palace and the burning of the great library, and they are very numerous: none of the mandarins, he adds, dared to prosecute their authors. Taxation is very light—not one-hundredth part of what it is in France. With regard to legislation, the Academy of Sciences at Pekin is the only legislative power. If any official thinks that a custom, generally observed in his province, might with advantage be used over the whole country, he sends an account of it to this body, which examines it, and, if it thinks the custom useful, orders it to be tried in the other provinces; if successful there, it is finally adopted, inscribed in the code, and becomes law. Though M. Sîmon reserves a more extensive account of education in China for another work, it is easy to gather his views from the present book. The Government gives full liberty to all to open schools. The children are well taught, and there is scarcely a Chinaman who is not able to read, write, add up accounts, and draw. The foundation of the education is laid in the family. From their earliest years, children are taught their duties and their rights. They are taught respect for others, and hence respect for themselves. Obedience to usages, humanity, justice, and right feeling —these are the foundations of their education. Besides the family education there are two kinds of public instruction,—primary and superior. Primary education is given in the institutions attached to the family temples, where there are such, or in private schools, which anyone is at liberty to open. The education of every child is provided for, apart from Governmental aid, the rich paying for their poorer brethren. Inasmuch as each Chinese sign conveys an idea, the child that is taught to write the Chinese characters learns not only words, but ideas, and he is forced to explain and comment on these to his teachers. And it is to this fact, in addition to the influences of family councils and family readings, with the profuse inscriptions in every public place, that M.

Sîmon ascribes the amazing intelligence and precocity of Chinese children. With regard to higher education, it is open to all. The Government give barely the necessary expenses; the rest is contributed by private donors and by the students themselves, of whom there is always an abundance. The directing staff is paid by Government, the teaching staff by the students. Those who wish to enter the public service are trained and examined at the Hanlin College or University of Pekin. All appointments are given to the graduates according to their degrees; the higher the degree the more honourable and lucrative the post. The graduate takes precedence of all minor officials, and ranks with a minister or viceroy, whose post he frequently fills when he has had a little experience in public life. He has rooms allotted to him in the palatial universities. For these degrees the competition is very severe. All the professions stand on an equal footing, except those of teaching and letters. In no country is the man of letters of such influence as in China. Old age alone makes others as worthy of respect as he. Whenever M. Sîmon found the Chinese distrustful or indifferent to him, he always humoured this opinion of their value of learned men, by seeking out the most learned man in the place and paying his respects to him. The tutor retains a life-long power over his pupil, and frequently the people, when they have had some cause of complaint against an official, have sent long distances to bring his tutor to expostulate with him. The great goal of the literary man is to obtain a public post, such posts being held in high esteem in China. There are few vacancies, however, and the vast majority of candidates being unsuccessful become tutors, public writers, &c.; others turn their talents to commerce and agriculture, and so elevate the educational standard of the industrial classes. Labour is so honourable that handicraftsmen rank as high in public estimation as lawyers and doctors.

M. Sîmon sums up his views of Chinese civilization, of which a few examples have been given here, by stating that the fact which always seemed to him the most wonderful "was the progressive substitution of individual for collective action in all the works of civilization, from the simplest to the most complex, from mental to material. The individual freed from the slavery of collectivity, independent, and free in unity, thanks to that unity, is the salient fact apparent from the study of the relations between the people and the Government in China, and appears to me to justify the theories prevalent there." Very few readers who possess a personal acquaintance with China and the Chinese will be found to agree with all of M. Sîmon's statements of fact, or with all of his conclusions from them. But he has nevertheless produced a book which deserves to be carefully studied, and which will strike the mind by the originality of its propositions and the skill and ingenuity with which they are defended. In these days, when the Chinese are treated amongst many highly civilized communities in different parts of the globe with loathing and scorn, and when elective Legislatures do not hesitate to speak of members of the Chinese race as *hostes humani generis*, it is perhaps well to be reminded, as M. Sîmon forcibly reminds us, that this race has solved, apparently with success, some of the social and political problems before which Western statesmen and philosophers stand helpless.

THE METHOD OF CREATION.

The Creator, and what we may know of the Method of Creation. The Fernley Lecture of 1887. By W. H. Dallinger, LL.D., F.R.S. (London: T. Woolmer, 1887.)

IT is not the province of this journal to deal with theological questions; at the same time, the one discussed in this volume is in such close relation with science, and of such universal interest, that a brief sketch of Dr. Dallinger's argument may be permitted. He deals with a question which takes precedence of those sundering Churches,—one which may briefly be stated thus: Have the recent advances in physical and biological science placed the Theist in an unreasonable position? Obviously this is a fundamental question. If the answer be in the affirmative, all investigations into the minutiæ of theology are less than the shadows of a shade.

Dr. Dallinger commences by pointing out the necessary limits of scientific inquiry. On this he insists, not in any hostile spirit, but only because it is so often forgotten. "The researches of science are physical. The observable finite contents of space and time are the subjects of its analysis. Existence, not the cause of existence; succession, not the reason of succession; method, not the origin of method, are the subjects of physical research. A primordial cause cannot be the subject of experiment nor the object of demonstration. It must for ever transcend the most delicate physical reaction, the profoundest analysis, and the last link in the keenest logic. Absolute knowledge concerning it can only be the prerogative of itself."

This, of course, is a position which many so called Agnostics would frankly accept. But in working out the argument the author indicates that a more definite creed is attainable. Commencing with the physical universe, he shows that whatever discoveries have been made, whatever simplifications introduced into the so-called laws of which it is the result, the physicist is at last arrested by two mysteries—matter and force. But what are these, "the alpha and omega of existence" as some would call them? They are two names, and nothing more. We deal with the properties or qualities of matter, with the consequences of force, but we are no nearer to knowing the one or the other. In addition to these, however, many hard-headed thinkers assert "the existence of a third thing in the universe—to wit, consciousness." Now we may juggle as we please with these terms, we may construct on them elaborate systems explanatory of the universe; but beyond laws either mechanical or vital there lies inevitably, however we may try to smother it by words, the idea of causation; and from this idea that of "volition" cannot be separated. We are, as the author shows in an elaborate argument, reduced at last to this alternative: "either chance or mental purpose gave primal origin to all that is." The former he shows is almost inexpressibly improbable: most men will not hesitate to accept the latter.

Considerable space is next devoted to a discussion of Mr. Herbert Spencer's view that "from matter in motion, and nothing else, the whole universe is supposed to arise; life emerges; and mind in its most transcendent forms comes forth." In this discussion we are again confronted

with an alternative: either the primordial matter was in a state of homogeneity, and so "infinitely incapable of change," or the homogeneity was disturbed by some external force. But an outside influence is not in the philosophic system. "The admission of inability to evolve the universe without it is an admission that the mechanical philosophy fails at the outset. Nor can it serve the emergency to invoke 'force.' A Divine origin of the universe is usually rejected, because the Divinity eludes the methods of science. But we cannot supplant the Divinity by enthroning force. Science can tell us what force *does*, but it can no more find out what force *is* than what an infinite mind is. Force is an irresistible mental inference from matter in motion, but its ultimate nature is defiantly beyond the reach of science."

The phenomena of life, as exhibited in one of the lower and more minute organisms, are then considered. These are "free and self-originating action"; multiplication; and cyclic change in each new organism. Tiny and humble in organization as these creatures are, they differ vastly from chemical compounds of any kind. The force which animates them differs widely from any mode of force which we call physical. So far as we at present know, the break between "life and not life" is abrupt. Hence, whether or not in the remote past the transition from the one to the other may have been what we should call continuous, our present knowledge offers no explanation of it, and the fact is a stumbling-block in the way of a purely mechanical philosophy.

The remainder of the essay is chiefly devoted to a discussion of the theological aspect of the theory of evolution. This, as designed for the non-scientific part of his audience, need not be further mentioned in these pages. It will be enough to say that, as is now generally admitted by the more intelligent among theologians, he maintains that there is no necessary antagonism between their beliefs and scientific theories.

As might be expected from him, Dr. Dallinger is temperate in expression and eloquent in language. Some readers perhaps would have preferred a little more conciseness in style and statement, but it must be borne in mind that the discourse was delivered as a lecture to a non-scientific audience, who required leading gradually, or even alluring, into unfamiliar paths of thought. Among such persons the book cannot fail to do excellent work in allaying needless fear and silencing ignorant clamour; among opponents it will serve to show that the Theist's position is more defensible than they suppose, and that, in their own, unsuspected difficulties lurk beside the seemingly easy path of a euphonious terminology.

OUR BOOK SHELF.

The Harpur Euclid. Book I. By E. M. Langley and W. S. Phillips. (Rivingtons, 1888.)

THE editors are mathematical masters of two Bedford schools under the Harpur Trust; hence the title. For the work itself the title-page further informs us that it is an edition of Euclid's "Elements" revised in accordance with the Reports of the Cambridge Board of Mathematical Studies, and the Oxford Board of the Faculty of Natural Science. Extracts from these Reports are given in a prefatory note: this is the only part of the work which is not strictly adapted for the use of school-boys.

We began our task with no special liking for it, but had not proceeded far when we found that there were new adornments which rendered our perusal of the familiar lines very agreeable. We read on through 102 out of the 120 pages without break, and then ceased, as we had come to some matters which required more careful examination. The editors have kept to the usual sequence, but in many cases have replaced the Simsonian demonstrations by easier ones, and have discarded much of the superfluous matter which has led anti-Euclidians to inveigh so strongly against the "Elements."

We are glad to see that exercises come in right from the outset; these all seem to have been most carefully selected, and are such as a fairly intelligent boy ought to be able to solve from the previous propositions. We refer here to the examples in the body of the book. Frequent reference is made to that excellent, though perhaps hardly sufficiently appreciated, little book of Prof. Henrici, "Congruent Figures," and to the "Syllabus" of the Association for the Improvement of Geometrical Teaching. At the end, as a kind of appendix, are some judicious sections on properties of triangles, on quadrilaterals, on loci, on solving geometrical problems—(1) method of intersection of loci; (2) method of intersection of sets; (3) method of analysis and synthesis. Considerable pains has been bestowed on the arrangement of the text, the selection of the various types, and the drawing of the figures; in fact, the little book is the beau-ideal of a Euclid for boys. We wish we had had such a book in the "auld lang syne," and then our first perusal would not have been so painful. It is the authors' intention to bring out the successive books in like form. We wish them like success, and trust that their venture will find a welcome in many a school.

A Course of Quantitative Analysis for Students. By W. N. Hartley, F.R.S. (London: Macmillan and Co., 1887.)

AFTER the almost infinite number of books, mostly small, "and mostly to meet certain requirements of our own students" on qualitative analysis, it is a relief to meet with a small book for students—beginners—on quantitative analysis, written evidently for beginners, and in a manner to really lead them up from qualitative notions, not by one great bound, but by good sober practice and order, to the appreciation of the care and exactitude, and most important still, the "criticising" state of mind necessary to make a real analytical chemist.

As the author says in his preface: "To be a good analyst does not necessitate a profound knowledge of chemistry;" but any student who has worked at all well through this little book will have a good platform of knowledge under him, and be in a position to enlarge his knowledge with infinitely greater ease, and that very necessary regard for accuracy which is not possible to a student who has not done any quantitative work.

The author begins in a sensible manner by giving the metric weights and measures, with English equivalents, and then the dimensions of various laboratory apparatus, beakers, &c., and all this is very useful. In the introduction, manipulation and reagents are dealt with. The author might have added the use of folded or plaited filters. It is quite as safe and accurate to use them for quantitative purposes as to employ a pump.

Before proceeding to simple estimations of constituents of salts, &c., we have about twenty pages of introductory examples devised with the intention of enabling students to realize the meaning of the atomic and equivalent weights of elements; which they do not always do when put on to determinations without any introduction. This is the most useful and original part of the book. The following exercises, "simple estimations," are fairly in order of difficulty. The middle portion of the book is on volumetric analysis. It is short but workable, and is followed

by a good section on analysis of silicates and some technical products. The book does not attempt to cover all the field of analysis, but what is done will be found really useful by a beginner or a junior student.

W. R. H.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

"A Conspiracy of Silence."

THE Duke of Argyll can scarcely be congratulated upon his latest discovery of a new ground of attack upon geologists. In the year 1862 a very eminent physicist, whose loss we all so deeply deplore, made the somewhat rash suggestion that flint implements are found deep down in the drift, owing to their high density as compared with that of the matrix in which they are inclosed. Seeing that the material in which the implements are found is usually a *flint*-gravel, everyone acquainted with the subject saw that the suggestion was, to say the least, a somewhat unfortunate one, and Prof. P. G. Tait, in seeking for an opportunity to sneer at "advanced geologists," was scarcely kind to the memory of a deceased friend in rescuing such a suggestion from oblivion. But to the Duke of Argyll, the finding of a new basis from which to attack geologists seems to have been a chance which he could not afford to let slip.

The Duke of Argyll now asks when we are going to begin to discuss his magazine-article upon coral reefs. I reply that in the article in question there is not a single new fact or fresh argument—nothing which has not been already brought forward by Mr. Murray himself, or by Dr. Archibald Geikie, and met by Prof. Dana in a singularly exhaustive memoir well known to all geologists. The subject has, moreover, been treated at considerable length by Profs. Prestwich, Green, James Geikie, De Lapparent, and others. Surely no exception can be taken either to the eminence of the authorities who have written on the subject, to the length to which their notices have extended, or to the prominence of the journals or treatises in which these discussions have appeared. If it be said that the general scientific public have not had the matter fully laid before them, it is only necessary in reply to call attention to the pages of NATURE, in which a succession of articles dealing with the subject will be found.

The Duke of Argyll says that he has "nothing to retract." Here I regret to have distinctly to join issue with him. He has asserted that scientific men have refrained from discussing a particular theory, and that in taking this course they have been actuated by the worst of motives—a fear of the truth; he has charged the Geological Society with refusing in the spring of 1885, through its then President, to accept a certain paper from the same cause; and now he adopts and gives fresh currency to an equally offensive charge of a similar kind.

These charges have, each and all of them, been shown to be absolutely destitute of foundation. The Duke of Argyll must judge for himself if the principle of *noblesse oblige* should not lead him, not only to *retract* the charges, but also to *apologize* for having made them. But his Grace may rest assured that, until he does so, the grounds for the deep indignation at his conduct, which is so strongly felt both at home and abroad, will still remain.

JOHN W. JUDD.

On the Constant P in Observations of Terrestrial Magnetism.

I REGRET that Prof. Rücker should have largely misunderstood my last letter. I have not raised the question of fallible observations at all. Referring to the correspondence on pages 127–8 of the present volume of NATURE, my principal contention was and is that the ordinarily accepted formula for P differs by terms of the second and higher orders from Gauss's theory, and that that difference necessarily persists in any rigorous expansion of the formula. By the ordinarily accepted formula for P I mean Prof. Rücker's formula (α); and by Gauss's theory I mean my formulæ (1), (2), and (3). From two observations of $f(u)$, made respectively at the distances r and r_1, the L of Gauss's theory might be found by a direct solution of equations (1) and (2); but instead of that, it is customary to find L from equations (7) and (8) by substituting in them the value of P_0 computed through equation (α). To render the latter procedure rigorous, P should be used in (7), and P_1 in (8). Equation (11) shows that P and P_1 differ by quantities of the second and higher orders, and as the ordinarily accepted value of P_0 lies between P and P_1, it necessarily differs from one or both of these quantities, and therefore from Gauss's theory, by terms of the second and higher orders.

While freely admitting the justice of Prof. Rücker's criticism upon my arbitrary assumption that $P_0 = \frac{1}{2}(P + P_1)$, I cannot assent to the process by which he has deduced equation (γ). Equations (7) and (8) show that we may have either one L and two P's, or two L's and one P. In the latter case these equations become—

$$\tfrac{1}{2}L' = A(1 - P_0 r^{-2}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

$$\tfrac{1}{2}L'' = A_1(1 - P_0 r_1^{-2}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

and P_0 must be determined so as to make L′ and L″ as nearly as possible identical with L. To that end we must have $2L = L' + L''$; and then, from the difference between (7) + (8) and (15) + (16)

$$P_0 = B(A - A_1)\frac{r_1^2 + r^2}{Ar_1^2 + A_1 r^2} \quad . \quad . \quad . \quad . \quad . \quad (17)$$

Expanding to terms of the second order

$$P_0 = B\frac{(A - A_1)}{A}\left\{1 + \frac{r^2}{r_1^2 + r^2}\left(\frac{A - A_1}{A}\right)\right\} \quad . \quad (18)$$

Whence, by equation (13)

$$P_0 = \frac{r_1^2 r^2}{r_1^2 - r^2}\left(\frac{\log A - \log A_1}{M}\right) - \frac{r_1^2 r^2}{2(r_1^2 - r^2)}\left(\frac{\log A - \log A_1}{M}\right)^2 \quad . \quad . \quad (19)$$

This result agrees better with equation (14) than with equation (γ).

WM. HARKNESS.

Washington, D.C., December 30, 1887.

I AM afraid that the new method of calculating P_0 adopted by Prof. Harkness is not less arbitrary than that which he previously employed. He says that "P_0 must be determined so as to make L′ and L″ as nearly as possible identical with L." If the object is only to deduce a correct value of L by combining equations (15) and (16), this condition is certainly not necessary. For if we substitute from (17) in (15) and (16), and take the mean of the values of L′ and L″, we get by a very roundabout process the same value of L as we should have obtained without using P_0 at all. But we should have reached the same final result if we had started with the assumption that

$$(n + m)L = nL' + mL'',$$

where n and m are any numbers whatever. By properly choosing n and m we could deduce the correct value of L with any assigned value of P_0. It appears to me that the equation $2L = L' + L''$ is based upon the tacit assumption that L′ and L″ are to be combined in accordance with the rules applied to fallible measures, and cannot otherwise be justified if the only object is the correct deduction of L from (15) and (16).

If, however, P_0 is introduced to enable us to calculate another approximate value of L by observing (say) A_2 at some other distance, r_2, the best value to select will depend on circumstances. If r_2 is nearly $= r$ we shall get the best result by writing $P_0 = P$ and so on, so that the equation $2L = L' + L''$ is again arbitrary.

I am quite in agreement with Prof. Harkness as to the fact that if we start from the basis of equations (1) and (2) a small theoretical error is introduced by substituting P_0 for P and P_1. Indeed I think this step can only be justified by our knowledge that the inaccuracy thus caused is less than the error of experi-

ment. It is thus impossible to discuss the proper value of P_0, as Prof. Harkness wishes to do, without raising the question of fallible observations. If it is raised, the method of treatment by least squares follows.

Prof. Harkness tried to show that, although the second term which I introduced brought the approximate value of P_0 nearer to that given by the ordinary formula, it removed it further from another value which he regarded as the standard. I venture to think that I have justified my position by showing that the introduction of P_0 is useless unless the equations are regarded as fallible; that the ordinary value is that given by least squares, and that the standards proposed by Prof. Harkness are founded on assumptions which have no theoretical basis.

In conclusion I may perhaps be allowed to make two remarks, one of which would, I think, from the point of view assumed by Prof. Harkness have strengthened his case. In the first place he is wrong in saying that the ordinary value of P_0 lies between P and P_1. It is smaller than both of them if A is $>A_1$.

In the next place I may point out that by treating a number of fallible expressions of the type of equations (1) and (2) by the method of least squares, a general value of L could be found without the introduction of the small theoretical errors which have caused this correspondence. There is however little doubt that by the introduction of P_0 we obtain a more convenient and practically no less accurate method of dealing with the observations. ARTHUR W. RÜCKER.

Science Schools, South Kensington, January 10.

The Mist-Bow.

IN a letter to the *Times* of January 12, Prof. Tyndall calls attention to a white mist-bow, which he has seen on one or two occasions, and mentions its rarity of occurrence. It may therefore be of interest to record that I witnessed a similar phenomenon on January 9 last. My point of view was an elevated band-stand at the head of Weymouth Pier; the time 11 a.m. The air, as on the occasions mentioned by Prof. Tyndall, "swarmed with minute aqueous particles," *i.e.* was foggy, and on looking away from the sun, which was shining weakly, I saw a well-defined white bow cast upon the mist. The bow appeared to be about 60 feet distant. My point of view being high, a full semicircle was visible. It was, as may be imagined, a beautiful and graceful object. ALBERT BONUS.

St. Leonards, Exeter, January 13.

IN reference to Dr. Tyndall's letter in the *Times* of Thursday last upon the ullao as observed by him, I beg to call your attention to my paper read before the Stockport Society of Naturalists upon the same subject (see pp. 11 and 35). Not having seen the phenomenon described before, I ventured to call it the dew-bow. THOMAS KAY.

Moorfield, Stockport, January 14.

THE character and persistence of the recent fog have been so exceptional that perhaps you may deem the following observations on it worthy a record in NATURE.

I was staying in Mid-Devon at a place in the valley of the River Taw, some 10 miles north of Dartmoor. On Monday, the 9th instant, we were enveloped in a dense, damp, white fog, a rare occurrence in that part of the country. Surmising that the fog had no great vertical thickness, I sallied forth in the afternoon to mount a hill immediately to the eastward. At a slight elevation the sun was already making his appearance, and as I continued my ascent, and the fog became more and more thin, I saw before me on the then pale blue sky a beautiful white bow, similar to the rainbow, only broader and without colour. When the top of the hill was reached, the fog and bow had disappeared, the sky was deep blue, and the sun shining with quite spring-like warmth.

The scene I now had around me was most enchanting. The fog could be traced lying in the river valleys like arms of the sea, with the bordering hills simulating cliffs, and here and there an island appearing in the midst, whilst the distant Dartmoor hills stood out calm in unbroken sunshine. No movement of the air could be detected, but, below, the surface of the fog seemed as if being rolled along by a wind from the east towards the river valley. The white fog-bow is seldom seen, and I imagine owes its absence of colour to the minuteness and close proximity of the water globules, allowing the divided rays to coalesce and so again form white light. C. O. BUDD.

Atmospheric Effects at Sunset.

ON Sunday, January 8, upon leaving the house at half-past four in the afternoon, I observed that the clouds were suffused with a kind of pink or lurid coppery tinge, a sort of angry sunset tint spread over the whole sky. The clouds were of the stratus type which is common in a winter anticyclone, but were moving or rather driving with a swiftness quite unusual under such conditions. The barometer was very high and rising rapidly; but during the afternoon there were several violent and noisy gusts of wind almost amounting to squalls, though during the greater part of the day the atmosphere was still almost to stagnation. The air was mild and intensely humid, and everything was dripping with moisture. In fact the weather was in many particulars the opposite of what we expect during the prevalence of an anticyclone. The diffused sunset effects were quite unlike anything I ever remember to have witnessed before. The gas-lamps had just been lit, and the flames not only appeared of a greenish tint, but seemed to be inclosed in green glass. Several persons stopped me in the street and inquired what it all meant, and one acquaintance said, "What is going to happen?" In the green tint of the gas there is, of course, some suggestion of a colour complementary to the strange red glow which seemed to pervade the atmosphere. But in the absence of all, even the most rudimentary, knowledge of the subject, I should be glad if you or some of your readers can explain the cause to me and to others who witnessed the unaccustomed phenomenon.

CHARLES CROFT.

Prestwich, near Manchester, January 9.

Newton's "Principia."

IT may perhaps interest your readers to know that the 200th anniversary of the publication of Newton's "Principia" was solemnly celebrated on December 20 (old style) by a united meeting of two learned Societies of Moscow—the Imperial Society of Friends of Natural Knowledge, and the Mathematical Society. Prof. Mendeléeff, of St. Petersburg, was Honorary President. Prof. Stoletow (President of the Physical Section in the first-named Society) presented a sketch of Newton's life, and spoke on his optical discoveries; Prof. Zinger (President of the Mathematical Society) treated Newton's mathematical work; Prof. Joukowski pointed out his merits as founder of rational dynamics; and Prof. Ceraski exhibited the creation of celestial mechanics by Newton. The large hall of the Polytechnic Museum, where the meeting took place, was attended by the *élite* of the city. The lectures were illustrated by some optical experiments with electric light and some lantern-slides relative to Newton's biography. A. STOLETOW.

University of Moscow, December 21, 1887 (January 2, 1888).

Meteors.

IN the moonlight on the evening of January 2, at 10h. 58m., a fine meteor, equal in brightness to Jupiter, was observed by Mr. D. Booth at Leeds, and by myself at Bristol. As seen from Leeds, the meteor passed from Musca to the head of Cetus, and terminated its course about 3° east of α Ceti. It moved rather quickly, leaving a long thin train. The fore-part of the nucleus was tinted with red, but the train was yellow. At the finish the motion became slower. At Bristol the meteor was first seen when about 6° S.E. of ζ Draconis, and it travelled some 8° in the direction of β Cephei. Colour yellow, motion very slow. The course was evidently much foreshortened close to its radiant.

Comparing the two paths, it will be found that they intersect each other at $250° + 57°$, so that the meteor was not a member of the January Quadrantids, which attain a maximum on January 2, but belonged to a neighbouring shower of Draconids, which, between January 14 and 19, I have previously observed at $253° + 56°$. The meteor appears to have been observed earlier in its flight at Bristol than at Leeds, for at the latter place the observer was watching the southern sky, and only caught the later part of the course. From a mean of the two observations the height at commencement was 98 miles above a point west of Appleby, Westmoreland, and the end occurred at 60 miles above Chester. The earth-point was near Tiverton, in Devonshire. The real length of path was 109 miles, and it was inclined at an angle of $20\frac{1}{2}°$ to the horizon. The meteor was travelling in a direction from north to south, the bearing of the radiant being N. $8\frac{1}{2}°$ E.

It would be interesting to hear of further observations of this bright meteor. It must have been seen by many persons, as the night was very clear.

The fireball of February 21, 1865, had a radiant at $255° + 55°$, and close to that of the meteor of January 2 last, but the difference of date is too considerable to permit an inference that the wo bodies diverged from the same stream.

January 8. W. F. DENNING.

IN NATURE, November 10, p. 36, it is stated in reference to a meteor that "a Norwegian astronomer" is of the opinion that the track of the meteor must have lain too high to be heard. "He calculates from the reports to hand that the bursting of the meteor occurred at an altitude of about 6000 feet (*sic*), and he thinks that even this figure may be safely doubled."

It may interest some of your readers to know that on the night of July 3, 1884, at 8.27 p.m. standard time of the 75th meridian, a meteor was seen by me, as well as by others, here, and about 5m. 17s. afterwards a sound was heard something like distant thunder, except that it seemed to swell rapidly and steadily to a maximum intensity, and then diminish again in much the same way, but more slowly. I immediately connected the sound with the appearance of the meteor, and stated that it must have been a little over sixty miles distant, and from the estimated angle of elevation about *thirty miles* above the surface of the earth. This estimate was borne out by the accounts from other places of the course of the meteor. The sound I should be inclined to attribute to the rushing together of the air in the wake of the meteor, or perhaps more probably to the sudden compression of the air in front of it, and not to its bursting.

The following account of the meteor was given in the *Canadian Weather Review* of July 1884:—"A magnificent meteor was seen on the night of the 3rd at 8.27 p.m. standard time, passing from south-east to north-west, colours brilliant red and green. Two distinct explosions are reported to have been heard. After the first explosion a sinuous streak remained visible until covered by clouds; the time of flight was from seven to eight seconds, and the apparent size about one-fourth that of the moon. Reports have been received from Listowel, Hastings, Beatrice, Belleville, Lakefield, Pembroke, Peterborough, Kingston, Deseronto, Lindsay, and Huntingdon, all substantially agreeing as to course, size, &c.; it passed two or three miles south of Belleville, and about the same distance north of Lindsay." CHARLES CARPMAEL.

Toronto, December 16, 1887.

The Electrification of the Air.

IN writing upon the electrical condition of the Peak of Teneriffe, the Hon. Ralph Abercrombie (NATURE, vol. xxxvii. p. 31), begins by stating that "the limited number of observations on atmospheric electricity which have been already made all point, with one exception, to a normal positive difference of potential between a point some few feet above the earth and the ground itself;" and farther on he writes: "the electrical conditions of the Peak of Teneriffe [the one exception] were the same as in every other part of the world." As similar statements still find their way into text-books and treatises on electricity and meteorology, I trust you will permit me to point out that, unless a very special meaning be attached to the word "normal," this generalization is decidedly too wide.

In a paper read at the Aberdeen meeting of the British Association in 1885 (printed *Phil. Mag.*, November 1885), I pointed out that, in Madras at least, a negative electrification of the air was a normal, and not an abnormal, condition for many hours of the day at certain seasons of the year. Observations since taken have entirely confirmed the opinion that with a hot, dry, west wind the air at Madras is usually negatively electrified, and often to a very high potential.

With regard to observations made on mountains in the tropics, though perhaps hardly within what Mr. Abercromby terms "the zone of constant electrical discharge," I would venture to call his attention to a short paper on observations made on the top of Dodabetta (8642 feet) in the Transactions of the Royal Society of Edinburgh, vol. xxxii. p. 583.

I may add that during the periods of incessant discharges of sheet lightning which we often experience here the electrification of the air is sometimes positive and at other times negative, but generally positive. C. MICHIE SMITH.

Madras Christian College, Madras, December 14, 1887.

Wind Force at Sea.

PROF. WALDO, in the *American Meteorological Journal* for October, recommends the use of instruments for determining the velocity of the wind at sea. In a paper read before the Meteorological Society, I discussed the comparative results, obtained from a great number of observations under all conditions at sea, between two very simple and small anemometers, showing that, although the two instruments were on entirely different principles, the results obtained differed only by about 10 per cent. In a paper read in March last before the Meteorological Society, "Notes on taking Observations at Sea, &c.," I again urged the desirability of observers using some form of anemometer, so that more uniform results could be obtained, and I gave a table for correcting the apparent velocity of the wind as registered by the instrument for the speed of the ship and for aberration.

For instance, at the present time you may have two sailing-ships close together, one carrying top-gallant sails, the other only reefed top-sails, and the wind will be logged accordingly. Again, two steamers going in opposite directions are very likely to experience apparently different wind velocities, and the senses of officers in steamers are not so acute for detecting differences in wind velocities as are those of officers in sailing-ships. The use of instruments would eliminate these errors.

With instruments similar to those I use—the coefficient of friction of which is slight—the relative velocity of the wind may be obtained fairly accurately; and I contend that this is of more importance than the chance there is of obtaining the estimated true velocity; and, I may add, the trouble attending the use of these instruments is small.

There are two other subjects which, up to the present, have received little attention at sea, viz. the registration of rainfall and the electrical condition of the atmosphere. Observations on both could easily be carried out on board some ships, and the observations would be both valuable and interesting.

DAVID WILSON-BARKER.

A Troublesome Parasite of a Brittle-Starfish.

IN a valuable work on certain parasitic Crustacea ("Contributions à l'Etude des Bopyriens," p. 181), Prof. A. Giard and J. Bonnier have done me the honour of calling attention to my discovery of a Copepod (?) which lives in the body of an Ophiuran, *Amphiura squamata.* They regard the mutual relationship of the Copepod and the Ophiuran as an instance of the castration of the host by the parasite. Although all my observations indicate the correctness of some such an interpretation, I failed to recognize it as a fact until after they had pointed it out. The explanation seems a possible one, and is provisionally accepted, with a few modifications, as the best as far as research has gone. The modifications are important.

The state of knowledge of the subject is as follows. Ova and young of a Crustacean are found in the body of an American brittle-star, identified as *Amphiura squamata.* In some instances an adult Crustacean was also found in the same place. When these ova, young, or adults are found parasitic in the Amphiura, the remains of the ovary of the host appear as an amorphous mass, and there is no possibility of future young of the Amphiura in the brood sac, since the ova have been destroyed.

The conclusion seems inevitable, for observations indicate that the mother Crustacean makes her way somehow into the body of the host (Amphiura), then affects the brittle-star so that the young of the host will not develop, after which she leaves packets of ova to mature in the sacs where normally young Amphiuræ would develop. It thus happens that the products of the ovary of the host are destroyed before the Crustacean ova are developed, or while they are in an early stage of cleavage. Consequently it is legitimate to conclude that if the ova of the host is destroyed it may be done by the adult Crustacean.

If Prof. Giard and Bonnier are right in their interpretation that this is an instance of parasitic castration, as I think they are, we possibly have an interesting case of a parasite destroying the reproductive powers of the host *for the future good of her own offspring.* Such a condition of things is unique, and among Ophiurans the writer recalls but the single instance of the present case of Amphiura. The case of the Crustacean and its brittle star host seems to differ from that of Entoniscus in that in the one instance the destruction of the ovary may be of advantage to the parasite, while in the other the destruction or

modification of the spermary of the host is simply a concomitant circumstance of the parasitism. It seems hard to believe that the simple presence of the packets of Crustacean ova in the brood sac of an Amphiura would lead to a destruction of the ova of the brittle-star, but it does not seem impossible that the adult Crustacean could have spayed the Amphiura.

The character of this phenomenon is so unusual that one hesitates to accept it on insufficient data. There are gaps in my observations which may be serious to the theory. In the first place, it has not been observed that the Crustacean spayed the Amphiura. The ovarian gland of the brittle-star is destroyed, and indications point to the Crustacean as the culprit. Secondly, it is not known that the parasite enters the *brood sac* through the genital slits to deposit the ova. Thirdly, the difficulties of determination whether the ova are in the body cavity, stomach walls, or brood sac, are very great. I believe it is probable that they are in the brood sac. Lastly, the family name of the strange parasite who repays hospitality so ungraciously is unknown. There is no doubt that it is a Crustacean, as I have traced the egg through a nauplius into an adult.

As this condition of life is believed to be a novel one, and needs verification, the writer takes this opportunity to call the attention of marine zoologists to it, and to request correspondence from anyone who may have made similar observations. Before we can definitely accept the conclusions towards which my observations lead, there is a call for re-examination and verification of the observations. The most important question is to determine whether or not the ova of the Crustacean live in the brood sac.

Cambridge, Mass., U.S.A. J. WALTER FEWKES.

Raised Beaches *versus* High-Level Beaches.

IF you can find space for the subjoined list of shells from the ancient beach on the Thatcher rock in Torbay, it may prove acceptable to such geologists as interest themselves in the question recently resuscitated by Prof. McKenny Hughes, as to whether the ancient Devonshire beaches are "raised," as commonly supposed, or merely high-level, as some hold them to be.

Added to the late Mr. Godwin Austen's "Hope's Nose" list, my list runs up the total number of species from the two beaches to forty-six, and this without reckoning Mr. Godwin Austen's *Cardium tuberculatum*, which I think must have been an oversight for *C. echinatum*. This number has not, I believe, been beaten by any British raised beach hitherto.

When the Thatcher beach was accumulated, the northern shell *Trophon truncatus* was abundant in the neighbourhood; so was *Tellina balthica*, a shell which only occurs, I believe, in this vicinity, in or near the tidal harbours of Torbay.

The Thatcher collection evidences the great antiquity of the beach, a considerable change of temperature, differences in the rock-components of the coast-line, and variation in its contour. Of these subjects I hope some day to treat, but in the meantime the facts so far as they have been ascertained are presented to geologists in the following list of shells for them to deal with as they please:—

Ostrea edulis	*Litorina obtusata*
Pinna rudis	*L. rudis*
Mytilus edulis	*L. litorea*
M. modiolus	*Turritella terebra*
Nucula nucleus	*Scalaria turtonæ*
Cardium echinatum	*Natica alderi*
C. edule	*Adeorbis subcarinatus*
C. norvegicum	*Cerithium reticulata*
Cyprina islandica	*Purpura lapillus*
Astarte sulcata	*Buccinum undatum*
Venus exoleta	*Murex erinaceus*
V. fasciata	*Trophon truncatus*
V. gallina	*Fusus gracilis*
Tellina balthica	*F. jeffreysianus*
Lutraria elliptica	*Nassa reticulata*
Mactra subtruncata	*N. incrassata*
Solen vagina	*Pleurotoma striolata*
Mya arenaria	*P. brachystoma*
Saxicava rugosa	*P. turricula*
	Cylichna cylindracea
Patella vulgata	
Trochus zizyphinus	42 species.
Lacuna puteolus	

The shells have been identified in odd lots and at different times by the late Mr. Gwyn Jeffreys, Mr. J. T. Marshall, and Mr. D. Pidgeon, to whom my warmest thanks have been due. The bulk of the work has, however, been done by the last-named gentleman, without whose hearty co-operation, both in searching the beach material and naming the shells and fragments found therein, the list would have been shorn of much of its goodly proportions. A. R. HUNT.

Torquay, December 28, 1887.

Vegetation and Moonlight.

THE letter of your Trinidad correspondent, given in NATURE, vol. xxxvi. p. 586, referring to a Committee appointed to determine moon influence, has a practical interest for me. Among the wood-cutters in Cape Colony, both east and west, there is a fixed belief, which no arguments can turn, that to cut timber at, or shortly after, full moon, is to cut it when the sap is up; and when, consequently, it is out of season. The same belief prevails in various parts of Southern India, notably in Travancore. I have always combated the belief, pending time and opportunity to test it, indulging in the provisional hypothesis that the bush-workers' belief may be due to the fact that they can only work by night at or near full moon; and that at night trees should contain more sap than by day, when watery exhalation is active.

It seems possible that in the habitually cloudless nights of certain countries the moon may exert influences not noticeable elsewhere. It is well known in Cape Colony that fish, pork, and other provisions go bad if left exposed to moonlight; though possibly this may be due to the light acting as a guide to insects. D. E. HUTCHINS,

Cape Colony, December 8, 1887. Conservator of Forests.

Centre of Water Pressure.

DR. ROUTH has done me the favour of pointing out that in the first volume of his "Rigid Dynamics" he has given the following very simple result with regard to the centre of pressure of a triangle occupying any position in a liquid:—"This point is the centre of gravity of three particles at the middle points of the sides, with masses proportional to their depths."

This result of Dr. Routh's is one of many very remarkable theorems of integration published by him in the *Quarterly Journal*, No. 83, 1886. GEORGE M. MINCHIN.

A New Magnetic Survey of France.

IT should not be difficult to do foreigners justice without belittling our own countrymen, and *a fortiori* without robbing any of the latter of their birthright.

In Prof. Thorpe's paper in last week's NATURE there occurs the sentence, "Even the surveys of their own country (France) have been made for them by Germans and Englishmen." This sentence taken in connection with the opening paragraph of the paper conveys the unfortunate impression that Von Lamont, the author of the "Untersuchungen über die Richtung und Stärke des Erdmagnetismus . . ." and of numerous other similar works, was a German, the truth being that he was merely a "Scot abroad" (see NATURE, vol. xx. p. 425). T. M.

Bothwell, Glasgow, January 14.

TIMBER, AND SOME OF ITS DISEASES.[1]

V.

IT has long been known that timber which has been felled, sawn up, and stored in wood-yards, is by no means necessarily beyond danger, but that either in the stacks, or even after it has been employed in building construction, it may suffer degeneration of a rapid character from the disease known generally as "dry-rot." The object of the present paper is to throw some light on the question of dry-rot, by summarizing the chief results of recent botanical inquiries into the nature and causes of the disease—or, rather, diseases, for it will be shown that there are several kinds of "dry-rot."

[1] Continued from p. 254.

The usual signs of the ordinary dry-rot of timber in buildings, especially deal-timber or fir-wood, are as follows. The wood becomes darker in colour, dull yellowish-brown instead of the paler tint of sound deal; its specific weight diminishes greatly, and that this is due to a loss of substance can be easily proved directly. These changes are accompanied with a cracking and warping of the wood, due to the shortening of the elements as water evaporates and they part from one another: if the disease affects one side of a beam or plank, these changes cause a pronounced warping or bending of the timber, and in bad cases it looks as if it had been burnt or scorched on the injured side. If the beam or plank is wet, the diseased parts are found to be so soft that they can easily be cut with a knife, almost like cheese; when dry, however, the touch of a hard instrument breaks it into brittle fibrous bits, easily crushed between the fingers to a yellow-brown, snuff-like powder. The timber has by this time lost its coherence, which, as we have seen, depends on the firm interlocking and holding together of the uninjured fibrous elements, and may give way under even light loads—a fact only too well known to builders and tenants. The walls of the wood-elements (tracheides, vessels, fibres,

FIG. 17.—Portion of the mycelium of *Merulius lacrymans* removed from the surface of a beam of wood. This cake-like mass spreads over the surface of the timber, to which it is intimately attached by hyphæ running in the wood-substance. Subsequently it develops the spore-bearing areolæ near its edges. The shading indicates differences in colour, as well as irregularities of surface.

or cells, according to the kind of timber, and the part affected) are now, in fact, reduced more or less to powder, and if such badly diseased timber is placed in water it rapidly absorbs it and sinks: the wood in this condition also readily condenses and absorbs moisture from damp air, a fact which we shall see has an important bearing on the progress of the disease itself.

If such a piece of badly diseased deal as I have shortly described is carefully examined, the observer is easily convinced that fungus filaments (mycelium) are present in the timber, and the microscope shows that the finer filaments of the mycelium (hyphæ) are permeating the rotting timber in all directions—running between and in the wood elements, and also on the surface, much as in the case shown in Fig. 17. In a vast number of cases, longer or shorter, broader or narrower, cords of grayish-white mycelium may be seen coursing on the surface and in the cracks: in course of time there will be observed flat cake-like masses of this mycelium, the hyphæ being woven into felt-like sheets, and these may be extending themselves on to neighbouring pieces of timber, or even on the brick-work or ground on which the timber is resting. These cord-like strands and cake-like masses of felt, with their innumerable fine filamentous continuations in the wood, constitute the vegetative body or mycelium of a fungus known as *Merulius lacrymans*. Under certain circumstances, often realized in cellars and houses, the cakes of mycelium are observed to develop the fructification of the fungus, illustrated in Fig. 18.

To understand the structure of this fructification we may contrast it with that of the *Polyporus* or *Trametes* referred to in the last article; where in the latter we find a number of pores leading each into a tubular cavity lined with the cells which produce the spores, the *Merulius* shows a number of shallow depressions lined by the sporogenous cells. The ridges which separate these depressed areolæ have a more or less zigzag course, running together, and sometimes the whole presents a likeness to

FIG. 18.—Mature fructification of *Merulius lacrymans*. The cake-like mass of felted mycelium has developed a series of areolæ (in the upper part of the figure), on the walls of which the spores are produced. In the natural position this spore-bearing layer is turned downwards, and in a moist environment pellucid drops or "tears" distil from it. The barren part in the foreground was on a wall, and the remainder on the lower side of a beam: the fungus was photographed in this position to show the structure.

honey-comb; if the ridges were higher, and regularly walled in the depressed areas, the structure would correspond to that of a *Polyporus* in essential points. The spores are produced in enormous numbers on this areolated surface, which is directed downwards, and is usually golden-brown, but may be dull in colour, and presents the remarkable phenomenon of exuding drops of clear water, like tears, whence the name *lacrymans*. In well-grown specimens, such as may sometimes be observed on the roof of a cellar, these crystal-like tears hang from the areolated surface like pendants, and give an extraordinarily beautiful appearance to the whole; the substance of the glistening *Merulius* may then be like shot-velvet gleaming with bright tints of yellow, orange, and even purple.

It has now been demonstrated by actual experiment that the spores of the fungus, *Merulius lacrymans*, will

germinate on the surface of damp timber, and send their germinal filaments into the tracheids, boring through the cell-walls, and extending rapidly in all directions. The fungus mycelium, as it gains in strength by feeding upon the substance of these cell-walls, destroys the wood by a process very similar to that already described (compare Fig. 14, Article III.).

It appears, however, from the investigations of Poleck and Hartig, that certain conditions are absolutely necessary for the development of the mycelium and its spread in the timber, and there can be no question that the intelligent application of the knowledge furnished by the scientific elucidation of the biology of the fungus is the key to successful treatment of the disease. This is, of course, true of all the diseases of timber, so far as they can be dealt with at all, but it comes out so distinctly in the present case that it will be well to examine a little at length some of the chief conclusions.

Merulius, like all fungi, consists of relatively large quantities of water—50 to 60 per cent. of its weight at least—together with much smaller quantities of nitrogenous and fatty substances and cellulose, and minute but absolutely essential traces of mineral matters, the chief of which are potassium and phosphorus. It is not necessary to dwell at length on the exact quantities of these matters found by analysis, nor to mention a few other bodies of which traces exist in such fungi. The point just now is that all these materials are formed by the fungus at the expense of the substance of the wood, and for a long time there was considerable difficulty in understanding how this could come about.

The first difficulty was that although the "dry-rot fungus" could always be found, and the mycelium was easily transferred from a piece of diseased wood to a piece of healthy wood provided they were in a suitable warm, damp, still atmosphere, no one had as yet succeeded in causing the spores of the *Merulius* to germinate, or in following the earliest stages of the disease. Up to about the end of the year 1884 it was known that the spores refused to germinate either in water or in decoctions of fruit; and repeated trials were made, but in vain, to see them actually germinate on damp wood, until two observers, Poleck and Hartig, discovered about the same time the necessary conditions for germination. It should be noted here that this difficulty in persuading spores to germinate is by no means an isolated instance: we are still ignorant of the conditions necessary for the germination of the spores of many fungi—*e.g.* the spores of the mushroom, according to De Bary; and it is known that in numerous cases spores need very peculiar treatment before they will germinate. The peculiarity in the case of the spores of *Merulius lacrymans* was found by Hartig to be the necessity of the presence of an alkali, such as ammonia; and it is found that in cellars, stables, and other outhouses where ammoniacal or alkaline emanations from the soil or elsewhere can reach the timber, there is a particularly favourable circumstance afforded for the germination of the spores. The other conditions are provided by a warm, still, damp atmosphere, such as exists in badly ventilated cellars, and corners, and beneath the flooring of many buildings.

Careful experiments have shown beyond all question that the "dry-rot fungus" is no exception to other fungi with respect to moisture: thoroughly dry timber, so long as it is kept thoroughly dry, is proof against the disease we are considering. Nay, more, the fungus is peculiarly susceptible to drought, and the mycelial threads and even the young fructifications growing on the surface of a beam of timber in a damp close situation may be readily killed in a day or two by letting in thoroughly dry air: of course, the mycelium deeper down in the wood is not so easily and quickly destroyed, since not only is it more protected, but the mycelial strands are able to transport moisture from a distance. Much misunderstanding prevails as to the meaning of "dry air" and "dry wood": as a matter of fact, the air usually contains much moisture, especially in cellars and quiet corners devoid of draughts, such as *Merulius* delights in, and we have already seen how dry timber rapidly absorbs moisture from such air. Moreover, the strands of mycelium may extend into damp soil, foundations, brick-work, &c.; in such cases they convey moisture to parts growing in apparently dry situations.

A large series of comparative experiments, made especially by Hartig, have fully established the correctness of the conclusion that damp foundations, walls, &c., encourage the spread of dry-rot, quite independently of the quality of the timber. This is important, because it has long been supposed that timber felled in summer was more prone to dry-rot than timber felled in winter: such, however, is not shown to be the case, for under the same conditions both summer- and winter-wood suffer alike, and decrease in weight to the same extent during the progress of the disease. There is an excellent opportunity for further research here however, since one observer maintains that in one case at any rate (*Pinus sylvestris*) the timber felled at the end of April suffered from the disease, whereas that felled in winter resisted the attacks of the fungus: internal evidence in the published account supports the suspicion that some error occurred here. The wood which succumbed was found to contain much larger quantities of potassium and phosphorus (two important ingredients for the fungus), and Poleck suggests that this difference in chemical constitution explains the ease with which his April specimens were infected.

It appears probable from later researches and criticism that Poleck did not choose the same parts of the two stems selected for his experiments, for (in the case of *Pinus sylvestris*) the heart-wood is attacked much less energetically than the sap-wood—a circumstance which certainly may explain the questionable results if the chemist paid no attention to it, but analyzed the sap-wood of one and the heart-wood of the other piece of timber, as he seems to have done.

The best knowledge to hand seems to be that no difference is observable in the susceptibility to dry-rot of winter-wood and summer-wood of the same timber; *i.e. Merulius lacrymans* will attack both equally, if other conditions are the same.

But air-dry and thoroughly seasoned timber is much less easily attacked than damp fresh-cut wood of the same kind, both being exposed to the same conditions.

Moreover, different timbers are attacked and destroyed in different degrees. The heart-wood of the pine is more resistant than any spruce timber. Experimental observations are wanted on the comparative resistance of oak, beech, and other timbers, and indeed the whole question is well worth further investigation.

When the spore has germinated, and the fungus hyphæ have begun to grow and branch in the moist timber, they proceed at once to destroy and feed upon the contents of the medullary rays; the cells composing these contain starch and saccharine matters, nitrogenous substances, and inorganic elements, such as potassium, phosphorus, calcium, &c. Unless there is any very new and young wood present, this is the only considerable source of proteid substances that the fungus has: no doubt a little may be obtained from the resin-passages, but only the younger ones. In accordance with this a curious fact was discovered by Hartig: the older parts of the hyphæ pass their protoplasmic contents on to the younger growing portions, and so economize the nitrogenous substances. Other food-substances are not so sparse; the lignified walls inclose water and air, and contain mineral salts, and such organic substances as coniferin, tannin, &c., and some of these are absorbed and employed by the fungus. Coniferin especially appears to be destroyed by the hyphæ.

The structure of the walls of the tracheides and cells of the wood is completely destroyed as the fungus hyphæ extract the minerals, cellulose, and other substances from them. The minerals are absorbed at points of contact between the hyphæ and the walls, reminding us of the action of roots on a marble plate: the coniferin and other organic substances are no doubt first rendered soluble by a ferment, and then absorbed by the hyphæ. This excretion of ferment has nothing to do with the excretion of water in the liquid state, which gives the fungus its specific name: the "tears" themselves have no solvent action on wood.

It will be evident from what has been stated that the practical application of botanical knowledge is here not only possible, but much easier than is the case in dealing with many other diseases.

It must first be borne in mind that this fungus spreads, like so many others, by means of both spores and mycelium: it is easy to see strands of mycelium passing from badly-diseased planks or beams, &c., across intervening brick-work or soil, and on to sound timber, which it then infects. The spores are developed in countless myriads from the fructifications described, and they are extremely minute and light: it has been proved that they can be carried from house to house on the clothes and tools, &c., of workmen, who in their ignorance of the facts are perfectly careless about laying their coats, implements, &c., on piles of the diseased timber intended for removal. Again, in replacing beams, &c., attacked with dry-rot, with sound timber, the utmost ignorance and carelessness are shown: broken pieces of the diseased timber are left about, whether with spores on or not; and I have myself seen quite lately sound planks laid close upon and nailed to planks attacked with the "rot." Hartig proved that the spores can be carried from the wood of one building to that of another by means of the saws of workmen.

But perhaps the most reckless of all practices is the usage of partially diseased timber for other constructive purposes, and stacking it meanwhile in a yard or outbuilding in the neighbourhood of fresh-cut, unseasoned timber. It is obvious that the diseased timber should be removed as quickly as possible, and burnt at once: if used as firewood in the ordinary way, it is at the risk of those concerned. Of course the great danger consists in the presence of many ripe spores, and their being scattered on timber which is under proper conditions for their germination and the spread of the mycelium.

It is clearly an act approaching those of a madman to use fresh "green" timber for building purposes; but it seems certain that much improperly dried and by no means "seasoned" timber is employed in some modern houses. Such wood is peculiarly exposed to the attacks of any spores or mycelium that may be near.

But even when the beams, door-posts, window-sashes, &c., in a house are made of properly dried and seasoned deal, the danger is not averted if they are supported on damp walls or floors. For the sake of illustration I will take an extreme case, though I have no doubt it has been realized at various times. Beams of thoroughly seasoned deal are cut with a saw which has previously been used for cutting up diseased timber, and a few spores of *Merulius* are rubbed off from the saw, and left sticking to one end of the cut beam: this end is then laid on or in a brick wall, or foundation, which has only stood long enough to partially dry. If there is no current of dry air established through this part, nothing is more probable than that the spores will germinate, and the mycelium spread, and in the course of time—it may be months afterwards—a mysterious outbreak of dry-rot ensues. There can be no question that the ends of beams in new houses are peculiarly exposed to the attacks of dry-rot in this way.

The great safeguard—beyond taking care that no spores or mycelium are present from the first—is to arrange that all the brick-work, floors, &c., be thoroughly dry before the timber is put in contact with them; or to interpose some impervious substance—a less trustworthy method. Then it is necessary to aërate and ventilate the timber; for *dry* timber *kept dry* is proof against "dry-rot."

The ventilation must be real and thorough however, for it has been by no means an uncommon experience to find window-sashes, door-posts, &c., in damp buildings, with the insides scooped out by dry-rot, and the aërated outer shells of the timber quite sound: this is undoubtedly often due to the paint on the outer surfaces preventing a thorough drying of the deeper parts of the wood.

Of course the question arises, and is loudly urged, Is there no medium which will act as an antiseptic, and kill the mycelium in the timber in the earlier stages of the disease? The answer is, that mineral poisons will at once kill the mycelium on contact, and that creosote, &c., will do the same; but who will take the trouble to thoroughly impregnate timber in buildings such as harbour dry-rot? And it is simply useless to merely paint these specifics on the surface of the timber: they soak in a little way, and kill the mycelium on the outside, but that is all, and the deadly rot goes on destroying the inner parts of the timber just as surely.

There is one practical suggestion in this connection, however; in cases where properly seasoned timber is used, the beams laid in the brick walls might have their ends creosoted, and if thoroughly done this would probably be efficacious during the dangerous period while the walls finished drying. I believe this idea has been carried out lately by Prof. Hartig, who told me of it. The same observer was also kind enough to show me some of his experiments with dry-rot and antiseptics: he dug up and examined in my presence glass jars containing each two pieces of deal—one piece sound, and the other diseased. The sound pieces had been treated with various antiseptics, and then tied face to face with the diseased pieces, and buried in the jar for many months or even two years.

However, I must now leave this part of the subject, referring the reader to Hartig's classical publications for further information, and pass on to a sketch of what is known of other kinds of "dry-rot." It is a remarkable fact, and well known, that *Merulius lacrymans* is a domestic fungus, peculiar to dwelling-houses and other buildings, and not found in the forest. We may avoid the discussion as to whether or no it has ever been found wild: one case, it is true, is on record on good authority, but the striking peculiarity about it is that, like some other organisms, this fungus has become intimately associated with mankind and human dwellings, &c.

The case is very different with the next disease-producing fungus I propose to consider. It frequently happens that timber which has been stacked for some time in the wood-yards shows red or brown streaks, where the substance of the timber is softer, and in fact may be "rotten": after passing through the saw-mill these streaks of bad wood seriously impair the value of the planks, beams, &c., cut from the logs

Prof. Hartig, who has devoted much time to the investigation of the various forms of "dry-rot," informs me that this particular kind of red or brown streaking is due to the ravages of *Polyporus vaporarius*. The mycelium of this fungus destroys the structure of the wood in a manner so similar to that of the *Merulius* that the sawyers and others do not readily distinguish between the two. The mycelium of *Polyporus vaporarius* forms thick ribbons and strands, but they are snowy white, and not gray like those of *Merulius lacrymans*: the structure, &c., of the fructification are also different. I have shown in Fig. 19 a piece of wood undergoing destruction from the action of the mycelium of this *Polyporus*, and it will be seen how the diseased timber cracks just as under the influence of *Merulius*.

Now *Polyporus vaporarius* is common in the forests,

and Hartig has found that its spores may lodge in cracks in the barked logs of timber lying on the ground—cracks such as those in Fig. 1 (see p. 182). In the particular forests of which the following story is told, the felling is accomplished in May (because the trunks can then be readily barked, and also because such work cannot be carried on there in the winter), and the logs remain exposed to the sun and rain, and vicissitudes of weather generally, for some time. Now it is easy to see that rain may easily wash spores into such cracks as those referred to, and the fungus obtains its hold of the timber in this way.

The next stage is sending the timber down to the timber-yards, and this is accomplished, in the districts referred to, by floating the logs down the river. Once in the river, the wood swells, and the cracks close up; but the fungus spores are already deeply imprisoned in the cracks, and have no doubt by this time emitted their germinal hyphæ, and commenced to form the mycelium. This may or may not be the case: the important point is simply hat the fungus is already there. Having arrived at the

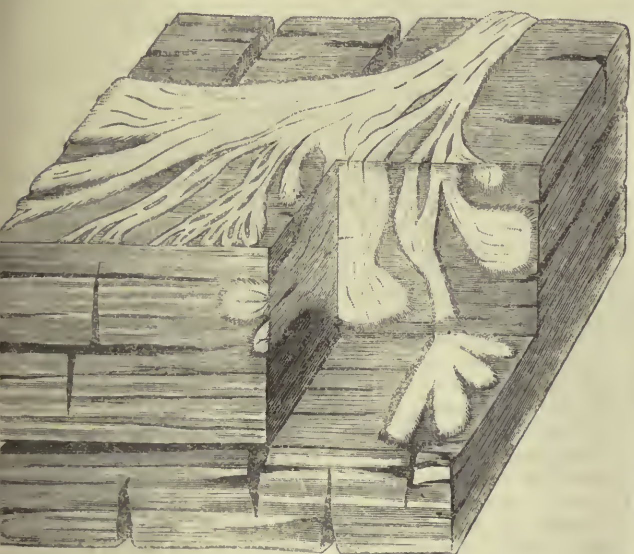


Fig. 19.—A piece of pine-wood attacked by the mycelium of *Polyporus vaporarius*. The timber has warped and cracked under the action of the fungus, becoming of a warm brown colour at the same time; in the crevices the white strands of felt-like mycelium have then increased, and on splitting the diseased timber they are found creeping and applying themselves to all the surfaces. Except that the colour is snowy white, instead of gray, this mycelium may easily be mistaken for that of *Merulius*. The fructification which it develops is, however, very different. (After R. Hartig)

timber-wharves, the logs are stacked for sawing in heaps as big as houses: after a time the sawing up begins. It usually happens that the uppermost logs when cut up show little or no signs of rot; lower down, however, red and brown streaks appear in the planks, and when the lowermost logs are reached, perhaps after some weeks or months, deep channels of powdery, rotten wood are found, running up inside the logs in such a way that their transverse sections often form triangles or V-shaped figures, with the apex of the triangle or V turned towards the periphery of the log.

The explanation is simple. The uppermost logs on the stack have dried sufficiently to arrest the progress of the mycelium, and therefore of the disease: the lower logs, however, kept damp and warm by those above, have offered every chance to the formation and spread of the mycelium deep down in the cracks of the timber. I was much impressed with this ingenious explanation, given to me personally by Prof. Hartig, and illustrated by actual specimens. It will be noticed how fully it explains the curious shape of the rotten courses, because the depths of the cracks are first diseased, and the mycelium spreads thence.

Obviously some protection would be afforded if the bark could be retained on the felled logs, or if they could be at once covered and kept covered after barking; and, again, something towards protection might be done by carting instead of floating the timber, when possible. At the same time, this is not a reliable mode of avoiding the disease by itself; and even the dry top logs in the saw-yard are not safe. Suppose the following case. The top logs of the stack are quite dry, and are cut into beams and used in building; but they have spores or young mycelium trapped in the cracks at various places. If, from contact with damp brick-work or other sources of moisture, these spores or mycelia are enabled to spread subsequently, we may have "dry-rot" in the building; but this "dry-rot" is due to *Polyporus vaporarius*, and not to the well-known *Merulius lacrymans*.

There can probably be no question of the advantage of creosoting the ends of such rafters, beams, &c.; since the creosote will act long enough to enable the timber to dry, if it is ever to dry at all. But the mycelium of *Polyporus vaporarius* makes its way into the still standing timber of pines and firs; for it is a wound-parasite, and its mycelium can obtain a hold at places which have been injured by the bites of animals, &c.: it thus happens that this form of "dry-rot" is an extremely dangerous and insidious one, and I have little doubt that it costs our English timber-merchants something, as well as Continental ones. Nor are the above the only kinds of "dry-rot" we know. Hartig has described a disease of pine-wood caused by *Polyporus mollis*, which is very similar to the last in many respects, and the suspicion may well gain ground that this important subject has by no means been exhausted yet.

H. MARSHALL WARD.

SCIENCE IN ELEMENTARY SCHOOLS.[1]

NOTHING could be more unsatisfactory than the present position of the knowledge and teaching of science in our elementary schools. Notwithstanding all the advantages that have been offered to pupil-teachers for the study of science, as a body they appear to be in a most deplorable state in this respect. Though success in the examinations of the Science and Art Department are now taken into account in placing the students of the training colleges for their teaching certificates, and average school-boys when they have been fairly taught are quite competent for these examinations, yet very few of the teachers have availed themselves of this privilege, and it does not appear that the training colleges have helped them in this respect. Very little, indeed, can be expected while the ordinary pupil-teacher is described, as he is in Mr. Oakeley's report on the working of the Training Colleges, as deficient in many elementary branches, notably mathematics. It is satisfactory, however, to notice that the quality of the candidates for admission to the Training Colleges is improving, and that these institutions are growing in teaching capacity and in popularity. The reports of the examiners for admission are not, with regard to the subject in hand, pleasant reading. One cannot expect good answering in science from candidates who are quite unable to paraphrase an ordinary piece of poetry, or to explain a common English expression. Accordingly we find that in Euclid, algebra, and mensuration, though a few papers were especially meritorious, the vast majority of the answers were very inferior. Few, if any, attempted the easy riders in Euclid, and the examiner remarks that he fears that the pupil-teachers receive but little assistance

[1] "Report of Committee of Council on Education (England and Wales), 1886–87."

from those who superintend their work. It is not easy to say whether this poor teaching or defective early training is at the root of the evil. It is worthy of remark that the metropolitan candidates, in their answers to the questions on Euclid, far surpass their provincial competitors. Many amazing blunders are quite common in the algebra papers, such as subtracting the terms of the numerator from those of the denominator, and completely ignoring the signs, and it is stated that the pupil-teachers at Chester at the end of their apprenticeship were unable to work a simple sum in algebra or to write out an easy proposition Mr. Fitch has a very able report on the Training Colleges for schoolmistresses, and from him it is plain that the same defects exist among the female as among the male pupil-teachers. At the admission examination the work in the arithmetic is satisfactory in point of accuracy, but it displays want of method, failure to appreciate the meaning of the question asked, and ignorance of principles. Thus very few of the candidates were able to give an intelligent explanation of simple arithmetical processes, such as subtraction or division. With them, as with the male pupil-teachers, book-work and memory are wholly relied on, and little attention is paid to the intelligent application of principles. "Scarcely three per cent. are able to do much more in the teaching of arithmetic than work sums more or less correctly on the black-board."

With such material to work on, it is not surprising that the results of the work at the colleges are not what they otherwise might be. Those who are below the average at admission rarely succeed very well in the array of subjects to be learnt in two years' training. With regard to the male students the reports at the close of the first year's training record that the answering of the questions set on the first book of Euclid was disappointing. The students appear to have learnt their propositions by rote, and to have displayed great want of neatness and accuracy. Though the riders were joined to the propositions on which their solution depended, and though all these riders were easy, very few of the papers were satisfactory. This inability to solve the easiest geometrical deductions is commented on again and again, and proves beyond doubt that either the students are negligently taught, or that they commit the book-work to memory without understanding it, and consequently are quite incapable of applying their knowledge to solve the simplest riders. The report for the second year is rather better; few candidates answered very well, and few answered badly, and the majority made a fair percentage of marks; but the same inability to apply their knowledge to the solution of easy deductions in Euclid is recorded. With regard to the answering in algebra and mensuration, there is nothing noticeable except that some students show a discreditable ignorance of the most fundamental questions, while the papers were generally satisfactory.

Summing up the results of the working of our male Training Colleges, Mr. Oakeley gives it as his opinion that the students are over-lectured at some of the colleges, and that the lectures are mechanically reproduced, and transferred as closely as possible to the examination papers. This, of course, is due to the defective early training of the students, and to lectures injudiciously delivered on subjects about which students know absolutely nothing. For instance, one lecturer delivered a very excellent discourse on the corrupt form of Latin used by the Roman soldiers in Britain, its causes and its effects, to a class of which few, if any, of the members knew anything whatever of Latin. Mr. Oakeley also points out one of the greatest defects in the present system of training pupil-teachers when he says that as a rule pupil-teachers see but one school at work; they have no opportunity of comparing the mode of teaching in other schools. This is, however, obviated at Homerton, and partly at Durham, by visiting neighbouring schools during school-hours.

The reports of the examiners on the progress made by the students of the female Training Colleges tell us that in arithmetic, questions on theory and principles are not well done; long problems are inaccurately done, and, as a whole, it is seen that there is yet much remains before it can be said that our present system is satisfactory as regards the knowledge given and the methods adopted. There appears to be among the students a very narrow view of their future work, a desire to regard the obtaining of their certificates as the goal and aim of their existence. The views on science of one of these maidens is worth recording:—"If I am successful in obtaining my certificate, I intend (D.V.) going in for two sciences. At the same time I propose attending a tonic sol-fa class to get my advanced certificate. Should the two sciences 'sound, light, and heat,' and 'electricity and magnetism' prove a success, I shall probably take up the science of hygiene." If the Training Colleges tend to remove the impression that the technical qualification is the end of the pupil-teacher's work, if they awaken a spirit of emulation among the students, aud enable them to teach more thoroughly and intelligently, then they will have fulfilled a large portion of their duties.

This being the stuff of which our elementary teachers are made, let us now glance at the reports of the work done in the schools under their guidance. With masters the majority of whom know little or nothing of even the elements of science the pupils cannot be expected to pass well in these subjects. Thus it is seen in the return of the number of pupils sent up on "specific subjects" (most of which are scientific), that only 16·51 of those eligible for examination have been so examined, and of these nearly one-half were from the London School Board District. One-half of the passes were in algebra and animal physiology. By a new arrangement the ten chief inspectors present biennial reports, five each year, and in the present volume the five divisions reported on are: the North-Eastern, the North Central, the Eastern, the South-Western, and Wales. All these agree that, with the exception of some of the cities and large towns, throughout the elementary schools science is untaught. This we can well imagine, when we have seen that the average teacher is completely ignorant of any of its branches, and it is the average teacher who is sent to the country schools. The explanation of some of the inspectors, that in the country for a great portion of the year the attendance of the children who are fit to be taught these subjects is very irregular, does not meet the question; for, even were the children most regular in their attendance, the subjects could not at present be taught, and until our average elementary teacher is altered they will not be taught. Following the individual reports on the subject, we find in the North-Eastern Division that arithmetic is accurately but unintelligently studied. So utterly mechanical is the teaching that in many schools mental arithmetic is regarded as a separate subject, and not as the adjunct and preliminary of all arithmetic. Having seen the complaints made by the examiners of the quality of the study of the pupil-teachers, it could only be expected that the same defects would show themselves in the scholars under their charge. Elementary science is unknown in the North-Eastern schools, except in Leeds, Sheffield, Bradford, Newcastle, and Sunderland, where algebra and animal physiology are taken up with fair results. But the inspector remarks that physiology is seldom so taught as to be of any practical benefit, and in the teaching of algebra there is a great want of thoroughness. In the North-Central Division, specific subjects are seldom taken; and about one-half the pupils sent up for examination on them passed. These subjects, taking them in the order of the number of pupils sent up on each, are algebra, magnetism and electricity, physiology, agriculture, and mechanics. In this division "arithmetic is always the most unsatisfactory

subject we have to deal with." The teaching of it is dull and mechanical, and the rules are rarely intelligently applied. In this large district there is one bright spot, which shows what can be done by ordinary industry and skill. It is the town of Nottingham, in which 2526 children were examined in specific subjects, of whom four-fifths passed. "Mechanics for boys and domestic economy for girls, are the subjects principally taken by the Nottingham Board Schools, and are taught by a specially qualified science demonstrator and assistant, who visit the various schools in turns, bringing the apparatus with them in a specially constructed hand-cart. The lectures given on these occasions are afterwards gone through again by the teachers of the schools, from notes taken at the time. These lectures are simple and interesting, and are given with great care and skill; the results are remarkably good, both as regards the actual knowledge acquired by the scholars and the stimulus given to the general intelligence. Besides the above-named subjects, physiology and algebra are often taken with very good results, and in one school the principles of agriculture are taught with marked success." This extract from Mr. Blandford's report shows that the neglect of elementary science is due, not to the dullness or irregularity of the pupils, as some of the inspectors would seem to imply, but frequently to the ignorance and incapacity of the teachers. In the Eastern Division specific subjects are rarely taken, but in the Norwich district mechanics, chemistry, and botany have been taught satisfactorily in one or more schools, and "are distinctly a gain to the boys." On the whole, however, this division is rather at a standstill. The quality of the education given has not risen as one would expect, and with regard to scientific and technical education, in the words of Mr. Synge, "there is plenty of room and need for progress in the immediate future, but at the present moment too little sign of its beginning." In the South-Western Division "elementary science has hardly any existence." In fact, except in some of the large towns, it is practically non-existent, and in the whole division, there were only about 600 children presented on specific subjects. In Wales, except in a few higher-grade schools, the teaching of science is unknown.

Some of the causes of this almost total absence of any scientific teaching in our elementary schools have been pointed out. Where science has been well taught it has borne good fruit, and where teachers and managers have set themselves steadfastly to overcome the difficulties in their way a high and encouraging measure of success has been obtained. Thus we have the remarkable testimony of the success of the experiment in Nottingham, and surely there are many other districts in England quite as competent to carry on this work as Nottingham. Why it could not be done in any town in England, it is difficult to see. In many cases where these subjects have been taught, the inspectors have wisely set their faces against them, finding but a wretched smattering amongst the pupils. Nothing else can be expected in remote rural districts, where the teacher, whose whole time is scarcely sufficient for the few rudimentary subjects, is so ambitious as to attempt to cram some of his pupils with the elementary knowledge of a science of which he is himself confessedly ignorant. But in our towns and cities competent teachers are always to be had. If the Board masters do not find themselves fit for the extra labour and extra knowledge required, there should be no difficulty in obtaining a specialist, as has been done at Nottingham. And in no place could the foundations of technical education be more surely laid than amongst the elder children of our elementary schools. In the Minutes and Instructions issued to Her Majesty's Inspectors, managers are requested to aid in every way they can the teaching of one or more specific subjects appropriate to the industrial or other needs of the locality, and the rudiments of two higher subjects to supply a foundation for future work. With this object it is suggested that where the teacher is not competent to do so—and this, according to the reports, is the rule, and not the exception—a specialist might be employed by a number of schools in a district, whose instruction would be supplemented by that of the ordinary teachers. There is only one instance, that of Nottingham, given in the reports of such suggestions having been followed.

Geography.—Where there is "a great absence of culture and general intelligence upon the part of a considerable number of the candidates," it is not surprising to find that, though the answers to the geography papers for admission to the male training colleges were fairly accurate, they were not intelligent. Here, again, the metropolitan candidates are superior to the provincial candidates, particularly in the map-drawing, though, in this particular, there has been a falling away of late. Amongst the female candidates, the geography was not very satisfactory, exhibiting inaccuracies in map-drawing, indefiniteness in the answers, and, generally, marks of defective early training. In the examinations for the first year's certificates the male candidates answered fully and accurately; but usually there was a slavish following of the words of the text-books and the lecturers' notes. At the end of the second year, there is the same report, book-knowledge without intelligence, and abundance of information imperfectly digested. With the females, the result is the same: verbatim reproduction of the books or notes they had read; fairly creditable answering; but "the style of the papers reveals the painful poverty of the general reading of the students, and the utter absence of any individuality, or attempt at description in their own words." In many papers there was a constant iteration of the same words and phrases, suggesting that the candidates had learned off by rote the answers to probable questions. With regard to the elementary schools, all the reports agree in saying that there has been a marked improvement in the teaching of geography. Where it is intelligently taught it is the favourite subject; but too frequently the children are not well grounded. While all divisions report progress in this subject, it is worthy of remark that all the maritime districts, and particularly those of the South-Western Division, including the counties of Hampshire, Dorset, Devon, Cornwall, and Somerset, surpass the inland schools in the knowledge of our country, its colonies, and its trade. And this is only natural. The teacher who would not, in Devonshire, interest a class of boys in the voyages of Drake, or who, in Somerset, would not rivet the attention of his pupils on the victories of Blake, would not be worthy of his post. Though the teachers may be congratulated, speaking generally, on the progress made in geography, there are many faults to be found. In portions of Wales and of the centre of England, geography is only fairly satisfactory. The pupils are weak in questions of latitude and longitude; they do not learn intelligently; because, most probably, they are taught mechanically and unintelligently. It should be within the power of every teacher by the use of an ordinary globe to make this portion of the subject intelligible to any ordinary boy; but few lads could understand a lesson on meridians and parallels, given by a teacher who does not use a globe at all. And yet this is quite common! Hence it is that the map-drawing is very poor, even where there is a good knowledge of geographical facts. Many of the inspectors complain of lack of globes, maps, &c.; and even where there is abundance of general maps, there are no local maps, a want which is very widely felt. In this respect our Board of Education might take a lesson from the Commissioners of National Education in Ireland, who have published local maps, and require each pupil in the higher grades to know, in addition to general geography, the map of his neighbourhood.

NOTES.

LAST week we printed an article advocating the claims of M. Giard to the new Chair of Darwinism in Paris. We are now informed that the appointment will certainly be offered to M. Giard, and that, if he declines it, it will be offered to Prof. Perrier, of the Paris Museum of Natural History. It is generally desired that the Chair should be connected with the Faculty of Sciences in the Sorbonne. M. Liard, the Director of Superior Instruction, in the Department of Public Instruction, is favourable to the whole scheme, and hopes are expressed that the lectures may be begun in the course of two or three months.

MR. G. J. ROMANES has been elected Fullerian Professor of Physiology at the Royal Institution. He intends to devote all the three years of his professorship to one continuous course of lectures on "Before and after Darwin." This year's course—"Before Darwin"—will be an historical survey of the progress of scientific thought and discovery in biology from the earliest times till the publication of "The Origin of Species." Next year's course will be "On the Evidence of Organic Evolution," and the third year's "On the Factors of Organic Evolution."

ON Saturday, the 21st inst., at three o'clock, Lord Rayleigh will deliver, at the Royal Institution, the first of a course of seven lectures on Experimental Optics. The remaining lectures of the course will be given at the same hour on the following Saturdays.

THE annual general meeting of the Anthropological Institute of Great Britain and Ireland will take place on Tuesday, the 24th inst., at 8 o'clock p.m. precisely, Mr. Francis Galton, F.R.S., President, in the chair. The following will be the order of business:—Confirmation of the minutes; appointment of scrutineers of the ballot; Treasurer's financial statement; report of Council for 1887; the Presidential Address; report of scrutineers; and election of Council for 1888.

DR. H. LLOYD SNAPE has been elected to fill the Chair of Chemistry at University College, Aberystwith, rendered vacant by the death of Prof. Humpidge. The new Professor acted for three sessions as Demonstrator of Chemistry at University College, Liverpool. Afterwards he studied at the Universities of Berlin and Göttingen under the direction of Profs. Hofmann and V. Meyer respectively. On his return to England he was appointed Director of the Department of Pure and Applied Chemistry in the Manchester Technical School.

WE regret to announce the death of Mr. Hayden, the well-known American geologist. He died on December 22 last.

MR. ANDREW GARRETT, an eminent American conchologist, died at his residence on the island of Huahine, Society Group, South Seas, on November 1 last, in his sixty-fifth year.

THE Meteorological Council has recently published Part III. of the Daily Synchronous Weather Charts of the North Atlantic and the adjacent continents. Parts I. and II. were respectively noticed in NATURE, vol. xxxv. p. 469, and vol. xxxvi. p. 178. The part just issued, dealing with the period from February 15 to May 24, 1883, comprises the weather for the end of the winter and the early spring. The charts show clearly how very different are the conditions which exist over the Atlantic in the winter from those which exist in the spring. The early charts contain numerous instances of storms, and the barometrical disturbances which accompany them embrace a large part of the North Atlantic Ocean. An interesting case of storm development is shown over the American Lakes on February 19. The disturbance subsequently traversed the Atlantic, and passed about 800 miles to the north of Scotland on the 24th, causing a moderate gale in the north of our islands, and gales generally over the north-west of Europe. There is another instance of rapid storm development off Florida on April 2, the disturbance growing into a severe hurricane when south of Newfoundland on the 4th. A storm-area was formed off the north-west coast of Africa on February 20. This seems to have originated in an intensifying of the northerly wind on the eastern side of the Atlantic anticyclone. On the 22nd the storm was fully developed, and the cyclonic circulation was complete, the barometer registering as low as 29·6 inches. This disturbance travelled to the westward as far as the middle of the Atlantic. On the 25th it was clearly dying out, but on the 26th it apparently gained fresh life, and on February 28 and March 1 it was causing a gale close to the American coast, and to the south of Newfoundland and Nova Scotia. It afterwards travelled eastwards, skirting to the north-west of Iceland on March 3, and finally struck the north-west coast of Norway on the 4th. There is also a case of a double-headed depression travelling to the eastward across the Atlantic between March 25 and 31. These charts show very clearly the explanation of the cold northerly and north-easterly winds experienced over England during the spring, the isobaric lines indicating a general extension of the Atlantic high pressure over our islands at this season.

AT the meeting of the French Meteorological Society on the 6th ult. a paper by M. André, Director of the Lyons Observatory, on the influence of altitude on temperature, was read. The observations were made in the environs of Lyons in the years 1881-84. The mean diurnal range was 18°·5 F. at the lowest station (574 feet), and 12°·3 at the highest station (2050 feet). M. Poincaré submitted a table showing the relation between the barometric movements at lat. 40° and 10° N., and the phases of the moon. M. Renon made a communication on the observation of fog. He considered the present method of observation to be defective, as the observer could only note what exists around him; a knowledge of the conditions at a considerable height above him was necessary, to arrive at satisfactory conclusions. This desideratum was also urged by M. Janssen.

ON January 3, snow fell in Christiania from a perfectly clear sky. After a strong southerly wind with cloudy weather in the morning the weather cleared, but at about noon it again thickened, and snow and sleet fell. In the afternoon the sky again became clear and continued thus, with a storm blowing from the west. Just before 8 p.m., however, thick clouds again gathered, the full moon became obscured, and snow began to fall heavily. A quarter of an hour later the wind swept the clouds away, and the sky became completely clear, with the exception of a few clouds in the east. The stars shone brightly, and the full moon illuminated the landscape; still snow continued to fall thickly for some ten minutes. That the snow could not have originated with the clouds in the east is proved by the circumstance that the wind was westerly. A well-known meteorologist ascribes the phenomenon to the presence at a certain elevation in the atmosphere of a very cold layer of air in which the ascending, comparatively warmer, air became condensed, the moisture being thrown out in the form of snow, but not in sufficient quantities to obscure the blue sky, the stars, and the moon. The great chilling of the layer of air referred to may have been caused by the coldness of the heavy snow clouds which a few minutes previously filled the atmosphere.

ON December 24, at 9.45 p.m., a brilliant meteor was observed in the north-western sky at Örebro, in Central Sweden. The light, variegated in colour, was very intense. The meteor seemed to fall perpendicularly to the earth with a slow motion, and dissolved itself without any report. On December 25, at about 5 p.m., another meteor, shining with an intense bluish-

white light, was seen going in a north-westerly direction at Karlskogo, also in Central Sweden.

ANOTHER important paper upon the synthesis of glucose is communicated by Drs. Emil Fischer and Tafel to the current number of the *Berichte*. They have succeeded in artificially preparing glucose directly from glycerine. It will be remembered that this synthesis was first effected by decomposition of acroleïn dibromide, CH_2Br . $CHBr$. CHO, with baryta water; glycerine aldehyde, CH_2OH . $CHOH$. CHO, being probably first formed, and afterwards polymerizing into glucose. Hence it might be expected that the same result could be achieved by direct oxidation of glycerine to aldehyde and subsequent condensation by means of alkalies. This supposition has been completely confirmed by experiment, and the new method is at once an easier and a cheaper one. A large quantity of glycerine was first oxidized by means of soda and bromine, the temperature being kept down to 10°. The bromine readily dissolved on shaking, and the evolution of carbonic acid gas soon rendered evident the progress of the change. After half an hour the reaction was found to be complete; the liquid was then acidified with hydrochloric acid and a current of sulphur dioxide passed through it until all the bromine was reduced. The liquid, after neutralization with soda, was found to contain a large quantity of glycerine aldehyde. About 1 per cent. more soda was then added, and the solution allowed to stand at a temperature of about 0° for four or five days. As the polymerization proceeded, the liquid gradually lost the power of reducing alkaline copper solutions in the cold, but, like sugar, rapidly reduced them on warming. In order to isolate the sugar thus formed, the phenylhydrazine compound was prepared, as in the former experiments, by neutralizing with acetic acid and adding phenylhydrazine and sodium acetate, heating six hours upon a water-bath. After some time crystals of the phenylhydrazine compound, $C_{18}H_{22}N_4O_4$, were deposited, and after purification were found to possess all the properties of the compound obtained from acroleïn dibromide; in fact, they were identical with it. This compound crystallizes in beautiful yellow needles, melting at 217°; on heating it with zinc dust and acetic acid, a base is obtained which, by the action of nitrous acid and subsequent neutralization with soda, yields, on evaporation, syrupy glucose itself. Not only does this later work of Drs. Fischer and Tafel confirm their former striking results, but it leaves the subject in a much more complete state, and furnishes chemists with a far readier method of preparing artificial glucose in the laboratory.

THE habits of a running spider of Southern Europe, *Tarantula narbonensis*, Latr., studied by Herr Beck, are curious. It makes a vertical round hole in the ground about 10 inches deep, and this, with a small earth-wall sometimes made round the mouth, is lined with web. A little way down is a small lateral hole, into which the spider shrinks when an animal falls into the tube; when the animal has reached the bottom the spider pounces on it. One can readily tell that a tube is tenanted, by the bright phosphorescent eyes of the spider turned upwards. In fight the spider erects itself on its last pair of legs, striking with the others. The bite is not fatal to man, but it causes large swellings. The children in Bucharest angle for these spiders by means of an egg-like ball of kneaded yellow wax tied to a thread. This is lowered with jerks into the hole, and the spider fastens on it and can be pulled out; whereupon another thread is passed round one of the legs, and the animal is played with.

LEMMINGS are very numerous in several valleys in Southern Norway this winter. In many places the snow is furrowed for miles by the march of these little animals on their migration southwards.

IN November last a Runic stone was found at Häggerstalund, in Sweden. A lady happened to notice a long stone in the proximity of a well-known Runic boulder, and having had it turned found that there was an inscription on the other side, which has been interpreted thus:—"Gårdar and Jorund raised these stones after (in memory of) their sister's sons, Ernmund and Ingemund." The newly-discovered stone is of importance, as it supplements the Runic inscription of the other stone, viz.:—"These memorial stones are made after (in memory of) the sons of Inga. She took heirloom after them, but these brothers (referred to on the other stone) will take heirloom after her. Gjad's (?) brothers; they died in Greece." The latter stone is made particularly interesting by the reference to the death of the two men in Greece.

WHILST digging for potatoes late last autumn on the Island of Fredöen, on the west coast of Norway, a man unearthed a flat gold armlet with Runic inscriptions, and bearing on the inside the year 875. On the outside is a large bright stone, but of what kind has not yet been ascertained. This island is rich in historical traditions from the Viking era.

IN a late issue of the *Izvestia* of the Russian Geographical Society M. Krasnoff makes some interesting remarks on the antiquities of Turkistan. He points out that in the stone inscriptions he has seen in the Tian-Shan the men are always represented on horseback, armed with bows, arrows, long pikes with flags, and curved swords. Their dress is like the present *khalat* of the Mongolians and the Turks. The scenes represented mostly relate to hunting, and the men are surrounded by stags, *arkhars* (wild sheep), foxes, tigers, wild boars, and some very big animal with a thick hairy tail, and with tusks like those of the mammoth. In the gorge of the Uzun-su, M. Krasnoff saw the drawing of a camel. There are no inscriptions proper by the side of these drawings; but plenty of wild sheep, like the *tau-tek* of our days, are represented in files along mountain-paths. These drawings are very rapidly obliterated, and will soon disappear. They ought to be reproduced by archæologists.

THE second number of the "Bibliographies of Indian Languages," by James C. Pilling, has just been issued by the U.S. Bureau of Ethnology. It treats of the Siouan stock.

A USEFUL Catalogue of British Mollusca, published by Mr. H. W. Marsden, of Gloucester, has been sent to us. The Catalogue has been compiled by Mr. Charles Jeffreys, from Jeffreys' "British Conchology," with alterations and additions to date.

THE Royal Botanic Garden of Calcutta has just completed the first hundred years of its existence, having been established in the year 1787. The *Times of India*, in reviewing the history of the Garden, points out the many advantages which it has conferred on India. It has practically established and has done wonders to promote the now flourishing tea industry of India. The directors were the first to introduce potato-growing in that country, and they imported the quinine-yielding cinchonas from South America, and thus took the first step towards the establishment of what is now one of the most successful Indian industries. Besides these great successes, India owes to that establishment, the *Times of India* thinks, almost all the efforts that have been made to improve the quality of Indian cotton, and to push its sale in the European markets. The best sugar-cane has been brought from the West Indian Islands, and has been planted in all parts of the country; and flax, hemp, tobacco, henbane, vanilla, coffee, cocoa, ipecacuanha, india-rubber, tapioca, and many other products have been systematically experimented on in the Garden. Nor has horticulture been neglected by the superintendents, for the presence in India

of a large portion of its exotic plants is due to them; and the improved systems of cultivation are in a great measure attributable to their efforts.

THE French Government has commissioned Count Horace de Choiseul, a member of the Chamber of Deputies, to proceed on a voyage of botanical research to Asia and the United States. He will visit the Botanical Gardens at Ceylon, Calcutta, Shanghai, Japan, San Francisco, &c., to collect botanical specimens not indigenous to France.

THE Royal Physical Society, of Edinburgh, seems to be doing much good work. At the second meeting for the present session, Sir William Turner in the chair, Mr. Hoyle read a note discussing the function of the Laurer-Stieda canal in the Trematoda; Mr. J. Arthur Thomson submitted an elaborate paper entitled "A Synthetic Survey of the Influence of the Environment upon the Organism"; the Secretary, Dr. Traquair, communicated a paper on an ornithological visit to the Ascrib Islands, by Mr. John Swinburne; and Mr. Brook gave some valuable notes on the marine Crustacea of the Clyde Estuary.

CAPT. WIGGINS, who successfully performed the sea voyage from Europe to Siberia last autumn in the steamer *Phœnix*, is shortly expected back in this country. He states that at the time of his leaving Yeniseisk, in Siberia, in October, the cold varied from 70° to 80° below zero, and that the mercury was frozen in the bulb.

AT the annual meeting and distribution of prizes at the School of Science and Art at Bromley, Kent, on Tuesday, Sir John Lubbock delivered an interesting address on technical education. He referred to a recent statement of Mr. S. Smith, one of the Commissioners on Technical Education, to the effect that it was not so much the longer hours and lower wages of Continental workmen, nor the tariffs, which were having such objectionable influence on our industries, but rather, in nearly all instances, the great attractiveness of the goods themselves, which had been made by workmen who had received special training in schools. Sir John Lubbock went on to say that if we had spent one tithe of the treasure which we sent abroad every year to buy the produce of the skill of other countries on the training of our own people, we should have been making these goods ourselves and shipping them to the East and West and to every country under the sun. We were constantly crying out for new markets, while there was a new market in every house in the country. We were apt, indeed, to forget how much we owed to science, because many things which were in reality scientific discoveries had become so familiar to us that we looked upon them almost as a matter of course. The electric light was still felt to be a triumph of science, but we forgot sometimes that the common candle was the result of a whole series of chemical discoveries. The Chinese were said to have examined candidates for the army until lately in the use of bows and arrows. We saw the absurdity of this; but we were not free from the same error ourselves.

IN a recent Consular Report there is a complete description of the Technical University of Belgium, which was founded in 1852, as well as a general sketch of the system of commercial and technical training prevailing in that country. Formerly the education in Belgian public schools (*Athénées*) was in the main classical, but in recent years a *section professionelle* (commercial and scientific) has been added, and now takes its place as an integral portion of the public-school system. Here youths intended for commercial pursuits, from the fourth class upwards, receive special instruction, and then pass on to the *Institut*, or University, where the course lasts two years. The number of pupils is 150, a number which would be largely increased, but for the difficult entrance examination, the inability of many parents to keep their children so long at school, and the prevailing idea that a youth intended for commerce cannot enter a counting-house too soon. The course at the *Institut* includes, besides languages, book-keeping, and the ordinary practical work of a merchant's office, a technical description of the ordinary articles of commerce, political economy and statistics, commercial and industrial geography, maritime and Customs' legislation, and the building and fitting out of ships. The fees range from £11 for the second year at the *Institut*, to £3 5*s*. per annum at the public schools. An extensive commercial museum, a chemical laboratory, and a commercial library are attached to the *Institut*. At the end of the course diplomas are given to the successful candidates, entitling them to the degree of *Licencié en Sciences Commerciales*. The rules, and a programme and syllabus of the lectures, are appended to the Report. The new language, *Volapük*, has been added as an experiment, mainly, it would appear, because of its possible utility for telegraphic communication.

THE additions to the Zoological Society's Gardens during the past week include a Mexican Deer (*Cariacus mexicanus* ♀) from the Island of Dominica, presented by Mr. George Anderson; a Water Rail (*Rallus aquaticus*), British, presented by Mr. G. J. Payne; two Black-headed Gulls (*Larus ridibundus*), British, presented by Mr. Thomas A. Cotton; two Common Peafowls (*Pavo cristatus* ♀ ♀) from India, presented by Mr. Richard Hunter; sixty-six Skylarks (*Alauda arvensis*), British, purchased; an Egyptian Vulture (*Neophron percnopterus*) from North Africa, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE MAURITIUS OBSERVATORY.—The report of the Director of the Royal Alfred Observatory, Mauritius, for 1886, shows that the activity of the institution continues to be exhibited in two directions, viz. meteorological and magnetic observations, and the photographic record of the state of the solar surface. The meteorological observations have been extended during the year by the addition to the daily routine, of observations of the duration of bright sunshine, commenced October 1, of maximum and minimum dry and wet bulb thermometers in screens, begun November 1, and of an earth thermometer at 10 feet below the surface of the ground, begun the same day, the necessary instruments having been received from England. The year 1886 was a particularly dry one, the rainfall being below the average in every month, and the annual fall the smallest on record. No hurricane visited the colony; indeed, the last took place so long ago as March 21, 1879; but several cyclones occurred in the Indian Ocean, some of which passed near the island, and storm warnings were issued. Dr. Meldrum gives considerable importance in his report to the connection between the meteorology of the island and its health. It appears that wet years give specially high fever and death rates, the greatest mortality usually following the maximum rainfall by about two months. At the same time there has been a persistent increase in the death rate of late years, which appears to be independent of meteorlogical causes.

The photoheliograph was in constant operation, 533 photographs having been obtained on 353 days, but the sunspots were much fewer and smaller than in 1884 and 1885. Two photographs were also obtained of the solar eclipse of August 29, which commenced at Mauritius a little before sunset.

OCCULTATIONS OF STARS BY PLANETS.—The following lis of possible occultations of stars by planets is in continuation of that given in NATURE, vol. xxxvii. p. 234:—

Planet.	G.M.T. of Conjunction in R.A.	Star.	Mag.	Pl. − ∗.	Max. Duration.
	h. m.			′	m.
♀	Jan. 25...18 30·0	...AOe$_2$ No. 17179	...8·5	−0·39...	5·0
♀	31...17 30·5	...S.D. −21° No. 4933	...9·3	+0·58...	4·7
♄	Feb. 5...11 18	...D.M. +20° No. 2073	...9·5	−0·88...	108
♄	16... 7 27	...D.M. +20° No. 2062	...9·5	+0·38...	121

OLBERS' COMET.—The following ephemeris for Berlin midnight is in continuation of that given in NATURE, vol. xxxvii. p. 234:—

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Jan. 22	17 18.29	3 49·6 S.	0·2866	0·3932	0·36
24	17 21 39	4 5·3			
26	17 24 44	4 20·4	0·2958	0·3948	0·34
28	17 27 44	4 35·0			
30	17 30 40	4 49·2	0·3048	0·3961	0·33
Feb. 1	17 33 31	5 3·0			
3	17 36 17	5 16·3	0·3137	0·3969	0·31
5	17 38 59	5 29·2			
7	17 41 36	5 41·7 S.	0·3224	0·3974	0·30

The brightness on 1887 August 27 is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JANUARY* 22–28.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 22

Sun rises, 7h. 55m.; souths, 12h. 11m. 46·3s.; sets, 16h. 28m.: right asc. on meridian, 20h. 16·9m.; decl. 19° 44′ S. Sidereal Time at Sunset, 0h. 34m.

Moon (Full on January 28, 23h.) rises, 11h. 58m.; souths, 19h. 8m.; sets, 2h. 29m.*: right asc. on meridian, 3h. 14·6m.; decl. 12° 39′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury	8 16	12 24	16 32	20 29·3	21 11 S.
Venus	5 7	9 15	13 23	17 19·6	21 7 S.
Mars	23 47*	5 19	10 51	13 22·8	6 13 S.
Jupiter	3 35	7 53	12 11	15 57·2	19 33 S.
Saturn	16 26*	0 19	8 12	8 22·5	19 56 N.
Uranus	23 29*	5 1	10 33	13 4·6	6 9 S.
Neptune	11 56	19 35	3 14*	3 41·8	17 54 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Jan.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. °	°
23	*f* Tauri	4	1 19	near approach	51	—
26	χ^3 Orionis	6	0 36	1 20	178	258
26	68 Orionis	6	4 57	near approach	215	—
28	B.A.C. 2683	6	5 10	near approach	209	—

Jan.	h.	
22	1	Uranus stationary.
23	14	Saturn in opposition to the Sun.
24	23	Jupiter in conjunction with and 0° 8′ south of β^1 Scorpii.
28	14	Saturn in conjuuction with and 1° 10′ north of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Jan. 25,	21 20	*m*
Algol	3 0·9	40 31 N.	,, 23,	20 37	*m*
S Aurigæ	5 19·7	34 3 N.	,, 28,		*M*
R Canis Majoris	7 14·5	16 12 S.	,, 27,	20 36	*m*
			,, 28,	23 52	*m*
S Cancri	8 37·6	19 26 N.	,, 28,	22 28	*m*
W Virginis	13 20·3	2 48 S.	,, 26,	5 0	*m*
R Camelopardalis	14 26·1	84 20 N.	,, 25,		*M*
δ Libræ	14 55·0	8 4 S.	,, 22,	19 51	*m*
			,, 25,	3 42	*m*
U Ophiuchi	17 10·9	1 20 N.	,, 24,	3 4	*m*
		and at intervals of		20 8	
β Lyræ	18 46·0	33 14 N.	Jan. 25,	23 0	*m*
V Cygui	20 37·7	47 45 N.	,, 22,		*M*
S Delphini	20 37·9	16 41 N.	,, 24,		*M*
Y Cygni	20 47·6	34 14 N.	,, 22,	20 40	*m*
			,, 25,	20 33	*m*
δ Cephei	22 25·0	57 51 N.	,, 28,	0 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ι Ursæ Majoris	133	48 N.	
,, σ Leonis	167	5 N.	Very swift.
,, α Coronæ Borealis	236	25 N.	January 28. Very swift.

GEOGRAPHICAL NOTES.

DR. MEYER has been giving an account of his ascent of Kilimanjaro to the Berlin Geographical Society, and from the brief abstract which has appeared his statements are not quite consistent with those made in his letter already referred to. For one thing, Alpinists are doubtful if Dr. Meyer got so close to the summit by a thousand feet as he himself thinks he did; and moreover, from his own statements, his aneroid was quite untrustworthy.

A SPECIAL meeting of the Paris Geographical Society was held on Saturday, to welcome MM. Bonvalot, Capus, and Pepin, who have been journeying in Central Asia. We have already on several occasions referred to this journey, during which the travellers crossed the Pamir, but not for the first time, as they themselves seem to believe. So far it would appear as if the original results of this expedition were of no great value.

THE paper at Monday's meeting of the Royal Geographical Society was by a young engineer, Mr. W. J. Steains, on an exploration of the Rio Dôce and its northern tributaries (Brazil). The Rio Dôce has been in past years a classical region for research in natural history, but for many years it has been neglected. It flows through a region that has scarcely been touched by the influences of civilization, a region which is the home of the Botocudos, one of the most primitive people on the face of the earth. The Rio Dôce lies between parallels 19°–21° S. latitude, and is formed by several small streams springing from the eastern slope of an important range of mountains known by the name of the Serra da Mantiqueira. This range, running in a north-easterly direction, forms a portion of the irregular "coast-range" of Brazil, and forms, so to speak, the "retaining wall" of the series of elevated, undulating tablelands composing the greater portion of Central and Southern Brazil. The total length of the Rio Dôce is a little over 450 miles. That portion of the Rio Dôce basin lying east of the Serra dos Aymôres is a densely wooded lowland, sloping gradually towards the coast from an elevation of about 900 feet. Near the coast this plain resolves itself into a long stretch of low alluvial ground, studded for the most part with small shallow lakes that communicate with each other by means of long, narrow, winding streams, called "vallões." The largest of these lakes is the Lagôa Juparaná, which communicates with the Dôce some 30 miles above its mouth by means of a narrow, tortuous, deep channel 7 miles long. The lake is 18 miles long, and about 2½ miles broad at its southern extremity. It is very deep, and with the exception of some low alluvial ground at its northern and southern ends, is surrounded by high wooded bluffs, composed for the most part of reddish clay overlying a stratum of coarse red sandstone. At the head of the lake is a river—the S. José, which rises in the Serra dos Aymôres, and flows through an unexplored district, inhabited by wandering hordes of wild Botocudo Indians. Throughout the whole of its course, the S. José flows through dense forest abounding in the much sought-after "Jacarandá," or rosewood tree (*Bignonia cærulea*, Will.) The Botocudos number about 7000 people, and among some of the more savage tribes cannibalism still prevails. Mr. Steains stayed several weeks among these people, and is therefore able to add something to our knowledge of them. In appearance Mr. Steains states, the Botocudos can scarcely be called prepossessing. The average height is 5 feet 4 inches. Their chests are very broad, and this accounts for the facility with which they can bend their bows, which are exceedingly strong, being made out of the tough springy wood of the Ayri or Brijaubá palm (*Astrocaryum Ayri*, Mart.). The feet and hands of the Botocudos are small rather than delicate, and these are in fair proportion to their legs and arms, which are lean but muscular. Concerning the colour of their skin, these Indians are of all shades, some being of a dark reddish-brown, whilst others, and especially the women, are quite light. With regard to features, the Botocudos struck Mr. Steains, as they have done

others, as bearing a wonderful resemblance to the Chinese, and if, instead of wearing their hair cut round their heads so as to form a kind of mop, they wore pigtails, the casual observer would scarcely be able to tell where the difference lay. The hideous custom for which the Botocudos have always been so famous, viz. that of wearing huge lip- and ear-ornaments of wood, is fast dying out, and at the present time is only to be met with among some of the older members of the tribes, who retain all the habits and manners of their primitive forefathers intact.

THE January number of *Petermann's Mitteilungen* contains a paper by Count Pfeil, describing his journey last summer in East Africa, from Pangani along the Pangani River, south through Useguha to the Kingani River, and north to Bagamoyo. Dr. Henry Lange briefly describes the region watered by the Rio Tubarao and Rio Ararangua in Brazil. Dr. H. Fritsche contributes a series of astronomico-geographical and magnetic observations at thirty-one places in North-West Russia and North Germany in 1885–6–7, and Mr. S. Brooke gives a short acount of an excursion he made into the West Australian desert, starting from Israelite Bay on the south coast.

IN the January number of the *Scottish Geographical Magazine*, Mr. John Murray publishes the final results of his long research on the height of the land and the depth of the ocean. The paper consists mainly of a series of elaborate measurements giving the detailed data on which he founds his general conclusions. The conclusions to which Mr. Murray comes are of great interest, but they are too important to be stated in a note. The mean height of the land of the globe he estimates at 2252 feet. He finds that 84 per cent. of the land of the globe lies between the sea-level and a height of 6000 feet. The mean depth of the ocean again is 14,640 feet. In contrast with the land, only 42 per cent. of the waters of the ocean lie between the surface and a depth of 6000 feet; while 56 per cent. of the ocean waters are situated between depths of 6000 and 18,000 feet. The total area of the dry land Mr. Murray makes to be 55,000,000 square miles, while that of the ocean is 137,200,000 square miles. The bulk of the dry land above the sea is 23,450,000 cubic miles, and the volume of the waters of the ocean 323,800,000 cubic miles. The amount of matter carried from the land each year in suspension and solution, he estimates at 3·7 cubic miles; it would thus take 6,340,000 years to transport the whole of the solid land down to the sea. Should the whole of the solid land be reduced to one level under the ocean, then the surface of the earth would be covered by an ocean with a uniform depth of about two miles. The volume of the whole sphere, Mr. Murray estimates at 259,850,117,778 cubic miles. With the data now published should be compared Mr. Murray's Aberdeen lecture (NATURE, vol. xxxii. p. 581).

IN the last number of the *Comptes rendus* of the Paris Geographical Society, M. Chaffaujon gives a detailed narrative of his recent journey up the Orinoco. The section of greatest interest is that which relates to the upper course of the river, which M. Chaffaujon found to be all wrong on existing maps. This he has traced with much care. He examined also with care the outlet of the Casiquiare, by which the river is connected with the Rio Negro and the Amazons. He finds the bank of the river here to be mostly gravel, and in the rainy season the river coming down from the mountains with considerable force impinges against the bank, and forces a passage out. He states that the place of outlet seems to be shifting downwards every year.

THE TOTAL ECLIPSE OF THE MOON, JANUARY 28.

A TOTAL eclipse of the Moon offers some special advantages for the exact determination of the diameter and distance of our satellite. Observations of the bright limbs are exposed to considerable errors from the effect of irradiation, and liable to be affected by personal habit in the observer. The method of occultations has, under ordinary circumstances, proved scarcely more successful, owing chiefly to the fact that immersion and emersion so seldom take place under similar conditions. But in a total eclipse of the Moon, the disappearances and reappearances occur at limbs under similar illumination, and since the diminution of the Moon's light allows much fainter stars to be seen close to the Moon than can usually be observed, a much greater number of observations can be made than under ordinary conditions, and the effects of local irregularities of the Moon's circumference can be eliminated by observations made at a great number of points. If, then, as many Observatories as possible would combine to observe the occultations of the small stars passed over by the Moon during its eclipse, the labours of a few hours would give materials for a better determination of its diameter and parallax than could otherwise be obtained from the observations of many years. In view of these advantages, and noting too how hitherto they had been neglected by astronomers, Dr. Döllen, of Pulkowa, published a paper in the *Astronomische Nachrichten*, No. 2615, previous to the eclipse of October 4, 1884, in which he gave a catalogue of 116 stars which would be occulted during that eclipse, and begged for the co-operation of as many observers as possible. Unfortunately, the weather in many places was very unfavourable, and even where the sky was clear an unforeseen hindrance to observation was experienced in the unusual faintness of the eclipsed Moon. The part of the sky, too, through which it was passing was bare of stars above the 9th and 10th magnitudes. Still the results were sufficiently successful to encourage Prof. Struve and Dr. Döllen to repeat the attempt, especially as under several aspects the approaching eclipse of January 28 presents more favourable conditions than that of October 4, 1884: the magnitude of the eclipse will be somewhat larger, and the duration of the total phase a few minutes longer. Accordingly, Dr. Döllen has drawn up a catalogue of 300 stars which will be occulted, whilst Prof. Struve has computed by a graphical method the times of disappearance and reappearance, and the position-angles of the occulted stars, for 120 Observatories, which he has invited to cooperate with him in the work of observation. The experience gained during the 1884 eclipse has led Dr. Döllen to include only those stars occulted during the total phase or immediately before and after, but he has thought it well to give stars down to the 11th magnitude.

Of the 300 stars given in Dr. Döllen's catalogue, the majority of course will not be seen to be occulted from any part of this country. The following, however, may be observed here:—

No.	R.A.	Decl.	No.	R.A.	Decl.
87...	130° 25′·18...	17° 26′·95 N.	164...	131° 3′·87 ..	17° 26′·81 N.
91...	27·98...	35·12	165...	3·96...	25·64
93...	28·70...	35·57	166...	4·48...	32·90
97...	29·14...	45·66	172...	6·26...	17·96
98...	29·53...	37·64	180...	10·35...	32·80
100...	30·08...	38·14	181...	12·61...	38·34
102...	30·18...	23·95	190...	16·58...	12·54
108...	34·21...	44·27	192...	18·52...	44·17
110...	35·90...	30·12	194...	19·26...	38·34
112...	36·51...	47·21	197...	21·11...	19·06
114...	130 37·43...	17 19·16 N.	198 ..	131 21·33...	17 26·69 N.
115...	37·44...	47·07	201...	23·15...	37·63
116...	37·89...	48·54	207...	24·96...	26·65
124...	40·69...	49·34	209...	25·71...	22·85
125...	40·76...	18·56	210...	26·11...	30·07
126...	41·76...	30·46	212...	28·48...	17·66
128...	43·50...	34·10	216...	30·76...	17·96
130...	45·17...	45·27	219...	31·77...	8·64
134...	48·24...	42·16	221...	32·45...	35·77
136...	49·50...	45·96	223...	32·58...	26·14
138...	130 50·10...	17 26·35 N.	224...	131 33·05...	17 32·50 N.
142...	54·18...	18·36	225...	33·65...	22·31
144...	54·71...	35·17	226...	33·71...	13·84
148...	56·91...	38·34	233...	37·74...	9·24
150...	57·53...	22·75	236...	39·74...	21·26
152...	57·97...	28·93	237...	40·51...	30·82
153...	59·04...	22·95	242...	43·43...	17·36
155...	59·88...	15·96	247 ..	48·32...	24·55
156...	131 0·48...	36·32	248...	48·44...	11·24
157...	0·75 ..	39·91	251...	49·29...	9·44

The positions given are the apparent positions for January 28, 1888, and are expressed for R.A., as well as declination, in degrees, minutes of a degree, and hundredths of a minute.

The following are the times of disappearance and reappearance as furnished by Prof. Struve for the stars which will be occulted

by the Moon at Greenwich. The angles are counted from the true North through the true East as in observations of double stars, &c. :—

Disappearances.

Star's No.	Angle. °	G.M.T. h. m.
148	74	10 23·1
152	107	25·8
156	80	30·5
Beginning of total phase		
150	131	10 32·3
157	65	33·8
153	128	34·8
142	154	37·1
166	89	38·7
164	111	39·7
165	116	41·1
180	86	52·0
155	163	55·3
172	145	58·8
181	63	11 1·3
198	100	17·6
194	57	18·6
197	127	24·4
207	97	25·5
201	56	27·8
210	84	28·1
209	110	29·4
190	164	34·2
212	127	41·2
223	94	42·9
216	124	45·3
224	70	46·4
225	107	46·9
221	56	49·4
226	138	58·2
236	105	12 0·8
237	70	3·5
End of total phase		
242	116	12 11·9
219	168	12·1
233	155	17·4
247	87	19·1

Reappearances.

Star's No.	Angle. °	G.M.T. h. m.
87	243	10 22·3
97	316	23·6
124	351	29·3
116	339	30·2
102	234	30·2
Beginning of total phase		
91	277	10 32·3
112	330	32·7
93	278	33·7
115	331	34·6
98	286	34·6
114	211	35·1
120	288	35·4
108	314	35·7
125	211	42·5
110	264	50·1
130	328	53·1
136	337	57·9
126	269	11 3·7
134	317	6·4
128	283	6·6
138	260	22·1
142	228	22·3
144	294	29·8
148	308	30·2
155	221	31·5
157	318	34·5
150	252	38·1
156	303	40·3
152	275	40·6
153	254	41·8
166	294	52·6
164	273	54·4
172	240	54·5
165	268	54·7
181	322	59·7
180	298	12 4·8
End of total phase		
190	222	12 10·9
194	328	11·2
201	330	19·4

The following table gives the magnitude of the occulted stars :—

Star's No.	Mag.	Star's No.	Mag.	Star's No.	Mag.	Star's No.	Mag.
100	9·5	150	10	181	10	219	10
108	9·3	153	10	197	10	221	10
126	9·5	157	9·4	198	9·5	225	10
128	9·5	164	8·0	201	8·7	226	10
136	9·5	165	9·4	209	10	236	9·5
142	10	166	9·5	210	9·5	247	9·2
148	10	180	9·5	216	10		

The remaining stars are all of the eleventh magnitude.

It would be advisable for intending observers to make a rough map of the stars they are to observe, and to acquaint themselves as completely as they are able with their configuration. The observations should be rehearsed as far as possible on previous evenings, that the necessary quickness in changing from one point of the Moon's limb to another may be acquired, and a fair acquaintance made with the sequence of the settings. It will be well probably, to somewhat reduce the list of stars for observation ; since some of the phenomena follow each other so closely that some must be lost, and if the work of selection is left for the actual time of observation probably more stars will be lost than necessity demands, and a risk of confusion and mistake will be incurred. The suggestion has also been made that the eye-piece to be employed should not be placed as usual in the centre of the field, but be made to revolve round it at the distance of the Moon's radius. The Moon would then be brought to the centre of the field, and kept there throughout the entire series of observations, and only the eye-piece would be moved. A fairly high power will probably be found the best for the work.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Among the lectures for the present term we note the following:—

Chemistry: Prof. Dewar, on Organic Chemistry; Mr. Pattison Muir (Caius), on Chemical Affinity; Mr. Heycock (King's), on Chemical Philosophy for Natural Sciences Tripos, Part I. ; Mr. Robinson, on Agricultural Chemistry.

Physics: Prof. Stokes, Physical Optics; Prof. J. J. Thomson, Properties of Matter; Mr. Shaw (Emmanuel), Thermodynamics and Radiation.

Geology: Prof. Hughes, Geology of a District to be visited at Easter; Mr. Marr, Principles of Geology.

Botany: Mr. Gardiner, Advanced Anatomy of Plants; Mr. Potter, Advanced Systematic Botany.

Zoology: Prof. Newton, Geographical Distribution of Vertebrates; Mr. Sedgwick, Morphology of Mollucsa and Echinodermata; Mr. Gordon, Morphology of Amniota, recent and extinct.

Physiology: Dr. Lea, Chemical Physiology; Mr. Langley, Advanced Histology and Physiology; Dr. Gaskell, Advanced Physiology of Vascular System.

Prof. Ray lectures on Pathology, and has practical classes; Prof. Latham on the Physiological Actions and Therapeutical Uses of Remedies; Dr. Anningson gives demonstrations in Practical Hygiene.

In Mathematics the following are among the lectures:—Prof. Cayley, Analytical Geometry; Mr. Forsyth, Modern Algebra, symbolical methods and ternary forms; Dr. Ferrers, Elliptic Functions; Dr. Besant, Integral Calculus, Definite Integrals, Mean Value and Probability, Calculus of Variations, and Differential Equations; Mr. Ball, History of Mathematics up to 1637; Mr. Mollison, Discontinuous Functions and Conduction of Heat; Mr. Whitehead, Grassmann's Ausdehnungslehre, with special reference to its applications.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 22, 1887.—"The Early Stages in the Development of *Antedon rosacea.*" By H. Bury, B.A., F.L.S., Scholar of Trinity College, Cambridge. Communicated by P. Herbert Carpenter, D.Sc., F.R.S., F.L.S.

In the orientation of the larva, J. Barrois' suggestion (*Comptes rendus*, November 9, 1886) has been adopted, viz. that the stalk of the Pentacrinoid represents the præoral lobe of other Echinoderms. Besides the right and left body-cavities, an anterior unpaired body-cavity is developed (distinct from the hydrocele), and opens to the exterior by the water-pore in the free-swimming larva.

A larval nervous system is developed, but is lost after fixation.

The vestibule of the fixed larva (Cystid) is formed by invagination, as described by Barrois (*Comptes rendus*, May 24, 1886).

The water-tube (stone canal), by opening into the anterior body-cavity (now very small), places the water-vascular ring in indirect communication with the exterior.

The anus opens in the same interradius as the water-pore.

In the skeleton, besides the parts already known, three underbasals are present, which are of great phylogenetic interest.

Geological Society, December 21, 1887.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—On the correlation of some of the Eocene strata in the Tertiary basins of England, Belgium, and the north of France, by Prof. Joseph Prestwich, F.R.S. Although the relations of the several series have been for the most part established, there are still differences of opinion as to the exact relation of the Sable de Bracheux and of the Soissonnais to the English series; of the Oldhaven Beds to the Woolwich series; and of the London Clay and Lower and Upper Bagshots to equivalent strata in the Paris basin. The author referred to the usual classification of the Eocene series, and proceeded to deal with each group in ascending order. The Calcaire de Mons is not represented in England, but may be in France by the Strontianiferous marls of Meudon. It contains a rich molluscan fauna, including 300 species of Gasteropods, many of which are peculiar, but all the genera are Tertiary forms. The Heersian are beds of local occurrence, and the author sees no good reason for separating them from the Lower Landenian or Thanet Sands. He gave reasons for excluding the Sands of Bracheux from this group. Out

twenty-eight Pegwell Bay species, ten are common to the Lower Landenian, and five to the Bracheux Sands, which present a marked analogy with the Woolwich series. These Sands of Bracheux are replaced in the neighbourhood of Paris by red and mottled clays. Out of forty-five species at Beauvais, only six are common to the Thanet Sands, and ten to the Woolwich series. Out of seventy-five species in the Woolwich and Reading Beds, nineteen occur in the Bracheux Beds, if we add to these latter the sands of Chalon-sur-Vesles. Respecting the Basement Bed of the London Clay (Oldhaven Beds in part), the author would exclude the Sundridge and Charlton fossils, which should be placed on a level with the Upper Marine Beds of Woolwich. He allowed that the former were deposited on an eroded surface, but this involves no real unconformity, whilst the palæontological evidence is in favour of this view, since, out of fifty-seven species in the Sundridge and associated beds, only sixteen are common to the London Clay. He therefore objected to the quadruple division. Either the Oldhaven should go with the Woolwich or with the Basement Bed. He admitted that the term "Basement Bed" is objectionable, and preferred Mr. Whitaker's term for the series, as he would limit it. The Lower Bagshot Sands the author would call "London Sands," whose Belgian equivalent is the Upper Ypresian, and the French the Sands of Cuise-de la-Motte, forming the uppermost series of the Lower Eocene. A group of fossils has been discovered in the Upper Ypresian sands of Belgium, which leaves no doubt of their being of Lower Eocene age, and consequently the Lower Bagshots must be placed upon the same horizon. There is no separating line of erosion between the London Clay and the Lower Bagshots, the upper part of the former is sandy, and the lower part of the latter frequently argillaceous. Similarly no definite line can be drawn between the Upper and Lower Ypresian; but in both countries this series is separated from overlying beds by a well-marked line of erosion. So also in France the base of the Calcaire Grossier (Bracklesham Beds) is a pebbly greensand resting on an eroded surface of the Sands of the Cuise-de-la-Motte. In Belgium, in Whitecliff Bay, and in the Bagshot district the Upper Eocene rests upon an eroded surface of the Lower Eocene. The reading of this paper was followed by a discussion in which the President, Mr. Whitaker, Dr. Evans, Dr. Geikie, and others took part.—On the Cambrian and associated rocks in North-West Caernarvonshire, by Prof. J. F. Blake.

PARIS.

Academy of Sciences, January 9.—M. Janssen, President, in the chair.—Remarks on M. Cornu's last note regarding the synchronizing of time-pieces, by M. C. Wolf. The author points out that M. Cornu has misunderstood the language of the English physicist, Mr. Everett, whose theory is shown to be perfectly applicable to the Vérité method of synchronization. The efficiency of this system has received a remarkable confirmation from the circumstances attending an accident by which the synchronizing apparatus was recently put out of order in the city of Paris.—Researches on ruthenium, by MM. H. Debray and A. Joly. In continuation of previous studies of this rare metal, the authors here deal with its oxidation and the dissociation of its bioxide. From these researches it appears that hyperruthenic acid must now be added to the list of compounds which are easily destroyed by heat, although obtained at such high temperatures that their existence was long considered problematical. Their formation at these temperatures is analogous to the dissociation of bodies that were supposed to be incapable of decomposition before H. Sainte-Claire's discovery.—Researches on the breath of man and other mammals, by MM. Brown-Séquard and d'Arsonval. These researches make it evident that the air exhaled by mammals, even in a healthy state, contains a very powerful toxic element, to which should probably if not certainly be attributed the bad effects caused by breathing a close atmosphere.—Variation of temperature of a condensed or expanded vapour while preserving the same quantity of heat, by M. Ch. Antoine. An easy method is given for calculating the final tension that results from the variation of a given temperature, and the final temperature that results from a given degree of condensation or expansion.—On the influence of temperature on the magnetic state of iron, by M. P. Ledeboer. Although it has long been known that a magnet raised to a red heat loses its magnetic properties, no successful attempt had hitherto been made to determine by direct measurement the actual degree of temperature at which iron ceases to be a magnetic body. The experiments here described now show that iron remains magnetic up to 650° C., after which a rapid variation is observed in its magnetic condition. At 750° the magnetic properties are scarcely perceptible, and at 770° they disappear altogether, returning in the same way as the metal cools down. This presents a remarkable analogy to the conclusions of M. Pionchon, who, in his recent paper on the specific heat of iron at high temperatures, has shown that this metal undergoes a sudden change of state between 660° and 720°.—On the present value of the magnetic elements at the Observatory of the Parc Saint-Maur, by M. Th. Moureaux. The absolute values, as deduced from the mean of horary observations recorded by the magnetograph are as follow: declination, 15° 52'·1; inclination, 65° 14'·7; horizontal component, 0·19480; vertical, 0·42245; total force, 0·46520; longitude of the Observatory, 0° 9' 23" E. of Paris; N. lat., 48° 48' 34".—On the employment of sulphureted hydrogen for purifying the salts of cobalt and nickel, by M. H. Baubigny. The experiments here described clearly show that from a mixture of the salts of these two metals it is impossible to obtain a pure sulphuret either of nickel or of cobalt by the action of sulphureted hydrogen. Dellfs' statements regarding the action of hydrogen on the salts of the heavy metals are thus shown to be groundless.—On a new method of quantitative analysis for the nitrites, by M. A. Vivier. This method consists in using the reaction discovered by Millon for the analysis of urea, but with absorption of carbonic acid and measurement of the nitrogen liberated in the process.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Treatise on Algebra: Charles Smith (Macmillan).—The Nervous System and the Mind: C. Mercier (Macmillan).—Reports on the Mining Industries of New Zealand, 1887 (Wellington).—The Ethic of Freethought: K. Pearson (Unwin).—Year-book of Pharmacy, 1887 (Churchill).—An Elementary Text-book of Physiology: J. M'Gregor Robertson (Blackie).—Bergens Museums Aarsberetning for 1886 (Griegs, Bergen).—Zoological Record, vol. 23, 1886 (Gurney and Jackson).—A Course of Lectures on Electricity: G. Forbes (Longmans).—Report on Indian Fibres and Fibrous Substances (Spon).

CONTENTS.

THURSDAY, JANUARY 26, 1888.

ODIUM MEDICUM.

NO one will deny the truth of the saying, "All men are mortal," but very few have any definite feeling that it applies to them personally so long as they are in the possession of health and strength. Almost everyone, however, has either suffered at a former time, is suffering now, or is afraid of suffering at some future time, from ailments of some sort; and therefore the treatment of disease has a personal interest for everyone. On this account the discussions which have been going on for about a month in the *Times* regarding homœopathy have attracted a good deal of attention; but it is difficult for lay readers to understand the merits of the discussion thoroughly unless they know something about the "pathies" generally. The fundamental idea of the "pathies" is that the body does not readily tolerate more than one diseased process at the same time, and therefore one morbid condition may be driven out by inducing another.

The nucleus of our present medicine may be said to consist of the accumulated experience in the observation and treatment of disease possessed by the priests of Cos, and recorded by Hippocrates, who is justly regarded as the father of medicine. His treatment was based upon empiricism, and was not governed by any absolute rule, for, although he stated that in general diseases are cured by their contraries, he also allowed that disease might sometimes be relieved by medicines which produced similar symptoms, and mentioned that under certain circumstances purgatives will bind the bowels, astringents will loosen them, and substances which cause cough and strangury will also cure them.

The principle that contraries are cured by contraries, *e.g.* that constipation is cured by purgatives, attained so much importance under Galen and his followers, that the other principle of like being cured by like was nearly lost sight of, and so the antipathic school had for a long time the preponderance. But the use of evacuants, which formed a large portion of the practice of Hippocrates and of medical practice down to the present day, could not always be brought under the head of antipathy, and so it came to be admitted that one abnormal condition in the body might be relieved by inducing another, which was neither of the same kind as itself, nor of an opposite kind, but was simply of a different nature, and this is the allopathic form of treatment. As an example of this we may take the fact that a pain in the head may be cured by a medicine which does not act on the head at all, but upon the bowels.

The antipathic and the allopathic systems of medicine were in vogue in the time of Hahnemann, and their imperfections were very evident to a man of his mental power and acuteness. He saw clearly that the enormous doses which were given in his time were often productive of great harm, and in experimenting with smaller doses he found that his results were better. He also found, what had been noted before by Hippocrates, that he obtained curative effects from small doses of remedies which in large doses produced symptoms similar to those of the disease. In the recognition of this fact Hahnemann agreed with Hippocrates; but, while the father of medicine, testing everything by experiment and relying simply on the result of experience, regarded the rule "*similia similibus curantur*" as only of partial application, Hahnemann converted it into a universal rule. He began at first by relying on experiment, and spoke of pure experience as the "only infallible oracle of medicine," but he afterwards quitted this sure ground, and committed himself unreservedly to a belief in his theoretical opinions, whether supported by facts or not, and said in regard to his doses that the maxim as to the very smallest being the best is "not to be refuted by any experience in the world.' The essence of his system of homœopathy consisted in the universal application of the rule regarding the similar action of the drug to that of the disease, and in the smallness of the dose.

Some modern homœopathists are inclined to regard the minute dose as not essential to homœopathy, but Hahnemann says: "The appropriation of the medicine to any given case of disease does not depend solely upon the circumstance of its being perfectly homœopathic, but also upon the minute quantity of the dose in which it is administered." The extent to which he carried the dilution of his medicines was extraordinary, and he imagined that the more they were diluted the more potent did they become. Thus he says in his "Materia Medica Pura" (Dr. Dudgeon's translation) that the curative power of aconite is marvellous when it is given "in the dose of a thousandth part of a drop of the decillionth development of power." But even this astoundingly minute dose was unnecessarily strong in some cases, in which he thought "a single momentary olfaction at a phial containing a globule the size of a mustard-seed, moistened with the decillionth potency of aconite, is quite sufficient." But it is difficult for those who have not studied the action of potent drugs like aconite to form any definite judgment regarding their effect in large and small doses; so that it may be worth while to give his views regarding vegetable charcoal, a substance about which everyone can form an opinion. Most people will be surprised to hear that Hahnemann gives no fewer than 720 symptoms as being caused by a few grains of vegetable charcoal diluted a million-fold with milk sugar. These symptoms are of the most varying nature, from aching of the corns to headache, palpitation, and rheumatism, with sometimes a peevish temper, and at other times an excessively cheerful one. The variety and severity of these symptoms clearly show that they were not due to the vegetable charcoal at all, but would have occurred whether the charcoal had been taken or not. But the most remarkable instance of a fallacy in Hahnemann's conclusions appears in his famous experiment on the action of cinchona bark in producing ague, which has been regarded by homœopathists as one of the most important proofs of the truth of the system. Hahnemann, at one time of his life, had suffered from ague, as we learn from Ameke's "History of Homœopathy," but he had probably been free from it for some time before he made his experiment with cinchona. It is well known that persons who have once suffered from ague are apt to have it return when their digestion is disturbed, or when they are subject to depressing influences. The dose of powdered cinchona bark which Hahnemann took was

very large, and similar doses have produced in other people vomiting and gastro-intestinal irritation. In Hahnemann it produced symptoms of ague, but instead of concluding that the cinchona had simply brought back an attack of his old enemy, by acting as an irritant to his stomach, he concluded that cinchona bark had a specific power to produce ague. Others who have tried the experiment, and who have not had ague before, have naturally failed.

Hahnemann's system was greatly ridiculed and opposed both during his life and since, and yet, in spite of its absurdities in regard to dose, it has a number of adherents. The reasons of this are perhaps not very hard to find. For instead of homœopathic medicines being disagreeable to the patient, as those of regular practitioners too often are, they are given in a form which is rather pleasant than otherwise, and Hahnemann's rules of diet and regimen were very different from those followed by regular practitioners of his time. While they were apt to consider that anything that seemed agreeable to the patient was dangerous and to be forbidden, Hahnemann, placing full reliance on the influence of his infinitesimal doses, allowed the desire of the patient for food and drink to be gratified within proper limits, and the temperature of the chamber as well as the quantity of the bed-clothes to be regulated according to the wishes of the patient. There can be no doubt that the attention given by Hahnemann and his followers to diet and regimen have been of great service, not only to the patients they have treated, but to the whole medical profession. It is obvious that such a system as Hahnemann's—gratifying the desires of the patient so far as it was judicious, giving remedies in such minute doses as could at all events do no harm, and at the same time encouraging the patient with the positive assurance that the infinitesimal doses were of the utmost potency to effect a cure—had a great advantage over the system of allopathy. This advantage was to a certain extent shared by antipathy, inasmuch as both it and homœopathy acted on a definite plan, and chose their drugs according to what they supposed to be fixed laws.

Although so far behind the other two in some respects, allopathy had this great advantage over them, that it depended simply on the results of experiment; and although it might be influenced, and was influenced at times, by prevailing fashions, its followers were still searching after truth, while the others falsely supposed they had already found it. With the development of pathology and a truer insight into the nature of disease, the term allopathy has fallen to a great extent into disuse, and most of what we might term the orthodox practitioners of the present day object to range themselves under any "pathy" whatever, but aim at a rational practice founded on the one hand upon the knowledge of the nature of disease, and on the other of the action of remedies. Where these are insufficient to guide them, they fall back simply upon empiricism; expecting, however, that before long, wider knowledge may increase their power to cure their patients. Their power is no doubt very greatly on the increase; and we have only to look at the fact that within the last few years they have been able by the use of substances belonging to the aromatic series of chemical compounds to regulate the temperature of their patients, so that whereas formerly physicians were obliged to stand by idly while their patients died of high fever, they can now prevent the temperature from rising too high with almost perfect certainty, and thus save their patients' lives. Every day fresh contributions are being made both to the physician's knowledge of the nature of disease and his power to modify it or prevent it.

Yet still the regular physician is but a seeker after truth, and as yet no infallible rule by which to select his medicines is known to him. He cannot lay down with dogmatism that the medicine which he is about to administer is the only one or the very best one that can possibly be given, as a homœopath might do. He is therefore to a certain extent at a disadvantage as compared with the homœopath, especially in the treatment of those cases where the disease is not extremely severe, and where the effect upon the mind of the patient counts for as much or more than the action of the medicine itself. The want of a definite rule on the one hand affords an opportunity for the homœopath to sneer at the regular practitioner, while at the same time he complains that the regular practitioner refuses to have any dealings with him. But there seems to be no other course open to the regular practitioner, for he considers that the homœopath must do one of two things: he either believes in homœopathy, or he does not. If he believes in homœopathy as founded by Hahnemann, and prescribes for his patients infinitesimal doses with a conviction that he is actually modifying the disease from which they suffer, the regular practitioner regards him as a fool; while he would apply a still stronger term to the man who does not believe in Hahnemann's system, and uses powerful drugs in large doses, but nevertheless professes to treat his patients homœopathically. It is as useless for a regular practitioner to treat a patient along with a believer in homœopathy as it is for a modern chemist to undertake a joint research with a believer in phlogiston; and therefore the regular practitioner refuses to meet him in consultation so long as he holds homœopathic doctrines. But if the homœopath gives up his belief in infinitesimal doses, and in the universal application of the rule "*similia similibus curantur*," he has given up the essentials of homœopathy, and has no more title to the name of homœopath than Hippocrates had. If he has given up the thing he should give up the name and join the ranks of orthodoxy, but if he still retains the name for the sake of gain he can hardly expect to be welcomed by the orthodox part of the medical profession. It is very unfortunate that the "*odium medicum*" should exist, but the homœopaths seem more to blame for it than the followers of rational medicine.

DARWINISM AND ETHICS.

The Ethical Import of Darwinism. By Jacob Gould Schurman, M.A., D.Sc., Sage Professor of Philosophy in Cornell University. (London: Williams and Norgate, 1888.)

Morality and Utility. By George Payne Best, B.A., M.B. (London: Trübner and Co., 1887.)

WE will consider these two little books together, as in some measure the latter, although earlier in publication, answers the former.

More than half of Prof. Schurman's essay (which altogether extends to but about 250 small octavo pages) is occupied with a preliminary discussion of Darwinian principles *per se*, or without special reference to ethics. Here his object is to argue in favour of "teleological variation" along "beneficial" or "predetermined" lines—supporting this argument in the usual way by denying that natural selection is a cause of organic change. Natural selection can only act on the materials supplied to it by variation: it does not itself create these materials, and therefore leaves where it was before, the question as to the *origin* of the fittest. This argument always appears to us transparently fallacious; but, as our object at present is to consider what Prof. Schurman has to say on "the ethical import of Darwinism," we will not occupy space by discussing the weaker half of his work. In ethics, however, he is strong; and, in our opinion, has produced one of the best defences of the intuitional side which has appeared since the publication of the "Descent of Man."

He begins by pointing out the distinction between the utilitarianism of Bentham and of Darwin—viz. the difference between "pleasure-giving and power-giving," or hedonism and life-serving. Next, he provisionally allows that the Darwinian theory furnishes a proximate or natural explanation of the "innateness, immutability, and universality of moral conceptions." He also allows that, at all events to a large extent, this theory is able to explain the authoritativeness of conscience. But, having thus frankly conceded all that the Darwinist has to demand, he turns upon him as follows:—

"Is it forgotten that, even if goodness be an end in itself—the sole end worth living for—it still remains true that honesty is the best policy, that honest acts are the most advantageous acts, and that they will accordingly be preserved through natural selection in the struggle for existence? All that natural selection requires is that something shall be useful; *what else it may be*, what other predicates it may have, natural selection knows not and seeks not. Be virtue a proximate or an ultimate end, natural selection tells us it will be preserved and perpetuated if it be useful; and it tells us no more. It is, accordingly, a gratuitous assumption which our exponents of evolutionary ethics make, when they decline to allow more than a merely relative value to morality."

The first thing to notice about this position is, that the Darwinian, *quâ* Darwinian, has nothing to do with it. All that the Darwinian, as such, undertakes to show is, that conscience and the moral sense, in all its protean forms, admit of being explained as proximately due to natural causes. Whether or not these natural causes are themselves the results of a final cause, intelligent and moral—this is a question which Darwinism leaves the ethical philosophers to wrangle about.

But now, suppose that a man is not only a Darwinian, but also an ethical philosopher, what is he to make of Prof. Schurman's conclusion that "it is a gratuitous assumption which our exponents of evolutionary ethics make, when they decline to allow more than a merely relative value to morality"? Surely such a man must feel that the burden of proof here lies with the intuitionists. It is they who affirm a supernatural quality of the moral sense, over and above the natural origin of it which (as agreed) the Darwinian has explained; therefore it is for *them* to show that *their* "assumption" of the *absolute* value of morality is other than "gratuitous." This burden Prof. Schurman seeks to discharge as follows:—

"In opposition to this mechanical theory of conscience, we hold that it is an ultimate function of the mind, and that in germ, as in full fruition, it must be regarded, not as an action, but as *an ideal of action*. . . . This view of the subject may be affiliated to Darwinism as readily as the other. For an abiding ideal of action is, to say the least, quite as beneficial as a chance action; and wherever there is an advantage, there natural selection may operate."

Now, without question, "an ideal of action is quite as beneficial as a chance action"; but is it not evident that the Professor is here proving too much? The more he can show that "an ideal of action" admits of being developed in the race by natural causes on account of its utility to the race, the more is he playing into the hands of his opponents, so long as they do not agree to assume with him that morality is of any absolute or ultra-human signification. But it is precisely this assumption which he is required to justify; and the above attempt to discharge his burden of proof, far from making "in opposition to the mechanical theory of conscience," is merely a re-statement of that theory, *plus* his original assumption.

Mr. Best is not nearly so matured a thinker upon ethical problems, and yet upon this important matter he displays a clearer vision than Prof. Schurman. He shows that the intuitive (or instinctive) side of conscience is everywhere an "ideal of action"; but he also shows that where it stands alone, or without reasoned perceptions of utility, it is everywhere an ideal impossible to realize. With considerable originality and success, he argues that the moral ideal, in all phases of its development, is essentially irrational, inasmuch as it could only realize itself completely in a population all the members of which "are equal, asexual, and immortal." He then goes on to ask:—

"Suppose such an idea should become actually operant, and endeavour to realize itself in thought, or in action, in this world of inequality, sexuality, birth and death, what kind of phenomenon might we expect to arise from the conflict between idea and fact? Might we not expect to find in those in whom the moral intuitions were best developed a constant protest against things as they are? Might we not expect to find a hankering after equality? Might we not expect to find some, in reaction against that inequality which, in the form of wealth, obtrudes itself before their eyes, take refuge in voluntary poverty; might we not expect others to endeavour by force or contrivance to bring about the reign of equality? Might we not expect the dim picture of an asexual world to make men revolt against sex and sexual relations, and cry up celibacy as the holiest condition possible?" &c.

Thus, then, the moral ideal is more or less out of joint with actual fact; and although it is easy enough to understand why such should be the case if it is but of relative significance—or of no further meaning than that which arises from its utility to the race—we cannot so well understand why such should be the case if it be of absolute significance. And, if we extend our view beyond the human race, we are met by a similar difficulty. Not only man, but the whole creation, groans in pain and travail—that is to say, the unmoral as well as the moral;

and, therefore, the creatures whose pain and travail cannot possibly serve any moral purpose. Yet the moral sense of man, in its most "intuitive" or least rational form, is outraged even by the practice of vivisection with a view to an ultimate amelioration of sentient life.

Our object in saying this much is to show that Prof. Schurman does not appear to have perceived the basal difficulty against which he has to contend. The question which he undertakes to answer is whether the moral sense is of absolute or only of relative significance. But this question he merely begs on behalf of the intuitionists. Of course, if it be thus assumed that the moral sense is of absolute significance, it is reasonable enough to show that the fact of its utility is not opposed to the assumption. But where the validity of this assumption is the matter in dispute, an intuitionist only plays into the hands of the utilitarian by arguing that in his view of morality "an ideal of action may be affiliated to Darwinism as readily as any other." Nevertheless, although we thus deem Prof. Schurman's essay a failure in its argument against the mechanical interpretation of conscience, it is otherwise an able contribution to the literature of ethics; and anyone who is already an intuitionist may properly accept the work as proving that there is nothing in Darwinism, *per se*, which can be logically regarded as inimical to his theory.

GEORGE J. ROMANES.

AN INDEX-CATALOGUE.

Index-Catalogue to the Library of the Surgeon-General's Office, United States Army. Vol. VIII., Legier—Medicine (Naval). (Washington, 1887.)

THE regularity with which the large annual volumes of this great work reach us is most reassuring, and now its completion in some six more years may be looked upon as practically certain, considering the vast resources of the United States, and the energy which its editors have shown. It still remains unique among printed catalogues in its immense lists of articles from every species of periodical literature, arranged under subject-headings, and drawn from more than 3400 Journals, Reviews, Transactions, &c. It has added to its list last year 165 new periodicals, and its tastes are sufficiently catholic to include such as the *Revue Philosophique*, which contains important matter bearing on the fundamentals of physiology and psychology, but hardly touching on any professional details.

The entries are carried up to the end of 1886; the volume has a few words of preface dated June 1887. When it is considered that the papers of Delhi, Madras, and Adelaide, for example, take some weeks to reach Washington, and that any of these may contain entries which should take a place in any part of this book of 1078 closely printed quarto pages, there seems to us certainly to have been no loss of time in publication. There are many entries, in this volume, of Chinese and Japanese books, magazine articles, and manuscripts, which the editors insert in English characters, and are kind enough to translate for us. Of the European languages also, Hungarian, Russian, and Polish are as a rule translated, much more freely than in the last volume; but Swedish and Danish rarely, and Portuguese, Dutch, Spanish, Italian, and Greek not at all. A very commendable practice has sprung up, though it is not found everywhere possible, of putting the date of birth after a living author's name. Thus we read Lussana (1820—), Luys (1828—), &c. It would be very convenient if this could be further extended, though of course the difficulties in the way are obvious. There are some very large collections of entries under such words as Liver (70 pp.), Lungs (30 pp.), Lithotrity and Lithotomy (40 pp.), and the extent of the bibliography is well illustrated when we find 213 books and 646 articles entered under such a simple heading as Measles. By far the largest aggregation, and one as yet unfinished in this volume, is under Medicine, which in the present volume occupies 288 pages. It is a heading under which the subdivisions have been difficult to arrange; but the large bulk of matter has, on the whole, been well distributed. Under such a subdivision as Medicine (Anecdotes, Curiosities, &c.), we naturally find strange companions, such as "Uriel to his Compeers; adapted by Ithuriel"; "The Doctor, by the Author of 'Betsy Lee'"; "Sniggers (J.), Gnihtontuobaodahcum," the last a *Spiegelschrift* in print. Under Medicine (Systems, Theories, and Practice), we find a large group of the elder writers who are chiefly of historical interest, extending from "Averrhoes: Incipit Liber de medecina Averoys qui dicitur Coliget, &c., imp. folio, Venetiis, 1482 (Gothic letter), to the writers of the last generation, such as Dr. C. J. B. Williams (1842), and, curiously enough, containing only one small volume among the modern hand-books, "Elements of Practical Medicine, by A. H. Carter, 1881," which might have come more appropriately among Medicine (Manuals) or Medicine (Practice of), along with the mass of modern text-books. Groups are chronicled under Medicine (Magical, Mystic, Spagyric) of some 300 books, and of some 250 under Medicine (Chronothermal, including the Thompsonian system), which serve to remind us of the chequered history and varied principles of the healing art. To the accuracy of this vast body of references, amounting to more than 40,000 in all, it is Time that will bear the best testimony, as it has borne to those of the earlier volumes. A first testing on such detail as is practicable shows the figures right, and the text sometimes—as, for instance, in the case of M. Luys—more accurate than that of the author's own publisher in his advertisement columns. It is a mistake, we must allow, but we trust a very pardonable one, to have spelt the name of a distinguished living physician as "S. Wilkes"; and it is a pity that, under the record of Hospital Reports (London), we should find mention only of those of the Hospitals of St. Thomas and St. George. But these are trifles; when we close the heavy volume we cannot help feeling a hearty admiration of so much hard and careful work well spent, not on the aggrandizement of any individual fame, but on the steady and strenuous advancement of learning.

A. T. MYERS.

OUR BOOK SHELF.

A Vertebrate Fauna of Sutherland, Caithness, and West Cromarty. By J. A. Harvie-Brown and T. E. Buckley. (Edinburgh: David Douglas, 1887.)

THIS is a good type of all that a hand-book on local natural history ought to be from a naturalist's point of view. While it appears to be as exhaustive as any two

workers can make it of the fauna of which it treats, its honest tale is not only plainly but also briefly told. In other words, we are spared those poor attempts at poetical prose and all the allied sins which seem so easily to beset the field naturalist. This is another way of saying that the work has been undertaken and executed in a purely scientific spirit. After a few introductory chapters on the geography, topography, physical aspects, &c., of the area, the authors proceed to give a systematic catalogue of the entire vertebrate fauna, beginning with the mammals and ending with the fish. In this catalogue everything relating to distribution, habits, &c., which can possibly be of any interest is likewise set forth in terse phraseology. The whole catalogue covers between 200 and 300 octavo pages, and is everywhere indicative of painstaking labour. Several well-executed plates embellish the volume, which throughout displays good taste as well as sound judgment. We are, therefore, particularly glad to read in their preface that the writers intend this to constitute "the first volume of a series, which, unlike most local faunas, lays aside to a great extent political boundaries, and is marked out by others, much more natural, such as watersheds." We trust that this first volume will meet with the recognition which it deserves; and in any case congratulate the writers on having so successfully accomplished so extensive and valuable a piece of work.

G. J. R.

Gospel Ethnology. By S. R. Pattison. (London: Religious Tract Society.)

THE author observes that the many-sided investigations of ethnologists do not seem to have included a study of the way in which Christianity has been received by different races. The problem suggested is undoubtedly an interesting one, but the present volume does not do much to solve it, being mainly made up of a series of anecdotes which go to show that the Gospel, in the form in which it is set forth by Protestant missionaries of the Evangelical school, has found a response in the hearts of individuals of almost every known race. It is reasonable to infer from this that the particular type of Christianity to which Mr. Pattison confines his attention contains motives that appeal to men in almost every stage of social development and culture. But so far as one can judge from the anecdotes, which are not chosen with a view to facilitating scientific analysis, it seems probable that in every case the really effective element in the missionary teaching lay in the Gospel story itself, not in the dogmatic construction built on it by missionaries of a particular school. At any rate, it is plain that no discussion of the problem which Mr. Pattison deals with can claim to be of scientific value so long as it deals only with the reception given to one form of Christian teaching. Nor is it enough to know that *individuals* of almost every race are capable of becoming sincere Christians of a particular school: the ethnologist, from his point of view, is much less concerned with individuals than with masses. A serious inquiry into the fitness of Christianity to become the religion of *societies* that have not come under the influence of the civilization of the Roman Empire would be extremely useful, but such an inquiry cannot be made to any purpose if one starts by identifying Christianity with one of its local and particular types.

There is not really any ethnology in the book before us. There is, indeed, a chapter which professes to give a survey of the races of mankind, but it is so badly done that the book, which is really, as has been said, a collection of anecdotes, would have been better without it. There are some good woodcuts of people of different races.

The British Journal, Photographic Almanac, and Photographer's Daily Companion for 1888. Edited by J. Traill Taylor. (London: H. Greenwood and Co., 1888.)

To all those who are engaged in the art of photography either as amateurs or as professionals this work will be extremely useful. Besides all the ordinary information, such as that of developing, toning, &c., there are articles of the most practical and theoretical nature—written by such men as Captain Abney, F.R.S., Rev. S. J. Perry, F.R.S., &c.—on subjects which are most interesting, and of great service to those who have attained the higher branches of the art.

All the various tables and formulæ are here added, together with a list of all the Photographic Societies at home and abroad.

Two pictorial illustrations are given, one being of the famous yacht *Thistle*, printed by Messrs. Morgan and Kidd on their argentic bromide paper (360 being able to be printed from one negative in an hour) from a negative by Mr. A. H. Clark; the other being a callotype by Messrs. Waterlow and Sons from a negative by Mr. T. B. B. Wellington on a Pall Mall plate, entitled "You naughty boy."

For the sake of young photographers the Editor has written twelve chapters to present in simple language a few lessons in the practice of the art.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

"A Conspiracy of Silence."

WHEN I read Prof. Judd's letter in your issue of last week, I felt as if one of the Eocene volcanoes of the Isle of Mull, which he has described so well, had broken out afresh and covered a great extent of country with erupted matter, decidedly, by a wonderful phenomenon, of the "acid series."

I have a very short reply to give:—

First, it is not the fact that I have made any attack on geologists. The fathers of British geology were among my dearest and most intimate friends, and I have the highest respect for many of the (comparative) specialists among whom, by the division of labour, the science is now divided. Among the most eminent of these I have always reckoned Prof. Judd himself.

Secondly, it is not the fact that I have accused anyone of conscious indifference to truth. I attacked the undue influence of authority in science, and in doing so I used the well-known formula "conspiracy of silence," which, on the face of it, is a metaphorical and rhetorical expression, but which has been used in his latest writings by Prof. Huxley precisely in the same sense, and has been applied by him to the most distinguished scientific body in the world—the French Institute.

Thirdly, it is not the fact that I have challenged discussion on my September article upon "Coral Reefs." I have challenged discussion upon the subject, and on the question of Darwin's theory—of which my paper was a mere popular abstract, and nothing more.

Recent discoveries by the staff of the *Challenger*—the observations of Prof. Semper—the papers of Mr. John Murray, and of Mr. L. Agassiz—and lastly, the recent admirable observations of Dr. Guppy, have, in their combination, afforded ample ground and materials for a review of the whole question; and I have a distinct opinion, which I repeat, that the influence of Darwinian authority and prejudice is one of the causes which has retarded, and is now retarding, any acknowledged solution of the question.

I have heard with extreme regret that Dr. Guppy, the most recent witness to facts irreconcilable with Darwin's theory, has felt compelled to resign his position as member of the London Geological Society—for what reason I do not fully know, but for some reason connected with his views on this subject.

Inverary. ARGYLL.

On some Unapparent Contradictions at the Foundations of Knowledge.

An argument parallel to that by which Mr. Tolver Preston proposes to prove that Space is nothing will prove with equal cogency that Time is nothing. But if Space is nothing and Time is nothing, then he has the choice of two alternatives, neither of which will he find particularly acceptable. If Space and Time are both nothings, they are identical. If Space and Time are not identical, then they are two nothings which differ. What is the difference between two nothings?

I would suggest that Mr. Preston should read Mr. Herbert Spencer's views on "The Relativity of Knowledge," contained in Chapter IV. of "First Principles." On his carefully thinking this out, and understanding it, I am willing to hope that the title I have adopted for this letter may appear to him appropriate to the subject-matter which he has brought under the consideration of your readers. F. Howard Collins.

Churchfield, Edgbaston.

Extraordinary Fog in January 1888, at Shirenewton Hall, Chepstow.

The recent fog has been so remarkable that it seems desirable to record its principal features. From the 7th to the 14th the air was completely saturated with moisture. The most notable feature was that of cold air passing over a warm ground, for from the 11th to the 15th the greatest cold on the grass did not descend to that read at 4 feet. Such a condition of the air as this has not been noticed since I commenced observations in 1838.

The following readings of the thermometers will illustrate this:—

Date.	Temp. 4ft. 10 a.m.	Diff. bet. wet and dry.	Min. 4 ft.	Min. grass.	Diff.
Jan.	°	°	°	°	°
7	43·3	0·0	40·0	34·5	−5·5
8	45·6	0·0	42·1	37·2	−4·9
9	41·2	0·0	37·0	30·3	−6·7
10	34·3	0·0	32·8	28·1	−4·7
11	36·7	0·0	33·3	37·3	+4·0
12	29·8	0·0	29·3	30·5	+1·2
13	28·3	0·0	26·7	28·3	+1·6
14	32·0	0·0	25·0	29·5	+4·5
15	33·9	1·0	28·0	30·0	+2·0
16	30·0	0·5	27·0	27·0	0·0
17	30·5	1·0	29·7	29·7	0·0
18	28·6	0·0	27·0	27·0	0·0
19	31·7	0·4	26·4	24·8	−1·6

Throughout the 12th after 9 a.m. the temperature on the grass was above 32°, whilst it was a frost from the height of 1 foot upwards; at 10 a.m. the temperature on grass was 32°·8, at 4 feet 29°·8, and at 10 feet 28°·6.

The fog lasted from the evening of the 6th till 3 p.m. of the 14th. On the 7th the clouds moved rapidly in W. current, and on the 8th they moved rapidly in S.W. current; on the 9th nearly calm and cloudless overhead; from the 10th to 14th overcast (except from 11 a.m. on the 12th till 12.40 p.m.). The chief direction of the wind was: 8th S.S.W., 9th S.S.E., 10th W.S.W., 11th and 12th calm, and from 13th to 18th between N. and N.E., and on the 19th E.S.E.

The fog was wet and yielded much moisture, viz.:—

7th ·079, 8th ·008, 9th ·015, 10th ·017, 11th ·031, 12th ·013, 13th ·020, 14th ·020, 15th ·023.

The barometer was very high, and almost stationary, reaching a maximum on the 9th at 10h. 30m. a.m., viz. 30·75 inches corrected and reduced to the sea-level.

On the 11th the fog cloud moved in a south current till 3 p.m., when it became north, and continued so throughout the 12th. On this day on the side facing the fog current every leaf and twig had a horizontal deposit of ice, increasing in length from half an inch at 4 feet above the ground to fully an inch at 10 feet; the outside edge of this ice being as thin as the fine edge of a knife; and the whole upper surface of all laurel and other large leaves that were horizontal had a coating of ice, so thin (although it could be detached without breaking) as almost to resemble gold-leaf, on which were transparent impressions of every irregularity, however minute. On the side of trees opposite to this current, instead of rime there were nearly pear-shaped transparent drops of frozen water, of various sizes, mostly as large as one-eighth of an inch in diameter; they were situated *not quite* at the point of every leaf; no leaf was without a frozen drop, and this had an extraordinary appearance, more especially amongst the crowded leaves of such plants as *Pinus insignis*, *Abies Webbiana*, &c. On the opposite side of these fir-trees the appearance was equally singular, as each leaf looked like a knife-blade of one-sixth of an inch in width, with a square apex. The ground-temperature being above 32°, the vivid green of the grass was a great contrast to the ice on the trees. E. J. Lowe.

"The Art of Computation for the Purposes of Science."

In a paper with the above title, in Nature, vol. xxxvii. p. 237, Mr. Sydney Lupton refers to some of our work as affording a good example of "the natural tendency of the human mind . . . . to exalt the accuracy of one's own experiments."

The experimental work referred to was a determination by the dynamical method of the vapour pressures of liquid benzene. A curve was drawn to represent these relations; three points were chosen, and the constants for the formula $\log p = a + ba^t$ were calculated. Mr. Lupton finds fault with the number of decimal places given for these constants, and makes three statements which are intended to put the experimental work in as unfavourable a light as possible so as to heighten the contrast with the extreme accuracy of the calculations. Mr. Lupton says: "Nine places of decimals are given with apparent confidence, when (1) only three of the whole number of experiments were made even in duplicate." We do not quite understand this statement, for on reference to the original paper (*Phil. Mag.*, Jan. 1887) it will be seen that the last six experiments in Series I. overlap the first six in Series II., while the last seven of Series II. are within the same limits of temperature as the first four of Series III. The second statement is that "the last pressure, 755, was obtained not by experiment at all, but by extrapolation from a freehand curve, the highest experiment being 79°·6 and 743·1 mm." We would point out that the experiment referred to is not the highest, for on the preceding page in our paper the boiling-point 79°·9 at 753·4 mm. is given. Again, the curve was not drawn by freehand, but by means of engineers' curves, which give very much more accurate results. It is quite true that the last pressure was obtained by extrapolation, but an extrapolation of 0°·1, or even of 0°·4 does not seem very excessive with a range of 80°. Mr. Lupton states, thirdly, that "a difference of $\frac{1}{9}$° at low temperatures produced no change in pressure which was appreciable by the apparatus used." But, as a matter of fact, at 0° a difference of 0°·1 corresponds to a difference of pressure of 0·15 mm., which is quite appreciable on our gauge. Perhaps, however, Mr. Lupton refers to the experiments at 36·15 mm., in which at the same pressure two *different* thermometers registered temperatures which differed by $\frac{1}{9}$°.

Mr. Lupton lastly gives much simpler constants, calculated from our data, and compares the pressure at 60°, calculated from them and from our constants, with the pressure given by Regnault. It happens that the number obtained with the simpler constants exhibits greater concordance with Regnault's value. Now while we would agree with Mr. Lupton in classing Regnault (as far at least as some of his work is concerned) with the select few who are entitled to an extra number of decimal places, yet we would point out that Regnault did not always succeed in obtaining perfectly pure substances to work with, and some of his results are rendered almost valueless on that account. In this case, for instance, the melting-point of Regnault's sample of benzene was 4°·44, whereas after the most careful purification we find that it melts at 5°·58, and the value obtained by Fischer (*Wiedemann's Annalen*, xxviii. 400) is almost exactly the same as ours. Again, Regnault failed to observe the existence of a difference in the vapour pressure of solid and liquid benzene (and other substances) at the same temperature, while this difference has been measured by Fischer by the statical and by ourselves by the dynamical method.

We are quite willing to admit that our decimal points are carried further than is necessary for the calculation of the vapour pressures, but we have frequently had occasion to calculate the values of $\frac{dp}{dt}$ for various substances, and we have found that in order to obtain regular values a large number of decimal places are required; if a smaller number are employed the

values of $\frac{dp}{dt}$ themselves require smoothing, which involves additional labour.

But if—since vapour pressures only are given in our paper—we have gone to one extreme, we think that Mr. Lupton has gone to the other, for at 79°·9 the pressure calculated from his constants differs by 3·8 mm. (= 0°·16) from that calculated from ours, and by 3·1 mm. (= 0°·13) from our observed pressure, and this difference is certainly too great.

It might also be supposed from Mr. Lupton's constants that the value of b in the formula $\log p = a + b\alpha^t$ could generally be expressed by a very simple number such as the one he gives (− 3·3), but this is not so. It happens that our constant differs only very slightly from the number − 3·3; it is − 3·30062, and by striking off the two last figures in this constant and making a corresponding slight alteration in the value of a, a much greater simplification is possible than would usually be the case.

Mr. Lupton gives five decimal places for log α, and we are unable to appreciate the advantage of using a table of four-figure logarithms where five places are required.

While recognizing the advantage of methods of computation, may we suggest, in conclusion, that, as a rule, only experimentalists are capable of judging the limits of accuracy of experiment, and that they may be trusted to save themselves trouble where trouble may be saved without sacrificing accuracy?

W. Ramsay.
Sydney Young.

"The Mammoth and the Flood."

The question raised in my previous letter is too important and is being too widely discussed to allow me to let it go by default, and as it has a certain freshness I cannot help thinking that it will prove interesting to many of your readers.

Your critic disposes of Sir Andrew Ramsey in a very unceremonious fashion. To describe the head of the Geological Survey, and the former President of the British Association and the Geological Society, as *an irrational uniformitarian* is to get rid of my attack in a very simple way. Surely some of his scholars or some of his subordinates will have a word to say for their late chief, and, if they cannot maintain his position, will offer some alternative. To the great mass of scientific men who are not geologists, teaching from such a source is naturally accepted as authoritative.

To pass on, however. Your critic speaks of my invoking a series of catastrophes to explain the difficulties surrounding the extinction of the mammoth. This is most inexplicable to me, and points to his not having read my book at all, which was neither fair to you nor me. My book is a perpetual protest against such a series of catastrophes, and an argument in favour of one catastrophe only. May I quote one statement among others?

"If we are to summon some normal cause not now operating for these facts, it certainly seems more reasonable that, with effects so completely alike over such a wide area we should summon *one cause*, and not *several*, and attribute the aberrant conditions showing so much uniformity to some uniform impulse. Here, again, the burden of proof is upon those who deny this view, and treat the remains not as the result of some widespread catastrophe, but as evidence of as many catastrophes as there are skeletons.

"It would be as unreasonable to invoke a separate storm and a separate date for the death of each one of the myriads of razor-bills and guillemots that strewed the western coasts of Britain on a fatal occasion a few years ago, and whose remains were all fresh and in the same condition, as to do the same for the myriads of fresh skeletons of mammoths, rhinoceroses, bisons, &c., in Siberia or in Europe. These debris of a former world have every sign that they formed parts of a contemporaneous fauna destroyed at one time, and are not the wreckage of centuries of deaths."

I now come to what is more important; namely, the theory which your critic resuscitates, after it has been given up by all the Russian inquirers, save one, for many decades—namely, the notion that the mammoths have been floated by the rivers from some undefined land and buried by river action, where they are now found.

Dr. Bunge, who has recently returned from a protracted residence on the Lower Lena, and has described his researches in detail before the St. Petersburg Academy, tells us expressly that mammoth remains are found very seldom indeed in the delta of the Lena, and very seldom also near that river. It is in the higher land separating the great rivers that the remains abound, and especially, as Wrangell and others showed long ago, and as Bunge has recently confirmed, in the mounds and low hills of the tundra. When found near rivers, it is near the short rivers, like those of North-Eastern Siberia, or near the head streams of the Lena, the Yenissei, &c., which could not float such carcasses.

In the next place, Northern Siberia is not a country of mountains and small valleys, but a vast, continuous, nearly level waste covered with moss, called a tundra, diversified by mounds and rounded hillocks, and threaded here and there by rivers running in deep channels—rivers which are frozen fast for a large part of the year.

When the late spring comes, and the ice in the upper reaches melts, while that lower down is still locked fast, there is no doubt a considerable flood in the estuarine parts of the Obi and other rivers, but this is temporary and transient, and it only covers the low lands where mammoth remains are most infrequent. It never covers and cannot cover the higher land. There is not supply of water to do it. To cover the higher points where the mammoth and other remains abound would require such a supply as would put the whole northern part of the continent under water, and thus destroy all animal life there every spring flood. Even if we could postulate river floods of this kind as I have shown, quoting a most experienced geologist, Schmidt, the Siberian rivers deposit no warp that could cover in the mammoths as they are found covered in, by deep beds of clay and gravel, not when lying on their sides only, but when standing upright, as they have several times been found. They must have been covered in by more than two yards of deposit also in a single year in all parts of Siberia, since the ground melts to that depth in the summer, which melting would destroy their soft parts. Appeals to river-floods therefore involve appeals to transcendental causes which are obsolete in other sciences than geology.

Lastly, why is this river portage invoked at all? We have not merely the mammoth carcasses to account for, but the trees found with these great beasts *still rooted*, and the land and freshwater shells showing a different climate when he lived.

Where are we to bring these debris of a former life from? We cannot go outside of Siberia; for the mammoth, so far as we know, has never been found in Asia outside that province. We cannot bring the mammoths found in Kamchatka, and the peninsula of the Chukchi, and the Liachov Islands (which are 150 miles from the mainland), from Central Siberia. Again the remains are very infrequent there compared with their abundance further north, while the mammoths from the north and south of Siberia can be discriminated. There is no sign of rolling on the bones, and the epiphyses are still attached. Evidence of every kind converges therefore upon the conclusion that the mammoths lived and died where their remains are found, and the problem that has to be faced is, how they were exterminated simultaneously from the Obi to Bering Straits, of all ages and sizes, and mixed with various incongruous beasts; how they were buried in the hillocks and high ground under vast, undisturbed, and continuous beds of gravel and clay; and how, lastly, their flesh was subsequently preserved. If all this can be explained without some appeal to the forces I have invoked, then *one factor out of many* in my argument can be answered. If not, it is no use going to Wonderland for hypotheses which only arouse ridicule among students of those sciences which claim induction for their basis. I am most anxious for an answer. Henry H. Howorth.

Bentcliffe, Eccles.

Is Hail so formed?

I noticed here yesterday a curious phenomenon—one that has not before come under my observation.

I was standing under a pine-tree that was laden with moisture from the foggy atmosphere; drops were falling to the ground from the branches, but what struck me was the fact that although most of the drops reached the ground in a liquid state, some of them were converted in their descent into *pellets of ice*.

It was very cold, but I had no reliable means of ascertaining the temperature at the time; it could not, however, have been far off freezing-point.

I was quite unable at first to account for the fact that some drops were frozen while others were not; it occurred to me later, however, that the drops which reached the ground as pellets of ice had been derived from the *topmost* branches, while those remaining uncongealed had fallen from the *lower* ones.

I based my conclusion on the assumption that the drops from the top of the tree in falling a greater distance, and in travelling more rapidly, than those beneath them, consequently suffered a greater loss of heat by more rapid evaporation, and hence were converted into ice before reaching the ground; but it seems to me nevertheless a most remarkable thing that such a result should depend upon so small a difference in altitude (10 ft. at the most), and the atmospheric conditions favourable for the production of such a phenomenon must have been so unusual as to make its recurrence very unlikely.

I have heard of a railway train becoming coated with ice after travelling through an atmosphere above freezing-point and laden with mist, but we can easily grasp the phenomenon when occurring on so large a scale.

CECIL CARUS-WILSON.

Bournemouth, January 14.

"British and Irish Salmonidæ."

ALTHOUGH calling in question statements made by reviewers is generally a thankless task, still, when an author believes himself to have been misquoted as well as erroneously corrected, a deviation from the usual course may sometimes be excusable. Acting under such an impression, and feeling sure that the Editor of NATURE, and the reviewer of my "British and Irish Salmonidæ," would be equally unwilling to promulgate errors to the public, I must ask for a small space with reference to the review of my work which appeared in your last issue.

Purporting to quote a sentence of mine (p. 31) as an example of my "originality in sentence construction," the reviewer has rendered it misleading by omitting five words which I have here re-inserted in italics and within brackets. Alluding to the water containing the recently expressed eggs and milt, he makes me say as follows:—"This is gently stirred with the hand (*and then allowed to stand*) until the eggs harden, or 'frees' as it is termed, being a period from one-quarter to three-quarters of an hour," &c. If newly expressed eggs and milt were thus stirred up from fifteen to forty-five minutes they could not "set," and would therefore have no occasion to "free," as the Americans have termed it, but such misplaced energy in the operator (which I never proposed) would assuredly destroy their vitality.

The reviewer says, "the description in the text of the mode of packing eggs which has been perfected at Howietoun seems to be erroneous, . . . while in a quotation in a footnote the correct account is given—namely, that the ova lie in direct contact with the damp moss, and are covered by another layer of the same, the muslin being only used in order that the layer of moss may be lifted and moved." The reviewer has here confused the text, or general principles as laid down, with the note (p. 42) of the mode pursued at Howietoun, which he asserts to be "the correct account"; but had he read the quotation to the end he would have seen that, besides the plan adopted at Howietoun for packing eggs going *long distances* when no muslin is used, a second mode is employed for those going *lesser journeys*, and was described as follows: "For shorter journeys eggs are thrown off the frames on to swans' down, which takes little more than half the time, and greatly facilitates the unpacking at the end of the journey."

The reviewer observes that "no reference is given to any work where the correct description of *S. namaycush* as a char can be found." If this remark is seriously made under the impression that the fish is not a char or a *Salvelinus*, I would refer among others to *Salvelinus namaycush*, Jordan, Bull. 16, U.S. Mus. 1883, p. 317; Bean, "Fish Com. Report," 1884, p. 1042; Garman on the "American Salmon and Trout," Boston, 1885, p. 5; to Brown Goode in his "Game Fishes of the United States," and his more recently published account in the "Fisheries and Fishery Industries of the United States," 1884, p. 485, &c. In this last work he observed of the *namaycush* that "the Lake trout is in fact a member of the same group of the salmon family with the chars," while I referred to his statements at p. 249.

FRANCIS DAY.

Cheltenham, January 14.

PHYSICAL SCIENCE AND THE WOOLWICH EXAMINATIONS.

IN June 1884 we called the attention of those who are interested in science and in the science-teaching in our public schools to some new regulations for admission to Sandhurst which had lately been announced, and to efforts that had been made by the President of the Royal Society, and others, to induce the authorities at the War Office to reconsider their scheme, which appeared likely to seriously handicap those public schools in which real attention to science is given in the regular school work, and to be unjust to young men of scientific ability.

Whilst we wrote, those regulations were already undergoing revision, and they were subsequently replaced by others in which certain improvements had been made, but in which the mark value of science was still so low as to be likely to do harm. In a second notice of the subject in August of the same year, whilst admitting that improvements had been effected, we expressed our opinion that even in their new form the regulations would tend to check freedom and progress in education, and act unfavourably on the work of those public schools which have aimed at widening the basis of education by introducing the study of physical science into the regular school work of all, or nearly all, their pupils.

We regret to add that this view has proved to be, to a considerable degree, correct. We hear, for example, that almost directly after the issue of the amended regulations at least one large school decided to omit all work in science from the instruction given to boys *at once upon their deciding to become candidates for Sandhurst.* In the interests of the subsequent career of the boys this was, and is still, considered to be almost invariably necessary. And we find that at the last four examinations only about 2 per cent. of successful Sandhurst candidates have offered a knowledge of some branch of physical science ("experimental science" in the regulations), whereas formerly the very moderate but much larger proportion of 8 per cent. did so. In the case of physical geography and geology the corresponding proportions are 19 per cent. during the four years that preceded the date of our article, and about 8 per cent. during the last two years.

No doubt the candidates for Sandhurst are not, as a rule, drawn from the class of boys to whom the study of science is particularly attractive, and it is not impossible that to some extent the present regulations for admission to Sandhurst may have had the effect of inducing scientific boys to enter more freely for the scientific branches of the army, to which admission is gained through Woolwich.

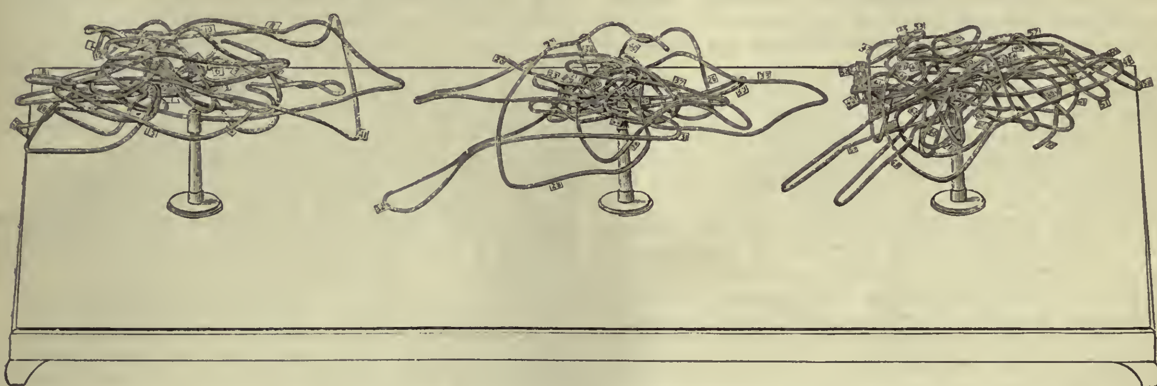
In the examinations for Woolwich, science has hitherto met with more liberal treatment than at Sandhurst, and has been taken up by a fair, but not excessive, proportion of the successful candidates, which has lately tended to increase in the case of chemistry and physics. It is therefore with the greatest regret that we learn that new regulations for admission to Woolwich are to come into effect in November, which will be likely to seriously further discourage the teaching of physical science. These regulations correspond pretty closely to those for Sandhurst, which we have previously discussed; it will be sufficient, therefore, to say that compulsory mathematics, optional mathematics, Latin, French, and German, form Class I., have each of them an allotment of 3000 marks; that Greek, English history, chemistry, physics, physical geography and geology, form a second class, to each of which 2000 marks are allotted; and that candidates may take all the subjects of Group I.,[1] or may substitute one subject from Group II. in place of one of those in Group I.

[1] They may also take any or all of Group III., viz. English composition, freehand and geometrical drawing, to each of which 500 marks are allotted.

Hitherto the mark values of all subjects, except mathematics, have been equal in the Woolwich examination, and free choice of subjects has been permitted to candidates. This has been fair to young men of different orders of ability; it must have secured officers of varied powers, and has satisfied the schools by leaving them free to do for each boy that which was best for him. In one respect the new scheme is better than the old—viz. in the grouping of the physical science subjects. But with such a bribe as will now be offered for Latin and modern languages, we cannot think that it will often be worth while even for boys of more than average scientific capacity to adopt the study of science if they desire to enter Woolwich. It is evident that, other things being equal, those who do so will come out lower in the list of those who succeed, and be more likely to find themselves amongst those who have failed, than will be the case with such as are of equal ability in the study of languages. We do not believe that it is the intention of the War Office authorities thus to partly bar the way into the scientific branches of the service against young men of more than average promise in the experimental sciences, subjects that will afterwards form a very important a part of their work in the Royal Military Academy; and we trust that leaders in science, and the representatives of those schools which are doing their best for their scientific boys, as well as for their unscientific boys, will not fail to unite in calling attention to the inevitable results of the final adoption of the present scheme. The reception that such representations met with in 1884, and the position accorded to physical science in the course of study for the cadets after having entered Woolwich, cause us to feel sure that such representations will not be without effect, especially if they be not too long delayed.

A MODEL OF AN EARTHQUAKE.

IN the latest part of the Journal of the Science College of the University of Tokio, Prof. Sekiya describes a very curious and remarkable model he has made to exhibit the manner in which a point on the earth's surface moves during an earthquake. Readers who have followed the recent progress of seismometry in Japan are aware that the motion which is recorded at an earthquake observatory is a prolonged series of twists and wriggles of the most complicated kind, so that the path pursued by a point on the surface of the soil has been aptly compared to the form taken by a long hank of string when loosely ravelled together and thrown down in a confused heap. Prof. Sekiya has taken advantage of a very complete earthquake record obtained by him with a set of Prof. Ewing's seismographs to follow out this path step by step, and to represent it, in a permanent form, by means of stiff copper wire. The earthquake he has modelled in this way took place on January 15, 1887, and was unusually severe, for Japan. It has been already described in NATURE (vol. xxxvi. p. 107), and we have given there a copy of the seismographic record by help of which the model has been constructed. The seismogram shows the vertical displacement and two rectangular components of the horizontal displacement, instant by instant, throughout the disturbance.



Professor Sekiya's Model of an Earthquake.

It was only necessary to go through the laborious task of compounding the three displacements in order to find the actual path. This, Prof. Sekiya has done for the first seventy-two seconds of the earthquake—a period which embraces all the most interesting features, although large movements in a horizontal plane continued for a minute more, and small movements for a still longer time.

After the seventy-second second, however, the vertical component of motion had virtually disappeared, so that the later part of the disturbance might be represented by a curve drawn on a horizontal plane. To avoid confusion, the model (a sketch of which is given above) is constructed in three parts: the first and second parts each refer to twenty seconds, the third to thirty-two seconds. The parts are mounted together on a lacquered stand 3 feet long, genuinely Japanese as to its legs, as the sketch will show. The model represents the absolute motion of the ground magnified fifty times. Little metal labels are attached to the wire to mark successive seconds of time, from 0, where the shock begins, to 72, where the model ends.

Prof. Sekiya is to be congratulated on his patience and skill. The model will serve to show at a glance the real character and enormous complexity of earthquake motion; it may also serve to open the eyes of seismologists of the older school to the perfection to which earthquake measurement has now been brought. We learn by a Japanese advertisement that a native firm (Seirensha and Co., Tokio) has undertaken to sell copies of Prof. Sekiya's model, lacquered stand and all, at a price so low that it should induce many private persons, not to speak of curators of museums and others officially interested in scientific novelties, to possess themselves of this pretty and instructive Japanese "curio."

ANTON DE BARY.

ON January 19, after a painful illness, died Anton De Bary, for many years the Professor of Botany in Strassburg. He had been suffering for some time since his visit to this country in September, and had undergone an operation which entailed the removal of parts of the face, but he did not recover.

He was born in Frankfurt in 1831, and studied in Berlin under Alexander Braun. From an early age he showed extraordinary powers as an original investigator, and was successively Professor of Botany in Freiburg, Halle, and Strassburg, having held the latter distinguished post since 1872. His indefatigable labours as the editor of the *Botanische Zeitung*, since 1867, are well known; and English agriculturists knew him from his admirable exposition of his investigations of the potato disease, in 1861, and in the Journal of the Royal Agricultural Society for 1876.

De Bary's influence on the progress of biology has been enormous, and in attempting to form an estimate of the value of his contributions to science, we must try to picture the state of botany in 1850 or thereabouts, when his labours began.

Little was known of the Thallophytes beyond the apparently endless species-making, which was coming into shape, however, under the discriminating hands of Agardh, Harvey, and Kützing; Fries, Léveillé, Berkely, and Corda: the zoospores of Vaucheria had been seen, and the conjugation of Spirogyra was known. Thuret and Nägeli were at work: Hofmeister was publishing his illustrious work on the embryology of the Phanerogams and Cryptogams: von Mohl was creating a new school of vegetable anatomy.

Surrounded by these influences, De Bary was working at the structure and development of the Fungi causing "Rusts" and "Smuts," and in 1853 he published his first book on this subject: Thuret observed the details of the fertilization of Fucus in the same year.

De Bary was also occupied with the Algæ, and in 1854 published his observations on *Œdogonium* and *Bulbochæte*: Pringsheim's papers on *Vaucheria*, *Œdogonium*, *Saprolegnia*, and *Coleochæte* appeared in 1855-58. The great botanical questions of the day centred around the development of the lower Cryptogams.

Then came De Bary's researches on the *Conjugateæ*, published in 1858, where the essentials of sexual reproduction are described with wonderful accuracy; and this was followed by his observations on the germination of *Lycopodium*, a piece of work so good that although we have only come into possession of most of the remaining facts quite recently, his old figures have been found worth reproducing.

But, as is well known, De Bary abandoned this newer pursuit of the green plants to return to his earlier love, the Fungi; and from about 1860 onwards he sent forth memoirs and books into the world of a nature to shake the tottering hypotheses of the day to their foundations, building up in their place the beginnings of what is rapidly becoming a mighty and coherent superstructure.

Until about 1850 little was known of Fungi beyond the mycelium and spores of the larger forms. The Tulasnes were at work, and had described several of the "Rusts," &c., before De Bary's book came out, and by 1853 the development of the Ergot of rye had been observed. Then followed their brilliant descriptions of the development and germination of the spores of *Cystopus*, *Puccinia*, *Tilletia*, *Ustilago*, and in 1861-65 Tulasne's "Selecta Fungorum Carpologia" appeared.

De Bary was already bringing forward the methods which distinguish his work so eminently from the anatomical method of his predecessors, and by 1863 he had not only cultivated many forms of Fungi, and repeatedly seen the sexual organs of the *Peronosporeæ*, but he showed that the fructification of the *Ascomycetes* is also to be traced back to the interaction of sexual organs. These may be regarded as the starting-points of the long series of researches into the sexuality of the Fungi which have already led to such remarkable results, and with which the names of De Bary and his school are so intimately associated.

In 1864, De Bary published the second edition of his book on the *Myxomycetes* (the first edition was in *Zeitschr. für Wiss. Zool.* 1859), and we ought to point out that the first edition of this work, coming at the time when the observations on zoospores by A. Braun, Thuret, Nägeli, Pringsheim, and De Bary himself, were astounding the botanical world, helped much towards clearer conceptions regarding the "sarcode" of the zoologists, and the protoplasm of the botanists.

This year (1864) also saw the first number of the celebrated "Beiträge zur Morph. u. Phys. d. Pilze," and in 1865 were produced the startling results of his further cultures of parasitic Fungi, in which he showed how—by regarding a parasite as an organism to be cultivated on its proper medium, just as we regard wheat as an organism to be grown on suitable soil—its life-history can be followed without those large breaks in continuity which render so much of the anatomical evidence worthless. By means of these researches De Bary proved the entrance of the parasitic Fungus into the host, and its progress in the tissues, so conclusively that any doubts still lurking on the main subject were for ever dispersed. The importance of these results cannot be rated too highly: they not only entirely altered the position of the agriculturist towards his fungoid enemies, but they introduced a new era in medicine. Their bearings on science were simply beyond valuation. From this point onwards the continuous observations of cultures under the microscope became extensive; and in the hands of those who were not too readily deterred by the technical difficulties and the laborious patience of such researches, there sprang up the beginnings of that knowledge of the diseases of plants which is now taking shape under the action of workers trained by De Bary himself.

Nor was this all. The startling facts of heterœcism were at the same time put before the world, and on such evidence that none could reject the phenomenon: De Bary proved that the so-called *Æcidium* of the Berbery is only a phase in the life-history of the *puccinia* of the rust of wheat. The repeated confirmation of this in later years, and the numerous similar cases which have been discovered since, sufficiently attest the accuracy of the original work; while its practical importance is obvious.

In 1866 was published the first edition of the "Morphologie und Physiologie der Pilze, Flechten, und Myxomyceten," a book which gave definiteness to the scattered knowledge of these organisms, and enabled the scientific world to see clearly the remarkable power of the man. His unflinching honesty and rigorous self-criticism and modesty had already attracted the attention of all who came in contact with him or his work; now, however, was seen the marvellous grasp of details, and the power of logical generalization which he possessed, and thenceforward the name of De Bary was associated with the leadership of the modern school of biologists he was himself creating.

As evidence of his untiring industry, it may be pointed out that not only did he publish the second number of the "Beitrage zur Morph. und Phys. d. Pilze" this year, but he had already taken in hand that monument of laborious investigation and critical reading, the "Comparative Anatomy of the Phanerogams and Ferns," which was not finished until 1877. The years 1869, 1870, and 1871 show indications of his new labours—undertaken, it should be mentioned, because the original plan had been interfered with—in articles in the *Botanische Zeitung*, on the epidermis, on *Cycads*, &c. Nevertheless the third number of the "Beiträge" appeared in 1870, full of new work, and important, on the *Erysipheæ* and *Ascomycetes*.

During 1874 and 1875 he published two papers on the fertilization and germination of *Chara*, and a memoir on *Protomyces*. In 1877 was published his and Strasburger's joint memoir on *Acetabularia*, and the book above referred to—the "Comparative Anatomy of the Ferns and Phanerogams"—was finished. The influence of this work has been enormous: criticism has been cast on the plan and mode of treatment, but probably all botanists

capable of judging are unanimous in praising its extreme accuracy, justice, and completeness. 1878 and 1879 saw the publication of the essays on Apogamy, and on Symbiosis, two bright and suggestive papers, which have had a wide influence on succeeding work, and which connect De Bary's name paternally with new doctrines in biology.

In 1881 he was busy with the promulgation of his new facts and deductions in connection with the *Peronosporeæ*, and the phenomenon of apogamy in the Fungi. In addition to articles in the *Botanische Zeitung* on the classification of the Thallophytes generally, and of the Fungi in particular, he published extended and important observations on the *Saprolegnieæ* and *Peronosporeæ* (the 4th number of the "Beitrage zur Morph. u. Phys. d. Pilze."), and the philosophical scheme of classification of the Fungi which forms the basis of our present system. Space will not admit of our referring further to his other memoirs, and it is impossible to even mention the numerous illuminating ideas and suggestions which are scattered through his papers, for we must proceed to the passing enumeration of his last two books, either of which would have sufficed for the reputation of an ordinary great man.

In 1884 was published his "Comparative Morphology and Biology of the Fungi, Mycetozoa, and Bacteria," and the best idea of De Bary's influence can be obtained by comparing this work with his "Morph. u. Phys. d. Pilze, Flechten, u. Myxomyceten," published eighteen years previously.

In 1885, De Bary brought together a series of lectures on Bacteria, since published in the form of a book: it is in his best style, and brings before the reader by far the clearest trustworthy general account of this astonishing and fruitful subject. Here, as everywhere, to take a subject in hand was to aid it: had De Bary done no more for "bacteriology" than observe and clearly describe the development of the spores of *Bacillus Megaterium*, his influence would have been felt; and the student is especially indebted for his careful sifting of the literature, and his suggestive indications.

One of his latest efforts was on the subject of infection, particularly with reference to certain *Pezizæ* and Sclerotia: he placed firmly on record the discovery that some of these Fungi may be harmless saprophytes until they have been cultivated—educated up to a higher degree of power—and then they can enter into and destroy a living host, which resisted them previously.

It should also be remembered that he was for many years editor of the *Botanische Zeitung*, and lent his aid to the forwarding of numerous botanical projects.

The above sketch may serve to convey some idea of the labours of the great Strassburg botanist. But, although they give a glimpse of the specialist's results, they afford no insight into his keen appreciation of all good work; of his humorous and never malicious disposition, in the laboratory, and in his writings; and of his sharp, but always just, criticism of anything pretentious. Nor is it possible to enter here into his abundant knowledge of species: he was one of the first to grasp Darwin's teachings, and perhaps never misapplied them. His close acquaintance with species and even local varieties of the plants around Strassburg, at any rate, could only be known to those who have walked with him; and the delight of those walks in Alsace!

As a lecturer he was not brilliant: he appeared shy and nervous when on the daïs, but in spite of his low voice and restless fingers he kept his hearers interested, and always taught clearly. Quaint he often was, in speech and manner, but the impressive truthfulness of his nature, the earnestness of his teaching, and the absence of any striving after effect, gave to his very quaintness a charm and dignity the influence of which will never be forgotten so long as his pupils live. H. MARSHALL WARD.

NOTES.

WE print to-day a leading article on "Odium Medicum." As the questions to which it relates have already been fully discussed in the *Times*, it may be well to state that we do not intend to publish any correspondence on the subject.

SOME time ago the Australian Governments, through Sir Graham Berry, represented to the Home Government the fact that in their opinion much good might be done by an "Antarctic *reconnaissance*," preliminary to an expedition for the thorough exploration of the Antarctic regions. In order that this suggestion might be carried out, the Australian colonies offered to contribute £5000, on condition that a like sum should be given by the mother country. The proposal was supported by the Colonial Office, by the Royal Society, and by the Royal Geographical Society; nevertheless, the Treasury has announced that it does not see its way to the granting of an Imperial contribution. The objects to be attained do not seem to it to justify the payment of even so small a sum as £5000. There will, of course, be much disappointment in the Australian colonies, but it may be hoped that the idea of a joint Antarctic Expedition will not be abandoned. Perhaps a larger scheme than the one which has just been rejected would have had a better chance of success.

THE scientific education of the mining population of Cornwall was for many years in the hands of the Miners' Association of Cornwall and Devon—an institution founded in 1859, at the suggestion of the late Mr. Robert Hunt, F.R.S. Some time ago this body was amalgamated with another Cornish institution, and the united organization took the name of the Mining Association and Institute of Cornwall. A movement has just been set on foot for increasing the efficiency of this Association by the formation of a Museum of Mineralogy, to be established at Redruth, or elsewhere, in the heart of the great tin and copper mining district. It is held that such an institution will in no way interfere with the existing museums in the county—such as those of the Royal Institution at Truro, and of the Royal Geological Society at Penzance. The new museum, instead of seeking to exhibit attractive specimens, will be essentially practical and educational—a place for the earnest student rather than for the casual visitor. It will endeavour to collect characteristic samples of ores, and typical specimens of such other minerals as are of interest to the miner or to the geologist. In recognition of the services which Mr. Robert Hunt rendered to Cornwall by his persistent advocacy of the necessity of giving the young miners a scientific training, it is proposed that the new museum shall bear his name. The Committee appeals for contributions, either in money or in minerals, and for suggestions as to the development of the scheme. Communications should be addressed to Mr. T. C. Peter, Town Hall, Redruth.

MR. J. E. HARTING has been appointed Librarian and Assistant Secretary to the Linnean Society at Burlington House, in the place of Dr. Murie, resigned. Mr. Harting has for some years past been engaged in fulfilling the duties of Zoological Librarian at the Natural History Museum, South Kensington, where he has organized what is now the best zoological library in this country, although possibly not the largest in regard to the number of volumes. The new appointment has been made opportunely at the expiration of the Government grant for the purchase of books at South Kensington, and has given general satisfaction.

THE forty-first annual general meeting of the Institution of Mechanical Engineers will be held on Thursday, February 2, and Friday, February 3, at 25 Great George Street, Westminster. The chair will be taken by the President, Mr. Edward H. Carbutt, at half-past 7 p.m. on each evening. The discussion on Mr. John Richards's paper, on "Irrigating Machinery

on the Pacific Coast," will be resumed. The following papers will be read and discussed, as far as time permits:—"On the Position and Prospects of Electricity as applied to Engineering," by Mr. William Geipel, of Edinburgh; "Third Report of the Research Committee on Friction: Experiments on the Friction of a Collar Bearing."

THE 1888 Conference of the Camera Club, the central institute for amateur photographers, will be held in the theatre of the Society of Arts on Tuesday and Wednesday, March 13 and 14, under the presidency of Capt. W. de W. Abney, F.R.S.

THE eighth annual general meeting of the Essex Field Club will take place at the Public Hall, Loughton, Essex, on Saturday evening, January 28, at seven o'clock. Mr. T. Vincent Holmes will deliver the annual Presidential address, taking as his subject "The Subterranean Geology of South-Eastern England."

A PUBLIC Conference on the Sanitary Registration of Buildings Bill will be held at the Society of Arts, John Street, Adelphi, on Saturday, February 4. The chair will be taken at 4 o'clock by Sir Joseph Fayrer, F.R.S.

A NATIONAL Hydrographical, Meteorological, and Climatological Congress is to be held at Madrid in February.

THE American Society of Naturalists held its annual meeting in the Peabody Museum, New Haven, on December 27 and the two following days. *Science* explains that this Society, composed of professors and specialists, leaving to other scientific associations the function of presenting and discussing results, devotes itself to the publication of new methods, improved apparatus, and aids to science-teaching. The work of the Society falls into two sections—biology and geology—and a day of each meeting is devoted to each of these topics, while the third day is given over to a general discussion on some attractive subject. The attendance at the recent meeting was large, and, according to *Science*, the proceedings were both interesting and profitable.

THE *Monthly Weather Review*, published by the Chief Signal Officer of the United States for October 1887, contains a discussion of the movements of high barometer areas over the North Atlantic Ocean for the year 1885. Fifty-two well-defined areas passed off the coast, of which seven traversed the ocean to Europe, and three moved north-easterly, to the vicinity of Iceland. The average time occupied by the fifty-two anticyclones in advancing from the 90th meridian to the coast was about one day and a half, this rate of progression being considerably greater than the average velocity of cyclonic areas over that region. These areas of high pressure have an important influence on the paths of storms. During October 1887 the paths of sixteen depressions are also traced; four advanced eastward over Newfoundland, one of which traversed the ocean from coast to coast.

THE Meteorological Council have published the observations taken at stations of the second order during the year 1883 (218 pp. large 4to). Observations taken twice a day are printed *in extenso* for thirty stations, and monthly and annual summaries and extremes for forty-four stations. The positions of the stations are shown upon a key-map, but the map also shows that considerable districts in the west of Scotland and Ireland, and even on the east coast, for instance between Dundee and Seaham, are still unrepresented. The barometer observations (reduced to mean sea-level) are given to the nearest ·01 inch, instead of the ·001 inch as heretofore. There is also a useful summary of the hours of bright sunshine for the stations which are furnished with sunshine-recorders, but the yearly values are not calculated.

WE have received a sheet on which are three photographs of the total eclipse of the sun, August 19, 1887, taken at Yōmeiji-yama (long. 138° 59′ 23″ E., lat. 37° 37′ 13″ N., alt. 115 metres), Echigo, Japan, by M. Sugiyama, the observer of the Tokio Observatory, under the direction of I. Arai, the Director of the Tokio Observatory and the chief of the Expedition. The photographs were taken in the following order: L.M.T. 3h. 40m. 36·5s. (1m. 8s. after beginning of totality); L.M.T. 3h. 41m. 25·4s. (1m. 57s. after beginning of totality); L.M.T. 3h. 42m. 6·2s. (34s. before end of totality). In sending us these photographs, Mr. I. Arai writes to us:—"While bad weather prevented nearly all the observations at other stations in our country, I was very fortunate, my station being entirely free from clouds, at least during the totality. But I regret to inform you that, as we were not equipped with complete instruments, and the telescope used was only of small size and not sufficient for photographic purposes, the result was not very satisfactory, because some of the coronal rays, extending outside of the field of the telescope, do not appear in the photographs. I did not, however, like to make the least modification, neither in size, nor in shape, believing that it would be best to leave the actual phenomena just as represented by the photographic apparatus."

SEVERE earthquakes are reported from Ontario and Quebec on January 11, but no damage was done. Shocks are also reported from Columbia (South Carolina), Summerville, and Charlestown. According to a telegram sent from New York on January 23, three shocks had occurred at Newburyport, in Massachusetts.

MESSRS. MACMILLAN have arranged to publish in their "Student's Series" a new biological text-book, "Lessons in Elementary Biology," by Prof. T. Jeffery Parker, of the Otago University, New Zealand. The book will be written on a modification of the "type" system, the earlier chapters consisting of detailed accounts of the morphology, physiology, and life-history of selected examples of the lower organisms. Briefer accounts of important types of the higher animals and plants will be given, but, as the work is intended for the study and not for the laboratory, it will not be necessarily limited to readily accessible forms, and the plan will sometimes be adopted of omitting certain points of structure, development, &c., which from their complexity or aberrant character are unsuited to an elementary work. The book will be written throughout in such a way as to bring clearly before the student the fundamental principles and generalizations of biology, and will be fully illustrated. It is hoped that it may serve to supplement the lecture-notes of a student attending an ordinary junior University course of biology, and, in the case of one working independently, to supply the connected narrative which is not readily obtained in suitable form either in a laboratory manual or in the ordinary text-books of zoology and botany.

THE "Zoological Record" for 1886 has just been issued. For sixteen years the annual volume of this most useful work was issued by the "Zoological Record" Association, which was aided by grants from various sources. At the close of 1886 the Association failed to obtain the renewal of some of these grants; and, being unwilling to carry on the publication of the "Record" any longer, it came to an agreement with the Zoological Society, by which the task was undertaken. The Council of the Zoological Society appointed a Select Committee to superintend the new enterprise, and Mr. F. E. Beddard was made editor. In the preface to the present volume Mr. Beddard explains that the only change he has made is the introduction of a section devoted to general subjects. This includes text-books and works of a general nature, many of which are again recorded under the several groups with which they are more especially concerned.

UNDER the title "A Year's Insect-Hunting at Gibraltar," there appears in the January number of the *Entomologist's Monthly Magazine*, a valuable paper by Mr. James J. Walker,

on the entomology of Gibraltar, concerning which subject next to nothing had previously been written. Mr. Walker, as an officer of H.M. gunboat *Grappler*, stationed there, had ample opportunities for studying the insect-fauna. His observations are mainly confined to *Lepidoptera* and *Coleoptera*. He says there is scarcely a day throughout the year on which butterflies may not be found; and he enumerates fifty-five species for the limited district, thirty of which have occurred on the isolated Rock itself. *Coleoptera* are very numerous, and he has already found 900 species, and is almost daily adding to the number. Apart from its purely entomological interest, the introductory portion is of great value, being a lucid *résumé* in a few pages of the topography of the Rock and the immediate neighbourhood, with sketches of the chief botanical, zoological, geological, and meteorological features, not forgetting the Barbary apes, which, reduced a few years ago to less than a dozen individuals, are now so numerous as to cause serious depredations in the gardens.

WE have received the volume for 1886 of the Journal and Proceedings of the Royal Society of New South Wales. Among the contents may be noted the Presidential Address by Prof. Liversidge, F.R.S.; description of an unrecorded Ardisia of New Guinea, by Baron von Mueller, F.R.S.; a comparison of the dialects of East and West Polynesian, Malay, Malagasy, and Australian, by the Rev. G. Pratt; preliminary notes on some new poisonous plants discovered on the Johnstone River, North Queensland, by T. L. Bancroft; notes on the process of polishing and figuring 18-inch glass specula by hand, and experiments with flat surfaces, by H. F. Madsen; notes on the theory of dissociation of gases, by Prof. R. Threlfall.

A LARGE number of new aromatic fluorine substitution products have recently been prepared by Drs. Wallach and Heusler (*Liebig's Annalen*, Band 243, Heft 1 and 2), the properties of which point to some interesting conclusions regarding the physical nature of fluorine itself. It is found that in all cases the specific gravity of a compound is raised by the introduction of fluorine instead of hydrogen. Thus while benzene at 20° has the specific gravity 0·8846, fluorbenzene, C_6H_5F, at 20° possesses a specific gravity of 1·0236. But, on the other hand, the substitution of fluorine is found to have a remarkably small effect in raising the boiling-point; for instance, fluorbenzene enters into ebullition at 85° C., a temperature only 5° higher than that of boiling benzene. What is, however, still more interesting is the fact that the difference between the boiling-points of corresponding iodine and bromine substitution products, and again between those of bromine and chlorine is smaller than that between the substitution derivatives of chlorine and fluorine. Whilst this difference of boiling-point between corresponding bromides and chlorides amounts to 20–23°, that between chlorides and fluorides approaches 40°. This fact, coupled with the small influence which the substitution of fluorine exerts upon the boiling-point, indicates the interesting probability that the boiling-point of free fluorine itself lies very much below that of chlorine (–33°·5), and that fluorine much more nearly approaches the volatility of hydrogen. Indeed, it appears likely that fluorine is one of the so-called permanent gases, and might form a worthy object for the attentions of those who have been so successful in inducing the other "permanent" gases to reveal their boiling-points; the difficulties in the way would of course be immense, but, in face of what has been done, are not perhaps insuperable. Under all circumstances fluorine attaches itself to carbon with far greater tenacity than any of the other halogens, as was clearly shown by leaving one of the new fluorides, bromfluorbenzene, C_6H_4BrF, in cold ethereal solution in contact with metallic sodium. After eight days a considerable quantity of sodium bromide had formed, but not a trace of the fluoride of sodium. The fluor-compounds themselves form a most valuable contribution to organic chemistry, and fill up a gap which has long been noticeable in the literature of the subject.

MR. J. A. CROWE, Her Majesty's Commercial Attaché for Europe, reports to the Board of Trade that the French Legislature has recently passed a law enacting that a prize will be given to the discoverer of a simple and practical test to ascertain the presence in spirits and alcoholic drinks of substances other than pure and ethylic alcohols. The conditions under which the award is to be made will be determined by the Academy of Sciences of the French Institute.

IN the last number of the *Zoologischer Anzeiger*, Dr. Otto Zacharias earnestly recommends the establishment of a zoological station on a German lake for the observation and study of the freshwater fauna.

THE other day three ladies in India received the degree of B.A.,—two at the University of Calcutta, and one at the University of Bombay.

A SEAM of good coal is reported to have been discovered in Cashmere. An officer of the Indian Geological Survey is to be sent to examine it.

RECENTLY an elk was shot in Galicia. It is now 130 years since the last of these animals was killed in Austria. It is believed that the one referred to had come from Lithuania.

IT is generally believed that the Polar bear cannot be tamed. Last autumn, however, a Norwegian skipper brought one of these bears with him from the Arctic Sea to Tromsö, and it has become quite tame. The bear plays like a dog with the crew of the vessel, and follows its master everywhere. It is nearly full grown.

THE Spitzbergen whale-fisheries have been more remunerative during the last two years than at any time during the past quarter of a century. Last year 1311 animals were killed. The whalers are English, Russian, and Norwegian.

A MAGNIFICENT gift has lately been received by the Ethnological Museum at Leipzig, from Dr. Alphonse Stübel (Dresden), Dr. Wilhelm Reiss (Berlin), and Consul-General Benedix Koppel (London). It consists of a rich collection of articles illustrating the culture and industry of ancient and modern South American races. The collection is divided into two parts: the first being objects belonging to the period before the Spanish conquest, the second being modern. There are many figures, vessels, weapons, and implements of stone and clay, found in the old Columbian, Bolivian, and Peruvian tombs, as well as ancient silver, copper, and bronze ornaments from Ecuador and Peru. The Columbian antiquities, and the ancient gold objects of the Chibchas, are specially noteworthy.

THE additions to the Zoological Society's Gardens during the past week include two Snow Finches (*Montifringilla nivalis*), European, presented by the Lord Lilford; two Cockateels (*Calopsitta novæ-hollandiæ*) from Australia, two Pale-headed Parrakeets (*Platycercus pallidiceps*) from North-East Australia, presented by the Hon. Stormont Finch-Hatton; an African Buzzard (*Buteo desertorum*) from Africa, presented by Mr. Sydney H. Carr, four Barbary Turtle Doves (*Turtur risorius*) from North Africa, presented by Mr. John Biehl; two Herring Gulls (*Larus argentatus*), British, presented by Mr. Thomas A. Cotton; a Common Barn Owl (*Strix flammea*), British, by Mr. Hugh Bromley; a Moorish Gecko (*Tarentola mauritanica*) from France, presented by Mr. J. C. Warbury; two Viscachas (*Lagostomus trichodactylus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE CAPE OBSERVATORY.—The second portion of the data upon which the forthcoming Cape Catalogue for 1885 will be founded has recently appeared. The first portion, containing the results of the meridian observations made during the years 1879, 1880, and 1881, was published by Dr. Gill some time ago, and the present volume gives the results from the beginning of 1882 to February 8, 1885, when, the programme for the observation of the fundamental stars of Schönfeld's *Durchmusterung* —which stars will form the most important part of the Catalogue —having been completed, further work with the transit instrument was suspended. An additional reason for the interruption of the meridian observations lay in the desirability of re-polishing the object-glass, and of replacing the micrometer screws of the circle microscopes, which were of gun-metal, by steel screws. The investigation of the errors of the screws used in the present observations forms the most important portion of the introduction, for the effect of wear upon them has attracted Dr. Gill's special attention, and has already formed the subject of a lengthy paper by him in the *Monthly Notices* of the R.A.S., vol. xlv. The transit instruments of the Cape and Greenwich Observatories are almost exactly alike in construction; it is therefore interesting to note that there are evident differences in their behaviour; thus the mean horizontal flexure of the Cape instrument, as determined by the collimators, amounts to nearly half a second—0″·462—whilst that of the Greenwich telescope is almost insensible.

The introduction is followed by 144 pages giving the separate determinations of the various instrumental corrections, the readings of the transit-circle thermometers, &c. The ledgers and catalogues for the years 1882, 1883, and 1884 occupy the remaining 400 pages, the catalogues for the three years containing respectively 863, 444, and 1301 stars, reflex or sub-polar observations of stars being counted separately.

THE PARALLAX OF MARS.—We have received a letter from Mr. C. G. Stromeyer, calling attention to the fact that Mars is stationary on March 4, and urging the desirability of determining its parallax by the diurnal method, the rather that it will then be near two sixth-magnitude stars, as will be seen by the following positions:—

	Mag.	R.A. h. m. s.	Decl. ° ′ ″
95 Virginis	... 6	... 14 0 48	... 8 46 47 S.
94 Virginis	... 6	... 14 0 22	... 8 21 27 S.
Mars	..	... 13 56 16	... 9 2 20 S.

Unfortunately, however, the parallax is small—only 11″·3, and only part of this is practically available for the diurnal method, as the planet cannot be observed through a longer period than eight hours at the utmost.

THE LONGITUDE OF ODESSA.—The *Astronomische Nachrichten*, No. 2820, gives the result of the determination, by Dr. E. Becker and Prof. Block, of the difference of longitude between Berlin and Odessa, which was carried out in July and August 1876 by the telegraphic method. The deduced distance in longitude of the centre of the axis of the Repsold meridian-circle of the Odessa Observatory to the east of the centre of the great dome of the Berlin Observatory is given as 1h. 9m. 27·29s.

THE WINKLER OBSERVATORY.—Herr Winkler notifies, in No. 2821 of the *Astronomische Nachrichten*, the transference of his private observatory from Gohlis, near Leipzig (N. lat. 51° 21′ 35″·1; long. E. from Greenwich, 0h. 49m. 29·65s.), to the neighbourhood of Jena. The growth of the city of Leipzig rendered the old site no longer a favourable one for observation. The transit-instrument and small 4-inch refractor are already temporarily mounted. The co-ordinates of the new observatory are taken at present as being N. lat. 50° 55′ 35″·6; long. E. from Greenwich, 0h. 46m. 20·8s. Herr Winkler publishes at the same time some observations of occultations and eclipses of Jupiter's satellites made in the first half of 1887, which were the last observations made at Gohlis.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 JANUARY 29—FEBRUARY 4.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 29

Sun rises, 7h. 45m.; souths, 12h. 13m. 19·9s.; sets, 16h. 41m.: right asc. on meridian, 20h. 46·1m.; decl. 18° 0′ S. Sidereal Time at Sunset, 1h. 14m.

Moon (at Last Quarter on February 4, 19h.) rises, 16h. 20m.*; souths, 0h. 15m.; sets, 8h. 1m.: right asc. on meridian, 8h. 46·0m.; decl. 17° 56′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	8 16 ...	12 46 ...	17 16 ...	21 18·8 ...	17 39 S.
Venus ...	5 20 ...	9 23 ...	13 26 ...	17 55·5 ...	21 47 S.
Mars ...	23 32*...	5 0 ...	10 28 ...	13 31·9 ...	7 2 S.
Jupiter ...	3 13 ...	7 30 ...	11 47 ...	16 1·6 ...	19 45 S.
Saturn ...	15 51 ...	23 45 ...	7 39*...	8 19·8 ...	20 6 N.
Uranus ...	23 1*...	4 33 ...	10 5 ...	13 4·5 ...	6 9 S.
Neptune..	11 29 ...	19 8 ...	2 47*...	3 41·6 ...	17 54 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Jan.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
29 ...	7 Leonis ...	... 6½ ...	17 41 ...	17 55	... 332 298
29 ...	ψ Leonis ...	... 6 ...	21 13 ...	21 24	... 320 300
Feb. 3 ...	80 Virginis	... 6 ...	3 33 ...	4 44	... 38 256

Feb. 3 ... h. 8 ... Mars in conjunction with and 2° 50′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4 ...	81 16 N. ...	Jan. 30,	21 0	*m*
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,, 30,	3 8	*m*
U Monocerotis ...	7 25·5 ...	9 33 S. ...	,, 30,		*M*
U Hydræ	10 32·0 ...	12 48 S. ...	,, 29,		*M*
R Crateris	10 55·1 ...	17 43 S. ...	Feb. 2,		*M*
δ Libræ	14 55·0 ...	8 4 S. ...	Jan. 29,	19 25	*m*
			Feb. 1,	3 16	*m*
U Boötis	14 49·2 ...	18 9 N. ...	Jan. 30,		*m*
U Coronæ	15 13·6 ...	32 3 N. ...	Feb. 1,	4 44	*m*
S Serpentis... ...	15 16·4 ...	14 43 N. ...	Jan. 31,		*M*
U Ophiuchi	17 10·9 ...	1 20 N. ...	,, 29,	23 57	*m*
		and at intervals of		20 8	
X Sagittarii... ...	17 40·5 ..	27 47 S. ...	Feb. 2,	5 0	*m*
U Aquilæ	19 23·3 ...	7 16 S. ...	,, 4,	5 0	*m*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	Jan. 29,	19 0	*m*
			Feb. 2,	4 0	*M*
Y Cygni	20 47·6 ...	34 14 N. ...	Jan. 31,	20 22	*m*
			Feb. 1,	20 15	*m*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

GENERAL PRJEVALSKY has begun to print his narrative of his fourth journey in Central Asia. It is expected to appear in May, and we may hope that it will find an English translator.

WE are glad to learn that the French explorer of the Gran Chaco, M. Thouars, is safe. The Bolivian Government succeeded in rescuing him from a perilous position among hostile Indians.

A SCOTCH merchant captain, Mr. Strachan, has just returned from New Guinea, many hitherto unexplored parts of which he seems to have visited. It is expected that he will be able to give information that will seriously modify the cartography of the Fly River region. He maintains that the forests in New Guinea are confined to a fringe along the banks of the rivers, and that the bulk of the interior is covered with grass. Captain Strachan has brought home with him a young Papuan boy.

THE steamer *Essex*, of the United States Navy, has been making a series of soundings between Cape Guardafui and Ceylon. In the Indian Ocean, between 60° and 70° E. long., a uniform depth of about 2000 fathoms is almost constantly met with, gradually decreasing as the coast is approached. The greatest depth met with was 2705 fathoms, off the coast of Africa, 160 miles from Cape Guardafui. To the east of this

maximum, the sea-bed rises suddenly to a depth of only 857 fathoms below the surface.

THE new part of the *Mittheilungen* of the Hamburg Geographical Society contains several papers of interest. Dr. Sievers concludes the long series of papers describing the results of his journeys in Venezuela with some remarks on his original route map of the Venezuelan Cordilleras, which are accompanied by an admirable reproduction of this map. In addition to this, Herr Frohberg arranges and discusses the barometric results obtained by Dr. Sievers. Dr. Zintgraff describes the Lower Congo from Banana to Vivi, and insists on the importance of the Congo for the exploration of the region behind the German Cameroons protectorate. Herr Weisser gives a fairly complete account of German New Guinea and the Bismarck Archipelago; and Herr Hernsheim does the same for the Marshall Islands.

As a supplement to the Indian Survey Report for 1885–86, there has just been issued the narrative of the journey of a native explorer, M—H, through Eastern Nepaul into Southern Tibet, as far as the town of Dingri, and westwards and southwards through Central Nepaul. M—H has succeeded in rectifying in many points existing information on the hydrography of the region traversed, and gives many useful notes on its physical geography and its flora, as well as on the people.

OUR ELECTRICAL COLUMN.

THE additional facts added to our knowledge of electricity in 1887 are not very numerous, but the impetus given to its practical applications was very encouraging. One of the most important scientific discoveries was that of Prof. J. J. Thomson, which formed the subject of the Bakerian Lecture, viz. that sparks in tubes dissociated iodine, bromine, and chlorine. In iodine the dissociation produced at 214° C. was as much as that effected directly by Victor Meyer at 1570° C.

PROF. EWING showed that there was apparently no limit to the magnetization of iron in strong magnetic fields when we increased the magnetizing force, and Prof. Roberts Austen showed that it was impossible to separate the elements of alloys by means of electric currents.

IMMENSE improvements have been made in the construction of dynamos, motors, accumulators, and secondary generators, and in consequence electric lighting and working of railways and tramways are upon a commercial and useful stage. Many other causes besides restrictive legislation have retarded electric lighting in England, but there are now many signs that this useful industry is in more senses than one about to commence a very bright career.

SEVERAL useful constants have been added to our notebooks during the past year. Dr. John Hopkinson is pursuing his examination of the specific inductive capacity of oils and other liquids.

MR. PREECE has determined the coefficient of self-induction of straight iron telegraph aërial wires to $0{\cdot}005 \times {\cdot}10^9$ centimetres per mile, while that of copper wire is practically *nil*. He has also measured the current which will just actuate a Bell telephone, and he found it to be 6×10^{-13} ampere.

THE application of powerful electric currents to smelting, as in the Cowles process for producing aluminium, and to welding, as proposed by Elihu Thompson, is gaining rapid progress, while the use of enormous dynamos for the deposition of pure copper from impure ores is gaining ground with giant strides. Messrs. Bolton, at Widnes, and Messrs. Vivian, as well as Messrs. Lambert at Swansea, are each depositing from forty to fifty tons of copper per week by currents of from 5000 to 10,000 amperes.

THE Society of Telegraph-Engineers and Electricians has decided to change its title to that of the Institution of Electrical Engineers—a change for the better. Mr. Graves, the new President, gave an exceedingly interesting address on the industrial importance of electricity, and he brought out the remarkable fact that there are at least 300,000 persons in the United Kingdom depending upon electrical industry for their daily bread.

SOME of our prominent workers in the field of electricity, such as Lord Rayleigh, Sir William Thomson, and Prof. Hughes, are conspicuous by their absence during the past year, although the two former have been by no means idle in other directions.

A NOTE ON VALENCY, ESPECIALLY AS DEFINED BY HELMHOLTZ.[1]

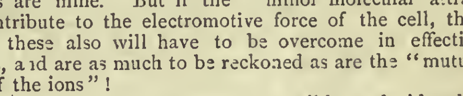
VERY little has been either said or written of late on the subject of valency—not because the topic is admitted to be exhausted, nor because our views can be regarded as reposing on a fixed basis of fact, but more I believe on account of the feeling being almost universally entertained that little is to be gained by continuing the discussion from our present standpoint.

My purpose in this note is to call attention to the extreme importance of reopening the discussion on account of the intimate bearing that it has on the work in which the Electrolysis Committee, jointly appointed by Sections A and B, are now engaged; and to urge that it is time that the gage thrown down by Helmholtz in the Faraday Lecture (Chem. Soc. Trans., 1881, p. 277) was uplifted by chemists.

We are told by Helmholtz that it is a necessary deduction from the fundamental law of electrolysis established by Faraday, that definite, as it were atomic, charges of electricity are associated with the atoms of matter; that, in fact, a monad bears a single charge, a dyad two, a triad three; and that when combination occurs the charges are still retained by the atoms but neutralize each other—"the atoms cling to their charges, and opposite electric charges cling to each other." I cannot help thinking, however, that Helmholtz deprives his statement of much of its force and simplicity by adding: "But I do not suppose that other molecular forces are excluded, working directly from atom to atom." He is led to do this apparently by being aware of the distinction which it is usual to draw between atomic and molecular compounds. The attempt should at all events be made—and in my paper on "Residual Affinity" I have already ventured the first step—to include both classes of compounds, molecular as well as atomic, in the discussion; indeed it is somewhat difficult to reconcile the passage above quoted with the following statement which occurs previously in the lecture: "The law of the conservation of energy requires that the electromotive force of every cell must correspond exactly with the total amount of chemical forces brought into play, not only the mutual affinities of the ions, *but also those minor molecular attractions produced by the water and other constituents of the fluid.*" The italics are mine. But if the "minor molecular attractions" contribute to the electromotive force of the cell, then conversely these also will have to be overcome in effecting electrolysis, and are as much to be reckoned as are the "mutual affinities of the ions"!

It is obvious that if it should prove possible to decide what number of charges are necessarily associated with any particular atom, the conception of valency will have acquired a definiteness which cannot possibly be attached to it as long as the views that have hitherto guided us are adhered to. A decision must involve the discussion of the question of the existence of molecular as distinct from atomic compounds.

To cast the apple of discord without further preface, I would direct attention to the insufficiency of the evidence on which it is usual to rely as proof that nitrogen, for example, is a pentad; nay more, I would assert that this very evidence should be interpreted as proof that nitrogen is not a pentad. It is commonly held that the behaviour of the alkyl tetra-substituted derivatives of ammonium is such as to negative the idea that these are "molecular compounds" of triad nitrogen, and that it must be assumed that the elements of the binary compound which are added to the ammonia derivative are *distributed* in the ammonium derivative; for example, that in the formation of tetramethylammonium iodide from trimethylamine and methyl iodide the methyl and iodine of the iodide part company and separately attach themselves to the nitrogen, thus:—



But I contend that the properties of tetramethylammonium iodide and hydroxide prove that such is not the case: the iodide, it is well known, can be boiled for hours with the strongest caustic potash solution without undergoing change; there is not a single

[1] A Paper read by Prof. Henry E. Armstrong, F.R.S., in Section B of the British Association at Manchester. Communicated by the Author.

case on record, however, of any haloid compound other than an alkylic compound behaving in this manner; the chlorides, bromides, and iodides of every element except carbon are almost at once converted into hydroxides by such treatment, and a nitrogen iodide would surely be acted on. The behaviour of the iodine is much more nearly that of iodine in methyl iodide, and, it may be said, exactly that of the iodine in iodobenzene; indeed it would seem that in the alkyl-ammonium haloid compounds the halogen is always less easily displaced by the action of alkalies than it is in the parent haloid alkylic compound.

The remarkable resemblance of the tetra-substituted ammonium hydroxides to potassium hydroxide has led to their being regarded as in every respect analogous to this latter, and would appear to preclude the idea that they are molecular compounds of an alcohol with an ammonium derivative. But attentive consideration of their properties will suffice, I think, to show that the apparent discrepancies are not only explicable, but that they actually support the molecular compound hypothesis. Thus it might be said to be improbable that tetramethylammonium hydroxide should behave as a powerful base, and have the same heat of neutralization as potassium hydroxide, if methyl-alcohol were one of its proximate constituents; but it is to be remembered that the salt which results from the action of an acid on methyl-alcohol is liable to suffer reconversion into the alcohol by the action of the water produced in the interchange; also that in many cases the methyl salt is insoluble in water, or nearly so. The heat developed on neutralizing methyl-alcohol therefore falls far short in amount of that which would be evolved if the interchange were complete, and if the product were capable of interacting with water, and perhaps also with itself in the way that apparently is possible in the case of metallic salts. In the case of the tetramethylammonium hydroxide, the action of acids is total as the change is irreversible, or almost so, under the conditions which obtain during the formation of the salt, just as in the case of the conversion of potassium hydroxide into a salt; moreover, the product is easily soluble, even when acids like muriatic are used. Why the methyl-alcohol, or other methyl derivative, retained in the ammonium compound behaves so differently as compared with the unassociated methyl derivative, is a question which, for the present, we must be content to put aside unanswered. I am also of opinion that in discussing their constitution no particular weight can be attached to the mode in which the tetralkylic ammonium hydroxides undergo decomposition when heated, as the products in some cases are an amine and an alcohol, but in others an olefine and water, instead of an alcohol; in the case of the phosphonium salts the diversity is still greater (Chem. Soc. Proceedings, 1886, p. 164). That amines may act as "dehydrating" agents in the manner required if the molecular compound hypothesis be adopted, appears by no means improbable.

What is here stated of the tetramethyl compounds is true of tetralkylic ammonium haloid compounds generally, in the sense that they are all less readily acted on by alkalies than are the parent alkylic haloid compounds; but just as these latter are more readily attacked by alkalies and other agents the more complex the alkyl, so are the tetralkyl ammonium compounds; in no case, however, do they manifest a reactivity at all comparable with that of simple metallic or non-metallic haloid compounds—always excepting those of carbon.

The argument used above would apply equally to the phosphonium and sulphine compounds; indeed with greater force.

In many other respects the behaviour of nitrogen in aminic compounds is altogether peculiar and irreconcilable with the assumption of pentadicity. Thus it is commonly pointed out that the basic properties of aniline, for example, become lessened and ultimately almost annulled by the introduction of chlorine or bromine into the phenyl radicle; and that acetamide, $C_2H_3O \cdot NH_2$, and other similar compounds formed by the introduction of acid radicles into ammonia are all but destitute of basic properties; the power to form ammonium compounds, therefore, is not a simple function of the nitrogen atom, but is largely dependent on the nature of the radicles associated with the nitrogen atom. Other illustrations are afforded by the hydrazines. Thus phenyl-hydrazine, $C_6H_5 \cdot NH \cdot NH_2$, although it contains two atoms of (triad) nitrogen, forms with hydrogen chloride the compound $C_6H_5 \cdot N_2H_3 \cdot HCl$, which crystallizes unchanged from fuming muriatic acid, in which, moreover, it is almost insoluble. Ethyl-hydrazine, however, forms a dichlorhydride, $C_2H_5 \cdot N_2H_3 \cdot 2HCl$, but on evaporating the aqueous solution of this salt a monochlorhydride is obtained; and unsymmetric diethyl-hydrazine, $(C_2H_5)_2N \cdot NH_2$, is a monobase like phenyl hydrazine.

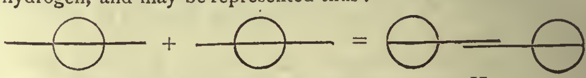
Hence it may well be argued that we have no reason to assume that nitrogen is pentad in the ammonium compounds, or phosphorus pentad in the phosphonium compounds, or sulphur tetrad [1] in the sulphine compounds; but that these are all to be reckoned as molecular compounds.

What then is the valency of the elements in question? and what is a molecular compound?

In answer to the first of these questions, the proposition may be advanced that gasefiable hydrogen compounds are the only compounds available for the direct determination of valency, and that the valency of an element—the number of unit charges necessarily associated with its atom—is given by the number of hydrogen atoms combined with the single atom of the element in its gasefiable hydride.[2] In cases where such hydrides are unknown, the determination of valency is very difficult; it can be but provisionally effected, and only by most carefully weighing all the evidence relating to the constitution of the compounds available for discussion.

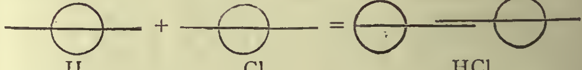
But if it be granted, for example, that nitrogen is a triad, and that iodine is a monad, how are we to explain the fact that the methyl compounds of these two elements unite to form so well characterized a molecular compound as tetramethylammonium iodide? how are such molecular compounds constituted? My own view has long been that the nitrogen and iodine in such a case are both possessed of a certain amount of *residual affinity*; and I would define a molecular compound as one formed by the coalescence of two or more molecules, unattended by redistribution of the constituent radicles, and in which the integrant molecules are united by residual affinities. In other words, the unit charge must be capable in certain cases of directly promoting the association, not merely of two, but of at least three, atoms. To put this hypothesis in terms which cannot be misunderstood, let unit valency or charge be represented by a unit line, and further be it supposed that the charge *penetrates the atom*, then the atom with its unit charge may be represented thus:—

i.e. the unit charge may be held to consist of three portions, the buried portion a, and the free portions $f + f'$. The facts, as they present themselves to me, also appear to necessitate the assumption that, in the case of different elements, the charge penetrates the atom—and in the case of some polyad atoms, different directions in the atom—with varying degrees of freedom.[3] The union of two atoms may then be pictured as an overlapping of the unit lines. If the atoms are freely penetrated by their charges, each atom may tend to move out to the end of the line, leaving either no portion, or but a very small portion, free; a conception of this order would appear to apply in the case of hydrogen, and may be represented thus:—



H. H. H_2.

But if the atom be not easily penetrated by its charge, it will not move out to the end of its line, and the resulting compound molecule will possess more or less "residual affinity;" this conception would appear to apply to the non-metals generally, and to some of the metals; it may be illustrated thus:—



H. Cl. HCl.

I have thought it permissible to state my views in this form merely in order to advance the study of molecular compounds

[1] Probably one of the strongest arguments in favour of the conclusion that sulphur is divalent may be based on its inactivity in the closed-chain compound *thiophen*, which does not unite with methyl iodide, nor does the sulphur in it or its homologues permit of oxidation in the manner that is characteristic of the element in thioethers.

[2] If this be granted, it follows that the maximum number of charges which an atom can carry is four; in other words, that the possible maximum valency is attained in the case of carbon.

[3] This is practically but a modification of Helmholtz's statement that "the phenomena are the same as if equivalents of positive and negative electricity were attracted by different atoms, and perhaps also by the different values of affinity belonging to the same atom, with different force."

by the introduction of a working hypothesis, an *absolutely artificial* mode of expression such as is here adopted being perhaps pardonable in the absence of any explanation which may serve to guide us in extending our inquiries as regards the structure of such compounds, a knowledge of which is all-important to a rational conception of the nature of chemical change generally. Moreover, I do not hesitate to affirm that, from the chemical point of view, it is impossible to adopt the Helmholtz explanation of valency, unless physicists are prepared to grant the possibility of the "division" of the unit charge somewhat in the manner here suggested; and it is in order to impress this that I have ventured to give utterance to these speculations.

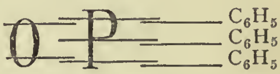
To return to the consideration of the compounds previously referred to, it may be supposed that the nitrogen of trimethylamine and the iodine of methyl iodide are possessed of residual affinity, and hence the two molecules unite to form the molecular compound tetramethylammonium iodide, which may be represented thus:—

The phosphonium and sulphine iodides may be regarded as similarly constituted. It is well known that the ammonium haloid compounds and their analogues are also capable of forming still more complex molecular aggregates with the halogens, &c.: they are therefore to be regarded as possessed of residual affinity; and that polyad elements, *e.g.* nitrogen, phosphorus, and sulphur, should still exhibit residual affinity in such compounds is not surprising in the light of the hypothesis advocated in this note; but it is scarcely compatible with the assumption that the halogen in the ammonium haloid compounds serves as the bond of union. On the other hand, if it be assumed, as I think it should be, that the formation of double metallic chlorides, &c., is the outcome of the possession of residual affinity by the halogen, the complete analogy which appears to exist between the ammonium haloid compounds and those of the alkali metals would seem logically to involve the inference that the halogen of the ammonium compound does not serve as the bond of union. I see but one mode of escape from this conflict of evidence, and that is to call in question the time-honoured assumption that the radical ammonium is the true analogue of potassium and sodium, which, be it remarked, is of necessity subject to doubt if the hypothesis that the ammonium salts are molecular compounds be entertained; and evidence which supports the conclusion that the per-haloid compound is formed by the addition of the halogen to the nitrogen (phosphorus or sulphur) is afforded by the observation that not only *haloid* ammonium and sulphine compounds, but also the *sulphates*, combine with halogens (Dobbin and Masson, Chem. Soc. Trans., 1885, p. 56; 1886, p. 846).

It is now proved by abundant experimental evidence that, whatever the order in which the radicles A, B, C, D are introduced in forming a tetralkylic ammonium compound N(ABCD)X, one and the same end product always results. This is commonly regarded as proof, not only that nitrogen is pentad, but also that the five affinities of the nitrogen atom are of equal value, and it would appear to favour the conclusion that the ammonium salts are in truth "atomic" compounds; but I see no reason why isomeric change should not occur at the moment of formation of a molecular compound—why the integrant molecules, in fact, should not interchange radicles. If the statement be confirmed[1] that the compound formed from dimethyl sulphide and ethyl iodide is different from that obtained on combining methylethyl sulphide and ethyl iodide (Krüger, *Journ. pr. Chem.*, 1876, xiv. p. 193), it will follow, not that sulphur is a tetrad, and that the four affinities are of unequal value, but that there is little or no tendency for isomeric change to occur in the formation of sulphines. The possible occurrence of isomeric change in the formation of molecular compounds, however, is a subject which certainly deserves careful study at the present time.

In the case of phosphorus, the existence of the highly stable gaseous pentafluoride PF_5, discovered by Thorpe, is undoubtedly regarded by many as final proof of the pentadicity of this element; but the existence of compounds such as H_2F_2, HFFR, &c., which clearly belong to the class of molecular compounds, is an indication of so marked a tendency on the part of fluorine to combine with itself, that for this reason alone (as Naumann and others have asserted) the pentafluoride is by no means necessarily regarded as an atomic compound. And I would here add that stability affords no criterion as between atomic and molecular compounds, every degree of stability being met with even among those of the former class.

An argument in favour of the pentadicity of phosphorus which apparently cannot be disposed of by any explanation based on conventional considerations has, however, been advanced by La Coste and Michaelis (*Berichte*, 1885, p. 2118), who have shown that the compounds obtained from diphenylchlorophosphine, $PCl(C_6H_5)_2$, and phenol is not identical with the triphenyl-phosphine oxide, $OP(C_6H_5)_3$, obtained by oxidizing triphenyl-phosphine, as it should be if the latter were a compound of the formula $(C_6H_5)_2P . OC_6H_5$; this last corresponding to the formula $Cl_2P . OCl$, which has been suggested as that of phosphorus oxychloride, and which appears to derive considerable support from Thorpe's observations on the specific volume of the oxychloride (Chem. Soc. Trans., 1880, p. 388). It is, however, conceivable that the oxygen and phosphorus are united by residual affinities, thus:—



Michaelis and Polis (*Berichte*, 1887, p. 52) have argued in the case of bismuth, which also is a member of the nitrogen group, that the pentadicity of this element is proved by the existence of the triphenyl dibromide, $(C_6H_5)_3BiBr_2$. But the mere production of such a compound proves nothing so long as its constitution is undetermined; it at most serves to strengthen the conviction gained from the general study of the element, that bismuth is a member of the nitrogen-phosphorus group.

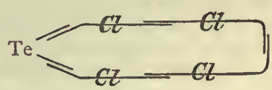
In other cases also it is possible that undue importance may have been attached to the existence of alkylic compounds of particular types: thus lead, judging from its general chemical behaviour, would appear to be a dyad; yet the existence of the tetrethide $Pb(C_2H_5)_4$, is commonly held to be a proof that it can function as a tetrad. But the properties of lead are such that I am tempted to suggest that it is one of the metals in which the "charges" have but a small degree of freedom; and it is conceivable that the tetrethide is actually a compound of dyad lead, each charge serving to bind two ethyl groups, thus:—

The same may be true of tin, although in this case the fact that we are dealing with an element of the carbon-silicon family tend to favour the conclusion that it may be a tetrad.

Also too much importance must not be attached to the existence of stable volatile chlorine compounds: thus tellurium *tetra*chloride may well be a compound of dyad tellurium, thus:—

Iron, and the other members of the family which boron heads, in like manner, I feel convinced, are triads even in their *ic* compounds: recent vapour-density determinations all support this conclusion.

It is even conceivable that chlorine may form *closed-chain* compounds, and that a tetrachloride may exist, such as is represented by the formula:—



I think it is especially noteworthy that so many well characterized and comparatively stable double chlorides exist formed by the union of chlorides of which one at least is *per se* very unstable; the tin-sulphur chloride, $SnCl_4 . 2SCl_4$, and the remarkable series of aurous compounds recently described by Lepetit (*Ann. Chim. Phys.*, 1887, p. 11) may be cited as examples.

[1] The number of *Liebig's Annalen* last issued contains a valuable paper by Klinger and Maassen disproving Krüger's statement.

If my contention in this and previous papers be correct, that residual affinity thus plays a far more important part than has hitherto been supposed, and that it must be taken into account in all discussions on valency, it follows of necessity that our views regarding the constitution of the majority of compounds at present rest upon a most uncertain basis: the constitution of the paraffins, of the benzenes, and of the haloid compounds and alcohols derived from the hydrocarbons of these series, may be regarded as determined with a degree of precision almost amounting to certainty; but in the vast majority of other cases we have as yet no secure method of arriving at conclusions which in any sense approach finality. There can be little doubt that in framing our modern conceptions of valency we have been too much influenced by the graphic symbols which have been so widely made use of. In the future it will be necessary to attach a more liberal interpretation to the facts, and it may be hoped that it will some day be possible also to take into account differences depending on the relation of the different forms of matter to the pervading medium.

The properties of compounds being demonstrably dependent on the intramolecular conditions, it is difficult for a chemist to resist the feeling that the peculiarities manifested by the different elements are also very probably the outcome of differences in structure; such an assumption indeed affords at present apparently the only explanation that can be given of the relationship manifest between different elements when these are classified in groups of "homologues" in accordance with the suggestion originally made by Dumas, which has now found full expression in the so-called periodic system of classification. There appears to be an increasing weight of evidence to favour the assumption that the influence exercised by compounds in cases of chemical change is local in its origin: that it is exercised more by a particular constituent or constituents—in particular directions, in fact—than by the molecule as a whole. The suggestion above made that "affinity" acts in particular directions in elementary atoms, and perhaps with different degrees of freedom in various directions, is therefore but an extension to elements of what is more or less generally recognized as the case in compounds. Some such hypothesis is certainly required to account for the existence of allotropic modifications both of non-metals and of metals; for the remarkable changes in magnetic and other properties which iron undergoes with change of temperature; for the different values of the dielectric constant—along the several axes in sulphur crystals; for the difference in electric conductivity of bismuth in two different directions in bismuth crystals; for the existence of planes in crystals in which cleavage takes place with special readiness, &c.—all these are instances which apparently afford evidence of atomic dissymmetry. May not valency after all depend—not in the number of "charges" carried by the atom, but—on the number of directions in which the ever-present "lines of force" are free to act?

WORK OF THE KEW OBSERVATORY IN 1887.

THE Annual Report of the Kew Committee, just issued, shows that the activity of the staff of the Kew Observatory is still well sustained, and the various departments devoted to observations—magnetic, meteorological, and solar—verification of scientific apparatus of various kinds, rating of time-pieces, and experiment, all show a considerable turn out of work. In addition to the regular periodical magnetical observations, the main results of which are given in a concise form in the appendixes, assistance was rendered to Profs. Rücker and Thorpe in respect to their valuable magnetic survey of Great Britain, which we are glad to learn they have now completed, after having devoted the greater portion of their vacations to the task for the last four years. The labours of the Krakatão Committee of the Royal Society, the Magnetic Committee of the British Association, the late Prof. Balfour Stewart, and other investigators, have also been supplemented by aid afforded by the Kew staff.

The meteorological staff have during the year recorded, principally on behalf of the Meteorological Council, who defray the expenses attendant on the work, some 57,126 observations averaging over 150 per diem; the resulting monthly and annual means are, by permission of the Council, published as appendixes.

The multiplication of Observatories engaged in solar photography at home and abroad having rendered unnecessary the co-operation of Kew in that branch of science, so energetically carried on there by the present Chairman, Mr. De la Rue, and the late Prof. Balfour Stewart, twenty years ago, the photoheliograph has only been employed of late years as an ordinary telescope, by means of which the counting of new sunspot groups is continued after Schwabe's method.

An appendix shows that during the last year 44 new groups were catalogued, and that on 60 days out of 180 days of observations the sun's surface was free from spots.

Under the heading "Experimental Work" we find a good deal of attention has been devoted to the photography of high cirrus clouds simultaneously from two points, with the view of determining their position and motions; and to the question of the proper construction of black bulb thermometers; and also to preparatory operations with the Indian Government pendulum apparatus, preliminary to repeating the observations made at the Observatory by Basevi, Heaviside, and Herschel.

There is a long list of various instruments compared and certified during the year in the verification department, which shows that nearly 14,000 articles belonging to one or the other of twenty-seven different classes have undergone treatment; as instruments newly brought within the influence of the verifier, attention is directed to range-finders for the use of the Army and Navy, telescopes of the Admiralty pattern, and surveying aneroids.

The popularity of the Kew certificates, as to the time-keeping of watches, shows that the demand for a guarantee as to the accuracy of performance of a watch other than the maker's name actually exists, and no less than 510 watches and 27 marine chronometers have been submitted to the rating department since the last report was issued.

An appendix showing the behaviour of the best of the watches during the test is given, and it is found that places in this list are being strongly contested for by watch manufacturers, as the blue ribbons of the trade. In consequence of the growth of the work done at Kew, steps have been taken to obtain the permission of Her Majesty's Chief Commissioner of Works and Public Buildings to enlarge the Observatory, which at present remains almost in the same condition as it stood 130 years ago when originally erected as His Majesty George III.'s private Observatory at Richmond.

THE TOTAL ECLIPSE OF THE MOON, JANUARY 28.

BY the kindness of the Astronomer Royal for Scotland, Mr. Gledhill, of Mr. Crossley's observatory at Bermerside, and Mr. Stothert, all of whom took part in the observation of the eclipse of October 4, 1884, we are enabled to give Prof. Struve's times and position-angles for the stars that will be seen to be occulted by observers stationed at Edinburgh, Halifax, and Bath. A comparison of these tables will enable intending observers in other parts of England to form a sufficiently correct list for their own locality.

The following ten stars, not included in the list given in NATURE for January 19, will be occulted as seen from Edinburgh:—

Star's No.	R.A.	Decl.	Star's No.	R.A.	Decl.
	° ′	° ′		° ′	° ′
103	130 30·76	17 18·71 N.	183	131 13·94	17 8·64 N.
106	33·62	16·68	206	24·79	7·44
117	37·99	14·54	213	29·29	6·11
129	44·71	13·74	217	31·16	5·36
141	53 76	14·84	228	34·65	6·26

Star No. 106 is of mag. 9·3; No. 129, 9·5; No. 206 is of the 10th magnitude; the others are all of the 11th magnitude.

EDINBURGH.

Lat. = 55° 57′ 23″; Long. = 3° 10′ 54″ W.

Disappearances.				*Reappearances.*			
Star's No.	Mag.	Angle.	G.M.T.	Star's No.	Mag.	Angle.	G.M T.
		°	h. m.			°	h. m.
152	11	94	10 24·3	108	9·3	333	10 26·0
150	10	116	26·9	87	11	259	26·0
142	10	135	27·2	103	11	226	28·5
148	10	57	27·8	106	9·3	216	29·8
129	9·5	173	28·0	91	11	290	30·3

EDINBURGH—(*continued*).

Disappearances.

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
153	10	114	10	29·6
Beginning of total phase				
156	11	64	10	33·4
141	11	152		35·0
164	8·0	97		37·3
165	9·4	101		37·9
166	9·5	74		39·6
157	9·4	45		40·5
155	11	141		42·3
172	11	128		50·1
180	9·5	70		52·9
181	10	43	11	7·8
198	9·5	87		15·2
197	9	114		17·6
190	11	143		19·7
207	11	85		23·1
209	10	97		24·9
194	11	32		27·5
210	9·5	70		27·5
183	11	171		30·1
212	11	114		33·9
201	8·7	31		36·4
216	10	111		38·2
223	11	80		40·3
225	10	94		42·2
224	11	53		48·3
226	10	124		48·5
206	10	163		49·2
219	10	147		55·3
236	9·5	94		55·5
221	10	30		57·6
213	11	166	12	0·9
233	11	140		4·2
237	11	54		4·4
242	11	105		5·0
217	11	168		6·6
228	11	157		7 2
End of total phase				
247	9·2	75	12	16·1

Reappearances.

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
117	11	200	10	30·3
98	11	300		30·9
Beginning of total phase				
100	9·5	302	10	31·3
93	11	292		31·4
102	11	250		35·1
114	11	235		46·4
129	9·5	206		48·1
110	11	277		49·2
125	11	235		53·5
134	11	334		55·0
126	9·5	282	11	1·0
128	9·5	295		2·2
141	11	227		19·0
138	11	272		20·4
148	10	323		20·4
157	9·4	335		22·1
144	11	307		22·7
142	10	245		26·3
156	11	318		31·3
152	11	287		35·9
150	10	265		36·8
155	11	240		37·2
166	9·5	306		44·7
181	10	340		45·2
164	8·0	284		49·5
165	9·4	280		50·4
194	11	351		53·3
172	11	254		54·7
180	9·5	311		
183	11	213	11	56·2
201	8·7	352	12	0·9
End of total phase				
190	11	240	12	14·3

BERMERSIDE, HALIFAX.

Lat. = 53° 42′ 10″; Long. = 1° 5′ 58″ W.

136	9·5	29	10	23·9
152	11	101		24·3
148	10	67		24·4
150	10	123		28·9
142	10	144		30·9
156	11	72		31·0
Beginning of total phase				
153	10	122	10	31·6
157	9·4	57		36·0
164	8	105		37·8
166	9·5	82		38·3
165	9·4	108		38·8
141	11	167		42·7
155	11	152		47·4
180	9·5	79		51·7
172	11	137		53·5
181	10	53	11	3·3
198	9·5	93		15·6
197	10	121		20·4
194	11	47		21·0
207	11	91		23·6
190	11	153		25·7
209	10	104		26·4
210	9·5	78		26·9
201	8·7	47		30·0
212	11	121		36·8
223	11	88		40·8
216	10	118		41·0
225	10	100		43·8
224	11	62		46·2
221	10	46		51·3
226	10	137		53·5
236	9·5	99		57·5
206	10	181	12	2·2

103	11	212	10	20·8
87	11	252		23·9
112	11	342		25·1
115	11	342		27·1
108	9·3	323		30·9
91	11	283		30·9
Beginning of total phase				
93	11	185	10	32·1
98	11	293		32·4
102	11	242		32·4
100	9·5	295		32·9
114	11	225		41·8
130	11	339		45·7
136	9·5	352		47·4
125	11	225		49·0
110	11	271		49·3
134	11	325	11	0·7
126	9·5	276		2·1
128	9·5	289		4·0
141	11	215		13·1
138	11	267		21·0
142	10	238		24·3
148	10	315		25·2
144	11	301		25·9
157	9·4	326		28·3
155	11	232		34·5
156	11	310		35·5
150	10	258		37·1
152	11	281		37·9
153	10	261		40·5
166	9·5	301		48·3
164	8	278		51·6
165	9·4	274		52·1
181	10	331		52·5

BERMERSIDE—(*continued*).

Disappearances.

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
219	10	157	12	2·4
237	11	63		3·0
242	11	110		7·8
End of total phase				
233	11	147	12	9·9
213	11	187		16·1

Reappearances.

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
172	11	247	11	54·4
180	9·5	305	12	0·0
194	11	338		3·0
End of total phase				
201	8·7	339	12	11·1
190	11	232		12·9

BATH.

Lat. = 51° 23′ 19″; Long. = 2° 22′ 51″ W.

152	11	108	10	22·6
156	11	80		27·0
150	10	131		29·3
157	9·4	66		30·3
Beginning of total phase				
153	10	129	10	31·7
142	10	156		34·3
166	9·5	90		35·3
164	8·0	112		36·6
165	9·4	116		37·9
180	9·5	87		48·5
155	11	165		52·9
172	11	146		55·9
181	10	64		57 5
198	9·5	102	11	13·9
194	11	58		14·5
197	10	128		21·2
207	11	98		21·9
201	8·7	58		23·4
210	9·5	86		24·3
209	10	111		25·9
190	11	167		32·0
212	11	128		38·0
223	11	95		39·2
216	10	126		42·0
224	11	71		42·2
225	10	108		43·5
221	10	57		45·1
226	10	139		55·2
236	9·5	107		57·4
237	11	72		59·5
242	11	117	12	8·6
End of total phase				
219	10	172	12	10·7
233	11	158		15·2
247	9·2	88		16·0

97	11	316	10	20·5
102	11	233		26·6
124	11	349		27·0
116	11	338		27·3
91	11	276		29·1
112	11	329		29·7
93	11	278		30·4
114	11	208		30·9
Beginning of total phase				
98	11	286	10	31·3
115	11	329		31·7
100	9·5	288		32·2
108	9·3	314		32·6
125	11	208		38·3
110	11	264		46·6
130	11	328		50·2
136	9·5	336		55·2
126	9·5	268	11	0·1
128	9·5	282		3·2
134	11	316		3·3
142	10	226		17·8
138	11	259		18·3
144	11	294		26·3
155	11	218		26·5
148	10	307		26·9
157	9·4	316		31·4
150	10	251		34·0
152	11	274		36·9
156	11	302		36·9
153	10	253		37·8
166	9·5	292		49·1
172	11	238		50·2
164	8·0	271		50·6
165	9·4	267		50·9
181	10	321		56·8
180	9·5	297	12	1·6
190	11	219		5·8
194	11	327		8·6
End of total phase				
201	8·7	328	12	17·1

The Chief Assistant, Royal Observatory, Greenwich, Mr. H. H. Turner, will be obliged if successful observers will write or telegraph to him, *immediately* after the eclipse is over, the number of immersions and emersions observed, and the character of the night for observing.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Prof. Sylvester is not to lecture this term; Mr. Es on will give for him courses on Higher Plane Curves and on Involution.

Prof. Pritchard promises two lectures on Modern Methods of determining Stellar Parallax, besides a longer course on Lunar and Planetary Theories.

In Physics, Prof. Clifton is giving only an elementary lecture; Mr. Walker lectures on the Polarization of Light, treated mathematically, and Mr. Baynes is to take up Fourier's Theorem and its Application to the Conduction of Heat.

In Chemistry, besides the usual systematic courses, Prof. Odling is lecturing on the Paraffins, and Mr. Veley on Physical Chemistry.

In the absence of Prof. Moseley, Dr. Hickson is lecturing on the Morphology of the Chordata; Mr. Hatchett Jackson lectures on Parthenogenesis.

Prof. Burdon Sanderson is treating of the Nervous System, and Prof. Bayley Balfour of the Algæ.

Dr. Tylor is giving Anthropological Elucidations of Greek and Latin authors, and the Reader in Geography is continuing the courses which he began last term.

The Professorship of Geology is to be filled up in the course of this term: applications will be received by the Registrar up to February 1.

The Radcliffe Fellowship will be awarded this term; also the Burdett-Coutts Scholarship, as soon as the Professor of Geology is appointed.

The number of men reading Medicine is steadily increasing, and now that it is possible for a man to pass his B.A. examinations and his first M.B. examination in four years, there can be little doubt that the increase will continue.

CAMBRIDGE.—Mr. H. D. Rolleston, B.A., M.B., of St. John's College, has been appointed Demonstrator of Pathology.

Sir F. A. Abel, F.R.S., has been appointed to deliver the Rede Lecture this year.

The Disney Professor (the Rev. G. F. Browne) will lecture on Tuesdays this term on Sculptured Stones of pre-Norman type in the British Islands. The inaugural lecture will be given in the Senate House on January 31.

Dr. F. Warner's lectures on the Growth and Development of the Intellectual Faculty began on January 25. The lectures aim at describing and analyzing the action of the brain of a child, with special application to educational methods.

SCIENTIFIC SERIALS.

THE most important article in the numbers of the *Journal of Botany* for December 1887 and January 1888 is one by Mr. S. Le M. Moore, "On Epidermal Chlorophyll," in which he shows that the presence of chlorophyll-corpuscles in the cells of the epidermis is of much more common occurrence than is usually stated in text-books, and that these chlorophyll-corpuscles very commonly contain starch-grains. The other articles relate to botanical nomenclature and to the distribution of British plants. In addition, Mr. J. G. Baker continues his "Monograph of the Tillandsieæ," and Colonel Beddome contributes a paper on Ferns of Perak and Penang.

IN the *Botanical Gazette* (Crawfordsville, Indiana) for December 1887 is an interesting paper by Mr. Byron D. Halsted, showing that under certain conditions pollen-grains may contain three nuclei, instead of the two usually found in them.

Bulletin de l'Académie Royale de Belgique, November 1887.—Action of the acids on the taste, by J. Corin. The object of these researches has been to ascertain what relation there may exist between the acid taste and chemical composition. The author arrives at the curious result that acidity increases with the quantity of basic hydrogen contained in the acid molecule, and decreases with the weight of the molecule itself.—Physical observations on Saturn, by Paul Stroobant. These observations, extending over the period from January 27 to April 20, 1887, show that the famous divisions of Encke and Struve appear to be subject to great modifications, especially as regards the actual position occupied by them. No doubt the state of the terrestrial atmosphere, the proximity of the moon, and other outward circumstances, must exercise a considerable influence on the character of the manifestations. But the changes here recorded, such as the disappearance of Encke's division while that of Struve is still visible, make it evident that other and more recondite causes are at work in producing these shifting appearances.—Experimental researches on the sense of vision in the Arthropods (second part), by Felix Plateau. In this section the author passes from a study of the Myriapods to that of the higher order of the Arachnidæ, and arrives at the general conclusion that in all the sub-groups of the Spiders, Scorpions, and Phalangidæ the visual sense is very feebly developed. They exhibit in general a vague perception of movement acting on their nervous system, rather than a clear sight of any definite object. In the case of *Epiblemum scenicum*, distinct vision does not seem to extend beyond a distance of 1 centimetre, while *Tegenaria domestica* and others seem unable to distinguish form at all. Even in the closest proximity they rush with equal avidity on true or false objects of prey. The scorpions also show little evidence of sight, shunning the light and awaiting, rather than pursuing, their prey, which they fail to detect except at very short distances. The same remark applies to the Phalangidæ, which compensate the defect of vision by the exquisite tactile sense of their extremities.

Rivista Scientifico-Industriale, November 1887.—On the heating of metallic points when discharging their electricity, by Prof. Eugenio Semmola. Some experiments are described scientifically demonstrating the fact that heat is generated while metallic points discharge their electricity, the points themselves becoming at the same time heated. It is suggested that this fact, now for the first time verified, might under certain conditions be utilized as a new means of studying atmospheric electricity.—On the anæsthesis and poisoning of plants, by Dr. Flaminio Tassi. An analysis is given of the researches and experiments carried out by Prof. T. Caruel, tending to show that certain plants really possess a property analogous to the irritability, excitability, sensitiveness, or contractibility of animals, as it is variously called; that this property is not derived from any particular nervous system, but from the vegetable protoplasm itself; that certain organic substances are alike fatal to plants and animals; and that a state resembling animal anæsthesis is also produced especially in those plants which are endowed with excitable organs, and in many flowers that open and close at fixed times.

THE last two parts of vol. xviii. of the *Izvestia* of the East Siberian branch of the Russian Geographical Society contain a variety of valuable information. In a paper on the lower course of the Upper Angara, Dr. Kiriloff brings together some interesting facts about the fishing in Lake Baikal, which, notwithstanding complaints about the disappearance of the *Salmo omul*, still yields every year about 30,000 cwt. of fish. MM. Priklonsky and Slyeptsoff contribute notes on the religious beliefs of the Yakutes, who, although christened, have retained in full their Shamanist religion and practices. M. Karpinsky gives some notes on the gold-diggings of the Olekma system. Especially valuable papers are contributed by M. Savenkoff, on his archæological researches on the Yenisei, and by M. Eleneff on the caves on the banks of the Biryusa River. It would be impossible to enumerate in a short note all the interesting data mentioned in M. Savenkoff's preliminary report. His numerous collections contain, among other things, big bones of the mammoth and the rhinoceros, which bear unmistakable traces of having been broken by man for the sake of the marrow, and thus belong to the very rare relics of the Palæolithic period in Siberia. His collections also include bones with grooves for the insertion of a stone arrow-head, and many interesting implements, showing that stone implements were largely used during the Bronze Age, and partly during the Iron Age. The full report of M. Savenkoff, which will contain accurate drawings of the Yenisei inscriptions, will be most valuable. As to the exploration of caverns on the banks of the Biryusa and the Yenisei, M. Eleneff gives only a short description of his diggings, with detailed drawings and lists of the implements and various things found: Chinese money from the thirteenth or fourteenth century in the upper layers, various iron implements in the middle layers, and Neolithic stone implements in the lowest layers. The same parts of the *Izvestia* contain preliminary reports about an excursion to Lake Kosogol and the Munku-Sardyk, by MM. Prein and Yaczewski, during which excursion the glacier of this peak was thoroughly mapped and photographed, and large collections of Alpine flora were gathered.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 22, 1887.—"Heat Dilatation of Metals from Low Temperatures." By Thos. Andrews, F.R.S.E.

The experiments of this paper were made to approximately determine the coefficients of heat dilatation of modern steels from low temperatures. The metals employed were wrought iron, "soft" Bessemer steel, "hard" Bessemer steel, "soft" Siemens-Martin steel, "hard" Siemens-Martin steel, "soft" cast steel, "hard" cast steel, &c., of known composition, specific gravity, &c., given in detail in the paper. The terms "soft" and "hard" relate only to difference of percentage of combined carbon. The ranges of tempera-

ture chosen for the observations were from $-45°$ C. to 300° C. The experiments were made on rolled bars of the various steels and also on large hammered forgings 5 inches diameter. Details are given in the paper of the general method of experimentation, and also of the methods adopted for reducing the metals to the very low temperature employed. The results of an extensive series of experiments are recorded in tabular form in the paper. The coefficients of dilatation were found generally to decrease with the reduced temperature. The author also found such to be the case in his recent observations on the heat dilatation of pure ice from low temperatures. There seemed to be a slightly greater dilatation in the direction of the length of the forged metallic cylinders than when measured across the diameter. It was also noticed that the coefficients of dilatation were greater in the case of steels having a lower percentage of combined carbon than in those containing a higher percentage.

January 12.—"Invariants, Covariants, and Quotient Derivatives associated with Linear Differential Equations." By A. R. Forsyth, F.R.S.

The memoir deals with the covariantive forms associated with the general ordinary linear differential equation. The most general transformation to which such an equation can be subjected without changing its character is one whereby the dependent variable y is changed to u by a relation $y = uf(x)$, and at the same time the independent variable is changed, say, from x to z. When these transformations are effected there are n relations between the coefficients P and Q of the equation in its two forms, and it is shown that from these others can be deduced which are of the form

$$\psi(\mathrm{P}) = \left(\frac{dz}{dx}\right)^{\rho} \psi(\mathrm{Q}).$$

Such a function ψ is called an invariant of index ρ.

Irreducible invariants are proved to be divisible into two classes, fundamental and derived. Each of the former, which are $n-2$ in number, consists of two parts; one of these is linear in the quantities P and their derivatives, the other is not linear, but has in every term as a factor either P_2 or some derivative of P_2. It is shown that the differential equation can be reduced to a canonical form without any term in $\frac{d^{n-1}u}{dz^{n-1}}$ or $\frac{d^{n-2}u}{dz^{n-2}}$; and hence each of the prior class of invariants is linear in the coefficients of the canonical form and their derivatives. These fundamental invariants are called priminvariants. The derived invariants are obtained from the priminvariants by two processes, which are called the quadriderivative and the Jacobian; they are most conveniently arranged in classes according to their degrees in the coefficients of the equation. The number of quadrinvariants is $2n-5$; the number of invariants of every degree higher than the second is $n-2$.

The relation between the independent variables of a semi-canonical form and of the canonical form shows that the dependent variable may be considered as a covariant. It is proved that there are other $n-2$ associate dependent variables, each satisfying a linear equation and possessing for the canonical form the invariantive property.

From this aggregate of dependent variables, a set of irreducible identical covariants is derived by the two processes formerly used for the invariants; when the equation is taken in its canonical form, all these covariants up to a certain order involve the dependent variables alone. There is also a set of irreducible mixed covariants which are the Jacobians of each of the dependent variables in turn, and one of the invariants.

Illustrations of the results are given for the equations of the second, the third, and the fourth orders; and in this connection, functions, called quotient derivatives, are obtained. Some of their properties are given, one of the most important being that they are covariantive for homographic transformation of both the dependent and the independent variables.

Finally, the characteristic differential equations satisfied by all concomitants are obtained; and among other inferences it is proved that the aggregate of concomitants constituted by the invariants and covariants obtained in the earlier part of the memoir is complete, *i.e.* that any concomitant can be algebraically expressed in terms of the members of that aggregate.

"Preliminary Note on the Nephridia of Perichæta." By Frank E. Beddard, M.A.

The following observations are the result of a study of a species of Perichæta, which is probably identical with Perrier's *P. aspergillum.* I owe a number of excellently preserved examples to the kindness of Mr. Shipley, Fellow of Christ's College, Cambridge.

In transverse sections of the anterior segments the nephridia are seen to form numerous tufts of glandular tubules closely related to the body-wall and to the septa. This appearance, which is also seen in dissections, is very different from that of most earthworms, and has been commented upon by other observers.

The remarkable appearance of the nephridia led me to infer that I should find the external apertures in each segment to be numerous, as I showed to be the case in Acanthodrilus. I am now able to state that this is also the case in Perichæta (in all probability in other species besides *P. aspergillum*). The external pores lie between the setæ, but have no regularity in their arrangement; frequently there were three or four between two successive setæ, as often there seemed to be only one or two. The minute structure of the terminal section of nephridia is slightly different from that of Acanthodrilus. Another point, to which I wish to direct attention in this communication, is that *in Perichæta there is a connection between the nephridia of successive segments.*

Quite recently, Ed. Meyer and Cunningham have shown that in *Lanice conchilega* the nephridia of each side are connected by a continuous longitudinal duct. This discovery is in accord with the presumed origin of the Annelid from the Platyhelminth excretory system, and also with the development of Polygordius (Hatchek) and Lumbricus. In Perichæta the connection between the nephridial tufts of successive segments is not brought about by a continuous longitudinal duct, one on each side of the body, but by numerous tubules which perforate the intersegmental septa. Thus it appears that *the nephridial system of Perichæta consists of a network of tubules.* In this respect Perichæta agrees with the leech Pontobdella, but differs in the presence of numerous nephridiopores in each segment. These facts appear to lend further support to the view that it is possible to derive the Annelid from the Platyhelminth excretory system.

Lang has pointed out that the "secondary" pores by which the excretory organ of the Platyhelminths communicates with the exterior have probably given rise to the nephridial pores in the Annelida; by a subsequent arrangement of these in a metameric fashion, and by the breaking up of the nephridial network, the paired nephridia have originated. The longitudinal canal has disappeared, except in the cases that I have already mentioned. In some Platyhelminths the longitudinal canals are, partly at least, broken up into a network; and it is this condition which has persisted in Perichæta and Pontobdella; moreover, in some Platyhelminths, where the "secondary" pores have become metamerically arranged, there are more than one pair to each "segment." For this reason it is perhaps allowable to regard the condition of the nephridia in Perichæta as more archaic than Pontobdella. The disappearance of the connection between the nephridia of successive segments leads to the condition which exists in Acanthodrilus; the reduction of the external pores, already perceptible in the posterior segments of *A. multiporus,* culminates in the disappearance of all but two in each segment. The irregularity in the position of these, which is best marked in Plutellus, is the last trace of the presence of multiple nephridiopores in each segment.

Royal Meteorological Society, January 18.—Mr. W. Ellis, President, in the chair.—The paper read was on the non-instrumental meteorology of England, Wales, and Ireland, by Mr. G. M. Whipple. This is a discussion of the observations of wind, cloud, thunderstorms, hail, snow, &c., made at the stations of the Royal Meteorological Society during the eight years 1878–85, and published in the *Meteorological Record.* The S.W. wind is the most prevalent, and blows on the average seventy-four days in the year; the W. wind occurs almost as frequently, blowing sixty-five days. The least dominant winds are the S.E. and N., which occur on twenty-seven days, and the N.E. on thirty-two days. Thunderstorms are most frequent in the eastern and midland counties, and least frequent in the north of Wales.—After the reading of this paper, the annual general meeting was held. The report of the Council showed the Society to be in a satisfactory condition, the number of Fel'

being 522.—Mr. Ellis in his Presidential address reviewed briefly the work and position of the Society, remarking that such a Society, whilst unable to carry out expensive original or experimental work, could yet act with great advantage in inciting volunteer workers throughout the country to united action, of which one recent example was the ready response to the request of the Society for photographs of lightning, an excellent collection of which had been obtained, and which would shortly be exhibited; in addition to which arrangements were being made for the more systematic observation of thunderstorms. Referring to the question of sympathetic relation between sunspots and magnetism and meteorology, he thought that any complete treatment of the question in its meteorological aspect seemed to require that it should be dealt with in a much more comprehensive manner than before, for which purpose observations more completely covering the surface of the globe might be necessary, if indeed not necessary also for the solution of many other meteorological questions, the present meteorological stations being distributed over the earth in such isolated clusters. The attention given to synoptic charts was most important, but the general meteorological characteristics of places should also still continue to be studied. After remarking upon other matters, he laid before the meeting tables showing the monthly means of amount of cloud from observations made in three different series at the Royal Observatory, Greenwich, extending in all from 1818 to the present time. In concluding, Mr. Ellis said that at one time the science of meteorology seemed likely to form an exception to the general rule of advance, for more than any other it has required the united action of many workers, but the field of inquiry of late years opened out allows us already to talk of the new or modern meteorology, phrases typical of the advance achieved, although the knowledge gained seems only to remind us of how much has yet to be done.—The following gentlemen were elected the officers and Council for the ensuing year:—President: Dr. Wm. Marcet, F.R.S. Vice-Presidents: Francis Campbell Bayard, William Ellis, Charles Harding, Richard Inwards. Treasurer: Henry Perigal. Trustees: Hon. Francis Albert Rollo Russell, Stephen William Silver. Secretaries: George James Symons, F.R.S., Dr. John William Tripe. Foreign Secretary: Robert Henry Scott, F.R.S. Council: Hon. Ralph Abercromby, Robert Andrew Allison, M.P., Edmund Douglas Archibald, William Morris Beaufort, Henry Francis Blanford, F.R.S., Arthur Brewin, George Chatterton, William Henry Dines, Henry Storks Eaton, Baldwin Latham, Edward Mawley, Dr. Charles Theodore Williams.

Chemical Society, December 15, 1887.—Mr. William Crookes, President, in the chair.—The following papers were read:—An apparatus for comparison of colour-tints, by Alfred W. Stokes.—The alloys of copper and antimony and of copper and tin, by E. J. Ball.—The constitution of the so-called mixed azo-compounds, by Francis R. Japp, F.R.S., and Felix Klingemann.—The interpretation of absorption-spectra, by G. H. Bailey.—The reduction of potassium bichromate by oxalic acid, by C. H. Bothamley.—The reduction of chlorates by the zinc-copper couple, by C. H. Bothamley and G. R. Thompson.—Preliminary notice on the oxidation of oxalic acid by potassium dichromate, by Emil A. Werner.—Isomeric change in the naphthalene series; No. 1, by Henry E. Armstrong.—Isomeric change in the naphthalene series; No. 2, β-Ethoxynaphthalenesulphonic acids, by E. G. Amphlett and Henry E. Armstrong.—Isomeric change in the naphthalene series; No. 3, β-Chloronaphthalenesulphonic acids, by Henry E. Armstrong and W. P. Wynne.—Isomeric change in the naphthalene series; No. 4, α-Haloidnaphthalenesulphonic acids, by Henry E. Armstrong and S. Williamson.—The sulphonation of naphthalene, by Henry E. Armstrong and W. P. Wynne.

Entomological Society, January 18.—Fifty-fifth aniversary meeting.—Dr. D. Sharp, President, in the chair.—An abstract of the treasurer's accounts was read by Mr. H. T. Stainton, F.R.S., one of the auditors; and Mr. H. Goss, the Secretary, read the Report of the Council.—It was announced that the following gentlemen had been elected as Officers and Council for 1888:—President: Dr. David Sharp. Treasurer: Mr. Edward Saunders. Secretaries: Mr. Herbert Goss and the Rev. Canon Fowler. Librarian: Mr. F. Grut. As other Members of Council: Mr. Henry J. Elwes; Sir John Lubbock, Bart., M.P., F.R.S.; Mr. Robert McLachlan, F.R.S.; Dr. P. Brooke-Mason; Mr. Edward B. Poulton; Mr. Osbert Salvin, F.R.S.; Henry T. Stainton, F.R.S.; and Lord Walsingham, F.R.S.—The President delivered an address, and a vote of thanks to him was moved by Mr. McLachlan, seconded by Mr. F. Pascoe, and carried.—A vote of thanks to the Treasurer, Secretaries, and Librarian, was moved by Mr. Kirby, seconded by Mr. Waterhouse, and carried. Mr. E. Saunders, Mr. H. Goss, Canon Fowler, and Mr. F. Grut replied.

Mathematical Society, January 12.—Sir J. Cockle, F.R.S., President, in the chair.—Messrs. J. M. Dodds and G. G. Morrice were elected members, and Mr. E. W. Hobson admitted into the Society.—The following communications were made:—The theory of distributions, Capt. P. A. Macmahon, R.A.—On the analogues of the nine-points circle in space of three dimensions, S. Roberts, F.R.S.—On a theorem analogous to Gauss's in continued fractions with applications to elliptic functions, L. J. Rogers.—A theorem connecting the divisors of a certain series of numbers, Dr. Glaisher, F.R.S.—On reciprocal theorems in dynamics, Prof. H. Lamb, F.R.S.

Mineralogical Society, January 10.—Mr. L. Fletcher, President, in the chair.—The following papers were read:—On the development of lamellar structure in quartz crystals by mechanical means, by Prof. J. W. Judd, F.R.S.—On the polysynthetic structure of some porphyritic quartz crystals in a quartz-felsite, by Colonel C. A. McMahon.—Notes on hornblende as a rock-forming mineral, by Mr. A. Harker.—On the invitation of the President, Mr. Allan Dick, who was present as a visitor, made some remarks on the process of kaolinization, illustrated by models of crystals.

Paris.

Academy of Sciences, January 16.—M. Janssen, President, in the chair.—Remarks on M. Wolf's last note on the subject of synchronization, by M. A. Cornu. The author is glad to find himself in harmony with M. Wolf on the important points that no synchronizing system is possible without some controlling or regulating apparatus, and that such apparatus forms an essential feature of the systems of Jones and Vérité.—Remarks accompanying the presentation of the third volume of the "Annales de l'Observatoire de Rio Janeiro," by M. H. Faye. This volume, which was presented by the Emperor of Brazil, is entirely devoted to the three Brazilian expeditions sent to the Island of Saint Thomas, Pernambuco, and Punta Arenas (Patagonia) to observe the transit of Venus in the year 1882. From a comparative study of the recorded results, M. Cruls has calculated the solar parallax at 8″·808.—Fresh researches on the phenomena produced by a potent toxic agent, which is constantly emitted with the air exhaled from the lungs of man and other mammals, by MM. Brown-Séquard and d'Arsonval. The experiments here described and made on seven rabbits entirely confirm the conclusions already announced regarding the powerful character of this volatile organic poison, which appears to be almost certainly an alkaloid. Further researches have been undertaken in order to determine this point by direct proof.—On spontaneous tetanus, by M. Verneuil. A case reported by Dr. Buisson, of Auberchicourt, is referred to as confirming in a striking way the author's opinion that there is no such thing as spontaneous tetanus, and that all reported cases will be found, if carefully studied, to be caused by some virus introduced in some way into the system.—On the canalization of the Isthmus of Panama, by M. de Lesseps. In supplement to his recent remarks on this scheme the author announced that the proposal to establish provisionally a lock canal for one at a dead level has just been adopted by the Company. He further explained how the extensive works already executed can be adapted to the new design, so that the Canal might still be completed and opened for traffic by the year 1890. It would moreover be so constructed that the original plan of a level canal might be gradually carried out without any interruption to the navigation. Both would be of the same length of 74 kilometres, with a breadth of 22 metres at the bottom, and 44 on the surface. Four locks will be needed, each 18 metres wide at the entrance, and with a total length of 180 metres.—On the barometric curves recorded during the third scientific expedition of the *Hirondelle*, by Prince Albert of Monaco. These barometric readings seem to show that the motions of the ship are insufficient to explain the oscillations recorded during the course of a storm, and that these oscillations accompany certain meteorological disturbances without at all aiding to forecast the weather.—On the measurement of the absolute intensity

of weight, by M. G. Defforges. The apparatus constructed by MM. Brunner Brothers on the principles here laid down has already been applied with satisfactory results to the measurement of absolute gravity at Paris, Lyons, Dunkirk, Algiers, Laghwat, and Nice.—On elliptical polarization by transmission through metals, by M. Georges Meslin. The author here studies the modifications which polarized light undergoes in its passage through metal plates thin enough to be transparent. As in metallic reflection, the two polarized vibrations in the plane of incidence and in the perpendicular plane undergo in relation to each other a certain retardation, while the rectilinear polarization becomes elliptical.—On the application of the phenomenon of transversal magnetization to the study of the coefficient of magnetization of iron, by M. Paul Janet. This question is here studied by means of a method of mutual induction which presents several advantages over other processes, and which may be easily applied to the study of the influence of the medium in the phenomena of induction.—On the decreasing solubility of the sulphates, by M. A. Etard. The author has already shown that between 103° and 190° C. the sulphate of copper becomes less soluble according as the temperature increases. He now finds that most of these salts undergo a certain disturbance at some point of the line of complete solubility, beyond which point the solubility increases less rapidly and even remains almost stationary. Details are given for the sulphates of zinc, manganese, and potassium.—Symmetric disposition of the centres of the four chief continents, by M. Alexis de Tillo. By graphic processes the author finds that the co-ordinates of the orographic centres of the continents are as under: Asia (with Europe) 43° N., 85° E. of Greenwich; Africa, 4° N., 27° E.; North America, 45° N., 102° W.; South America, 14° S., 56° W. The geometric centre of the Old and New World lies in the region of the Azores and Canaries, and the meridian of Delisle (20° W. of Paris) may in some respects be regarded as the *natural* meridian of the globe.

BERLIN.

Physical Society, December 23, 1887.—Prof. du Bois Reymond, President, in the chair.—Prof. Schwalbe gave a detailed account of the research which Dr. Aubel and Prof. Spring have carried out on the rapidity of the interaction between acids and zinc which is mixed with lead.—Prof. Vogel made a statement of his observations of the solar eclipse of August 19. As is well known, the observations during the whole of the lengthy period of totality were unproductive of results at all stations except those in Siberia—which were not much utilized by observers—owing to unfavourable weather. The speaker appears to have been among the most fortunate at Jurjewetz, where he was stationed in company with the Belgian astronomer Niesten, and the Russian astronomers Kortazzi and Belopolski, for at this place the sun was momentarily visible through the clouds. As a matter of fact, several photographs were successfully obtained, on which, as shown by a specimen exhibited, a corona and several protuberances were visible. These photographs, however, scarcely suffice as a basis for any scientific research. Prof. Vogel had also received a photograph of the eclipse taken in clear weather by an amateur in the Ural Mountains; it showed a complete but small corona, and near it is the image of a star, probably Mercury. Unfortunately no details are given about this photograph. The photographer Karelin has secured some very interesting results at Jurjewetz. This observer, using a very sensitive apparatus, had obtained some very successful photographs of the lunar eclipse, which had taken place about a fortnight before the solar eclipse. The plates were only exposed for 1/60 of a second, and working upon this experience he obtained photographs during the solar eclipse by a similar exposure of 1/60 of a second. The results were quite satisfactory, and from this the important conclusion may be arrived at that exposures of the above very short duration may be used during future solar eclipses. Herr Karelin has further taken a photographic landscape during the eclipse, and from a comparison of the time necessary to obtain this with the time required by the speaker's son to obtain a similar picture during full moon, the speaker concluded that the brightness during the solar eclipse was fifty-six times as great as that of the full moon. Prof. Vogel had intended to photograph the spectrum of the corona, but was not successful in his attempt. He further exhibited a photograph of the spectrum of pure oxygen contained in a Geissler tube and made luminous by the sparks from a battery. The photograph was then photographically enlarged, so that it could readily be seen by a large audience at the same time, and in this form it showed the red and green line, together with a long series of bands and lines extending far into the ultra-violet region. Many of the lines described by Dr. Schuster as single could be seen to be double in this photograph. One of the chief things shown by the enlarged photograph is that the oxygen-spectrum of the positive pole, and of the negative pole, as well as the spark-spectrum of the oxygen itself are here combined into a single spectrum. The speaker intends to apply this method of magnifying the photographs to the spectra of other gases, and thus make the enlarged spectra accessible for teaching purposes in the form of diagrams.

January 6.—Prof. du Bois Reymond, President, in the chair.—Prof. Oettingen, of Dorpat, spoke on the explosion of a mixture of hydrogen and oxygen obtained by electrolysis. As is well known, Bunsen has advanced the following view, based on his experiments, on the explosion of electrolytic gas: by the explosive union of the oxygen and hydrogen, when the spark is passed, a temperature of 3000° C. is produced, the water formed being at once dissociated at this temperature; the temperature of the mixture of gases formed by the dissociation then falls, whereupon a new union between the two takes place, and so on; hence the explosion of electrolytic gas is to be regarded as made up of a series of partial explosions following each other in rapid succession. The speaker had intended several years ago to subject Bunsen's theory to an experimental investigation, and hoped to be able to analyze the phenomenon by the use of a rapidly revolving mirror. As a matter of fact, when the mirror was rotated at a suitable speed, the image observed was not that of a single narrow strip of light, but was rather of considerable width; it was not found possible to interpret this image, notwithstanding that the somewhat complicated experiments were repeated many times. An endeavour was next made, with the assistance of a photographer, to obtain a record of the image, which was equally unsuccessful. He then underwent a course of photographic study; and when he had acquired sufficient experience, he last year repeated his former experiments, with a positive result, using the new methods of sensitizing the plates for the less refractive parts of the spectrum, and the most sensitive possible dry plates. The speaker had further shown, by a spectroscopic examination of the light emittted during the explosion of electrolytic gas, that the light is due, not to the combustion of the gases, but of sodium, which is doubtless accounted for by the incandescence of small particles of glass torn off by the passage of the sparks. He hence introduced, in accordance with the method of Dewar and Liveing, portions of finely powdered salts of various metals, such as copper, zinc, lithium, and cadmium, &c., into the eudiometer in which the explosion of the electrolytic gas was to be made, and now obtained, not only excellent spectra of the respective metals, but also quite distinct photographs of the images in the rotating mirror. A plane mirror was used, placed at fixed distances from the eudiometer and camera, which projected the images of the successive events taking place during the explosion on to the flat sensitized plate. The speaker exhibited a series of the photographs thus obtained: these presented the following appearances, most clearly when the salt used was chloride of copper. In the first place, a bright point, corresponding to the place of passage of the spark, from which a short bright ray passed both upwards and downwards in the tube; then secondly, at a fixed distance from this and occupying the whole length of the eudiometer, a bright image intersected lengthways from end to end by zigzag lines and transversely by parallel sinuous waves. The speaker interpreted the above images by referring the intersecting zigzag lines to a series of waves of impulse caused by successive explosions; he considered on the other hand that the sinuous waves are due to the small particles of the metal which are set in motion by the impulse waves, and hopes to render this explanation still more probable by a new series of experiments on the explosion of carbon-disulphide. According to Prof. Oettingen, the experiments of Berthelot, and Vieille, and of Mallard and Lechatelier, have no bearing upon the explosion which he has studied, occurring as it does in a few thousandths of a second, but refer to the combustion which occurs subsequently to the explosion.—Dr. Kötter spoke on the problem of determining the pressure exerted by the earth, discussed the difficulties in the way of estimating the pressure which the earth exerts upon a wall built into it, and stated the limits within

which some theoretical calculations may be relied upon.—Prof. Schwalbe announced that he is engaged in drawing up a Greek nomenclature in connection with physics, and invited the members of the Society to communicate to him any expressions borrowed from Greek which are either rare or difficult to understand.

Meteorological Society, January 3.—The President, Prof. von. Bezold, opened the meeting with a short speech in memory of the late member of the Society, Prof. Kirchhoff, whose many-sided works had not been without importance to the science of meteorology.—The Secretary then made his report on the activity of the Society during the past year, and on the establishment of new meteorological stations in connection with the circle of such stations surrounding Berlin promoted by the Society.—At the election of officers which then followed Dr. Vettin was chosen as President, and Prof. Von Bezold as Vice-President.—Dr. Hellmann spoke on the meteorology of the Iberian Peninsula. During a prolonged stay in Spain in the years 1875–76, the speaker was unable to study the rainfall of the country owing to insufficient data. Since then, however, some 760 annual statements have been published from 70 stations, so that he was now in a position to work out the rainfall, and he presented the results of this in the form of a chart, which formed the basis of his communication. The local distribution of rainfall is very varying. In the district of the Ebro and the whole of the south-east part of the country as far as Carthagena and Old Castile, the rainfall is very slight, the annual fall being about 270 mm.; on the other hand, on the west coast, and in the district of the Pyrenees, the rainfall is considerable, presenting a fall of some 1600 mm. per annum. The maximum fall is found in Serra da Estrella, where it amounts to 3500 mm. The course of the lines of equal rainfall of 300, 400, 600, 800, 1000, and 1600 mm. per annum is extremely curious, and was carefully discussed by the speaker. Two sections through the peninsula, on which the rainfall was represented by ordinates, showed how steep the gradients are when passing from the west coast towards the interior. The speaker threw a good deal of interesting light on the close connection which exists between the agricultural and social conditions of the inhabitants and the rainfall. It appeared that very profound differences have developed themselves between the districts where the rainfall is great and small, and in the latter where the district is well supplied with water or not, these differences completely governing the character and mode of life of the inhabitants. All the stations in common showed a minimal rainfall in the summer, occurring in the months of July and August. In the most southerly stations this minimum falls to 4 mm. for the above two months, whereas in the north-west it rises to more than 100 mm. The curve of maximal rainfall shows three typical forms and three transitional forms. One set of stations shows a maximum in winter, another set has its maximum in the spring, and the third shows it in the autumn, and between these three a graduated transition is observed. The quotient $\frac{\text{maximum}}{\text{minimum}}$ increases rapidly on going south. The difference in the amount of rainfall per annum could only be calculated for thirty-two stations, since it must be based on the records of ten consecutive years at least. The ratio of the extreme to the mean annual rainfall in the north-west, as well as in Central Europe, was two, while in the interior of the country this ratio rose to five. The rainstorms are rarely continuous; they occur chiefly in the morning, and are followed by sunshine: three days of continuous rain, or even of clouds, scarcely ever occur in the whole of Spain. This statement was confirmed by the records of the autographic sunshine recorder. Snow rarely falls in the Iberian Peninsula; the maximum fall of twenty-two snowy days was observed at a station on the upper Douro. At the southern stations snow falls once in thirty years, and it never falls at all at many stations. It is impossible to give any account here of the large mass of further details which the speaker brought before the meeting; they will shortly be published by him in a very extended form.

STOCKHOLM.

Royal Academy of Sciences, January 11.—An account of a memoir by Prof. Ewart, of Edinburgh, on *rigor mortis* and its relation to the putrefaction of fish, by Prof. Smith.—A report of the work done by the Swedish Ornithological Society, by the same.—On the organs and modes of attachment of the marine Algæ, by Count H. Strömfelt.—Mycological studies in Jemtland, by Dr. E. Henning.—On freshwater Algæ from Spain, by Miss M. Lewin.—Astrophotometric studies, by Dr. Charlier.—On the conductibility of illuminated air, by Dr. S. Arrhenius.—Remarks on the paper of Prof. Hoppe, "Zur magnetelectrischen induction," by Dr. Mebius.—On electric currents caused by mechanical pressure, by M. P. A. Siljeström.—Some derivates of naphthostyrite, by Dr. Ekstrand.—On barysite, a silicate of lead from the mines of Harstig, by Messrs. Sjögren and Lundström. On the recent remarks of M. Lebesconte concerning the Cruziana, by Prof. Nathorst.—Demonstration of some propositions of the theory of the elliptic functions, by Dr. Falk.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Institute of Actuaries' Text-book; Part 2, Life Contingencies; G. King (Layton).—Geography for Schools; Part 1, Practical Geography: A. Hughes (Clarendon Press).—Histoire des Sciences Mathématiques et Physiques, tome xii.: M. Marie (Gauthier-Villars, Paris).—The Elements of Graphical Arithmetic and Graphical Statics: J. Y. Gray and G. Lowson (Collins).—Dr. H. G. Bronn's Klassen und Ordnungen des Thier-Reichs; Erster Band, Protozoa, 35 to 41 Lieferung (Williams and Norgate).—Dynamics and Hydrostatics: R. H. Pinkerton (Blackie).—The Farmers' Friends and Foes: T. Wood (Sonnenschein).—Annuaire de L'Observatoire Royal de Bruxelles, 1888, 55 Année (Bruxelles).—Prodromus of the Zoology of Victoria, Decade xv.: F. McCoy (Trübner).—Pflanzenleben. i. Band: Kerner von Marilann (Leipzig).—Le Climat de la Belgique: A. Lancaster (Bruxelles).—Tableaux Résumés des Observations Météorologiques faites à Bruxelles (Bruxelles).—Untersuchungen über die Schneegrenze im Gebiete des Mittleren Innthales: F. R. Kerner von Marilann (Wien).—Journal of the Chemical Society, January, and Supplementary No. (Gurney and Jackson).—Transactions of the Seismological Society of Japan, vol. xi. (Yokohama).—Journal of the Society of Telegraph-Engineers and Electricians, vol. xvi. No. 68 (Spon).—Botanische Jahrbucher für Systematik, Pflanzengeschichte, und Pflanzengeographie, Neunter Band, iii. Heft: Dr. A. Engler (Williams and Norgate).—Actes de la Société Helvétique des Sciences Naturelles, Locle 85 (Neuchâtel).

CONTENTS.

THURSDAY, FEBRUARY 2, 1888.

THE COMPOSITION OF WATER.

DUMAS, in his well-known memoir on the gravimetric composition of water, which every student is taught, and rightly so, to regard as one of the classics of chemistry, states that of all analyses presented to a chemist that of water is the one which offers the greatest uncertainty. Critics of a certain type may possibly take exception to the literal accuracy of this remark. No one, however, will gainsay the statement that, in view of the momentous issues which depend upon our knowledge of the composition of water, this knowledge is not by any means so exact as the state of contemporary science demands. It is, of course, not merely the question of the quantitative composition of water, but the far more important matter of the relative values of the atomic weights of hydrogen and oxygen. Of all stoïchiometrical constants required by chemists nowadays, those of hydrogen and oxygen are infinitely the most important. Every chemist knows what is dependent on these ratios, and he knows too that the difficulties which their direct determination involves are well-nigh insuperable.

All the discussions within recent years on the validity of Prout's law have tended to show that so far as experimental work is concerned, the question may now be said to hang upon these particular values. It is not too much to say that, if any chemist could succeed in showing by irrefragable experimental evidence that the atomic weight of oxygen was exactly sixteen times that of hydrogen, he would in the present state of scientific opinion at once succeed in inducing his brethren to accept Prout's law and all its far-reaching consequences as articles of their chemical creed.

It may be worth while to examine very briefly the nature of the ground upon which the present accepted values for the relative atomic weights of hydrogen and oxygen are based. It will be generally conceded that the evidence upon which chemists have almost exclusively relied is that afforded by Dumas' gravimetric analysis of water, and by Regnault's determination of the specific gravities of oxygen and hydrogen.

Dumas' work was published in 1843. His method was identical in principle with that employed by Dulong and Berzelius for the same purpose, and consisted, as is well known, in heating copper oxide with an unknown weight of hydrogen, and determining (1) the loss of weight suffered by the oxide, and (2) the weight of the water formed. The decrease in weight of the copper oxide was assumed to represent the weight of the oxygen evolved, and the difference between this weight and that of the water formed was held to be the amount of hydrogen which had combined with that of the oxygen.

In all, nineteen experiments were completed, in which quantities of water varying from about fifteen to eighty-six grammes were formed. Treating the results in the manner adopted by Meyer and Seubert—that is, in accordance with the equation

$$x = \frac{b_1 + b_2 + b_3 + \ . \ . \ . + b_n}{a_1 + a_2 + a_3 + \ . \ . \ . + a_n} = \frac{[b]}{[a]},$$

in which a = weight of oxygen used, and b = weight of water formed—it follows that $[a]$ = 840·16 grammes, and $[b]$ = 945·44 grammes, whence the ratios H : O = 1 : 15·96.

When, however, we come to examine more nearly the details of the method of determination, we find that these ratios are certainly affected by errors of which the magnitude cannot be even approximately known. In the first place, the sulphuric acid solution employed to generate the hydrogen must have contained dissolved air, the effect of which would be to lower the ratio of the oxygen. This fact was not indeed unnoticed by Dumas, but its effect could not be estimated with any certainty. Moreover, it seems almost impossible to prepare hydrogen from zinc and sulphuric acid without the formation of more or less sulphur dioxide, the last traces of which can only be removed by prolonged exposure to potash solution. Copper is one of the few metals that have the power of forming a hydride, and although this hydride, like the palladium hydride, is more or less readily decomposed by heat, the affinity of hydrogen for copper may be still traceable even at moderately high temperatures. Melsens, working in Dumas' laboratory, found that the reduced copper did actually retain hydrogen, and in amount varying with the temperature to which it had been heated. The weight of the condensed water must have been increased, as Berzelius pointed out, by the air which it eventually dissolved. Now the effect of all these errors would be to lower the value for the atomic weight of oxygen. Of course there may have been errors working in the opposite direction of which we know nothing, but it is reasonably certain that the net result of the constant errors, so far as these can be ascertained, is to give too small a value for oxygen. Above all, there are the fortuitous errors, such as those caused by differences in the power of surface-condensation of the vessels employed; errors of weighing and of reduction to a standard atmosphere, &c.; which, although theoretically allowed for and eliminated by a sufficiently frequent repetition of the experiments, may, on the whole, tend to operate in a given direction. Lastly, there is a source of error of the same order in a circumstance which, as there is a certain touch of pathos in them, may be stated in Dumas' own words:—

"Il faut même ajouter que la durée nécessaire de ces opérations, en m'obligeant à prolonger le travail fort avant dans la nuit, en plaçant les pesées vers deux ou trois heures du matin dans la plupart des cas, constitue une cause d'erreur réelle. Je n'oserais pas assurer que de telles pesées méritent autant de confiance que si elles avaient été exécutées dans des circonstances plus favorables et par une observateur moins accablé de la fatigue inévitable après quinze ou vingt heures d'attention soutenue."

There is, above and beyond all, a fundamental flaw in the principle of the method, of which Dumas himself was fully conscious. After having declared that of all analyses presented to a chemist water is the one which offers the greatest uncertainty, he goes on to state to what this uncertainty is due:—

"En effet, 1 partie [d'hydrogène se combine avec 8 parties d'oxygène pour former de l'eau, et rien ne serait

plus exact que l'analyse de l'eau, si l'on pouvait peser l'hydrogène et peser l'eau qui proviendrait de sa combustion. Mais l'expérience n'est pas possible sous cette forme. Nous sommes obligés de peser l'eau formée, et l'oxygène qui a servi à la produire, pour en déduire, par différence, le poids de l'hydrogène qui en fait partie. Ainsi, une erreur de 1/900 sur le poids de l'eau, ou de 1/800 sur le poids de l'oxygène, affecte d'une quantité égale à 1/90 ou à 1/80 le poids de l'hydrogène. Que ces erreurs étant dans le même sens viennent à s'ajouter, et l'on aura des erreurs qui iront à 1/40."

Let us now turn our attention to the evidence afforded by Regnault's determinations of the densities of oxygen and hydrogen. Prof. Le Conte has detected some slight numerical errors in Regnault's reductions (*Phil. Mag.* [4] 27–29), and when the necessary corrections are made it follows that the density of oxygen is 1·105612, and that of hydrogen 0·069269; whence, on the assumption of Avogadro's law, we have the ratio O : H = 15·9611 : 1. This result is in such striking agreement with Dumas' value that it is generally held to afford the strongest corroboration of it. The number given for oxygen is probably among the most accurate of Regnault's determinations of gaseous densities; the subsequent results of Von Jolly, which are alone comparable in character with those of Regnault, when reduced to the geographical position of Regnault's laboratory have not materially altered the value. The number given for hydrogen is certainly not entitled to the same degree of confidence. Indeed, it has been stated that Regnault was himself of this opinion, on account of the great difficulty of procuring hydrogen free from air. It is hardly necessary to point out that even an extremely minute admixture of air would tend to lower the relative value of the atomic weight of oxygen. Moreover, the hydrogen in the course of its preparation must have been saturated with moisture; and although, of course, all precautions at that time known were taken to dry the gas, it is quite certain that it could not have been absolutely free from traces of water. The experiments of Dixon have shown how extremely difficult it is to dry a gas perfectly, and it is now recognized that the ordinary methods of desiccation still leave appreciable traces of moisture in it. The effect of this moisture in the case of hydrogen would be to increase its density, whereas in the case of the oxygen it would tend to decrease it. On the other hand, oxygen and hydrogen when measured under the standard conditions of temperature and pressure are not, strictly speaking, under exactly comparable conditions, and the assumption of the validity of Avogadro's law is not mathematically correct.

Within recent years the question of the composition of water has been again attacked, and with a fuller knowledge of the various sources of error which the progress of science has shown to be present in the older methods. Julius Thomsen found that 1 litre of dry hydrogen, measured under standard conditions of temperature and pressure, when burnt with oxygen gave, as the mean of eight concordant experiments, 0·8041 grammes of water. Accordingly, 2 litres of hydrogen, on combining with oxygen, would give 1·6082 grammes of water. Assuming the validity of Gay-Lussac's law, and using Regnault's values for the weights of the gases at standard temperature and pressure, the calculated weight becomes—

2 litres hydrogen	= 0·1791	grammes
1 litre oxygen	= 1·4298	,,
water	= 1·6089	,,

The difference is 0·7 milligramme. But the question may be immediately asked, "Is Gay-Lussac's law actually valid?" The work of Regnault and Amagat on the relation of volumes of gases to heat and pressure indicates that, as ordinarily stated, it cannot be absolutely valid. Dr. A. Scott has recently put the question to the test of experiment, and, from a long series of trials in which large volumes of gases were caused to combine, he finds that the most probable ratio is 1·994 : 1 (Proc. Roy. Soc., 1887, 398). Taking Regnault's data as before, we have—

1·994 litres hydrogen	= 0·1876	gramme
1 litre oxygen	= 1·4298	,,
water	= 1·6084	,,

which differs only by 0·2 milligramme from Thomsen's result.

Now, from Regnault's densities of oxygen and hydrogen, as recalculated by Prof. Le Conte, it follows that the weights of equal volumes of the gases are as 1 : 15·9611, which, on the basis of Dr. Scott's ratio for the combining volumes, gives—

$$O = 16·009.$$

Prof. J. P. Cooke and Mr. T. W. Richards, of Harvard College, have recently presented us with a further contribution to the subject (Proc. Amer. Acad. of Arts and Sciences, xxiii. 149), which merits very special attention, not only on account of the intrinsic excellence of the experimental work of which it is an account, but also because it is here attempted to obviate certain of the sources of error which have already been pointed out as inherent in Dumas' method. The method adopted by the American chemists was to pass a known weight of hydrogen over heated copper oxide and to weigh the amount of water formed. It will be seen that the essential feature in this method is that the weight of the hydrogen is known whilst that of the oxygen is obtained by difference; in contradistinction to the method of Dumas, where the weight of the oxygen was known and that of the hydrogen found by difference. The preparation of this hydrogen and the determination of its weight were, however, problems which required the highest manipulative skill. Obviously, everything depends upon the purity of the hydrogen. A glass globe of about 5 litres capacity and weighing about 570 grammes was so provided with stopcocks that it could be evacutated by the air-pump. The vacuous globe was weighed against a similar globe, in the manner already adopted by Regnault, filled with hydrogen, and its weight again determined. The weight of hydrogen taken was about 0·42 gramme. The hydrogen was then driven over the heated copper oxide by a current of dry air, and the water formed collected partly in a weighed tube, and partly by means of sulphuric acid and phosphoric oxide. The hydrogen was obtained by three different methods: (1) by the action of sulphuric acid

upon zinc; (2) by means of caustic potash and aluminium; and (3) by electrolysis. In all, sixteen experiments are given. The results are stated in the following table:—

Series.	No. of Experiments.	Sum of the weights of hydrogen. grammes.	Sum of the weights of water. grammes.	Atomic weight of oxygen. max.	min.	Calc. from sums.
I.	5	2·0876	18·7406	15·977	15·937	15·954
II.	5	2·0803	18·6740	15·962	15·942	15·953
III.	6	2·5350	22·7541	15·967	15·937	15·952

The final mean is O = 1 0·0017.

This very bald account does but scanty justice to the beauty and simplicity of the methods adopted by Prof. Cooke and Mr. Richards, and to the manipulative skill and patience with which they carried them out. With respect to the bearing of their result on Prout's hypothesis, the question seems to them to narrow itself to this point: Is the hydrogen they have made use of the typical hydrogen element? They are inclined to believe that the theoretical question in regard to Prout's law has been settled so far as analytical work can solve the problem. On this point, however, we are at issue with them. That statement implies a finality about our present quantitative methods which we have no right to assume. It implies, too, that the methods employed by the authors have yielded as close an approximation to the typical element as we are ever likely to obtain. Their method in principle no doubt removes one fundamental objection to Dumas' plan of work, considered as an experimental process, but it by no means removes all the sources of error, and anybody who will patiently sift out these sources and seek to appreciate their net effect must admit that the ultimate tendency is to apparently lower the relative value of the atomic weight of oxygen.

If we have regard to this fact, and if we consider too what we may call the volumetric evidence, as given above, it seems premature to assume that the ultimate question has actually been narrowed down to the point to which Prof. Cooke and Mr. Richards are disposed to conclude that they have brought it. T. E. THORPE.

PHYSICAL GEOGRAPHY OF THE SEA.

Handbuch der Ozeanographie. Von Dr. Georg von Boguslawski und Prof. Dr. Otto Krümmel. Two Vols. (Stuttgart: Engelhorn, 1884–87.)

THESE volumes belong to a series of geographical hand-books—each written by an acknowledged master of the subject—brought out on a uniform plan, under the editorship of Prof. Dr. F. Ratzel, by the well-known Stuttgart publishing firm of Engelhorn.

Dr. Ratzel's own volume on Anthropogeography, Dr. Hann's on Climatology, and Dr. Heim's on Glaciers have already appeared; and now, under the somewhat novel name of "Oceanography," the physical geography of the sea has been exhaustively treated. The editor has divided the subject into two parts, and given each to a specialist. In Volume I., Prof. Dr. G. von Boguslawski, of the Hydrographical Department of the German Admiralty, treats of the distribution, physical condition, and chemical composition of sea-water; while in Volume II., Dr. Otto Krümmel, Professor of Geography in the University of Kiel, discourses on the motions of the ocean. Each part is complete in itself, but the index to both is given only in the second volume.

It is somewhat difficult to give an idea of the multifarious contents of this exhaustive treatise. In Volume I., Dr. Boguslawski begins with an account of the shape and area of the different oceans, and then naturally describes successively the physical character of their various coast-lines, and the depths of the sea all over the world. But we must remark on the poverty of illustration and the absence of maps which characterize this and so many other first-rate German books. Here we have actually no map, however rough, to show graphically the depth of the oceans. The unfortunate reader who wants to get his information as easily and quickly as possible has first to read through about 100 pages of closely-printed type, and then to try and picture to himself the relief of the floor of the sea.

After discussing ocean depths, the author devotes a short chapter to the chemical composition of salt water; and then a somewhat longer space to the density or specific gravity of the sea. Colour and transparency are next discussed, and the last 200 pages are occupied with what is called maritime meteorology.

Of this space only thirty-four pages are devoted to wind and storms, and very rightly, as these subjects would require a special volume for their proper treatment. The remainder is occupied with a minute account of the temperature of the ocean, both on the surface and at various depths; and with a notice of the distribution of ice in high latitudes. Here too, the value of the admirable text is greatly diminished by the absence of maps.

So far no mathematics have been required, but it is impossible to treat of the motions of the sea without algebraical formulæ. Dr. Krümmel, however, uses great judgment in only giving the formulæ of motion, which only involve simple algebra, and not the investigation of the formulæ, that would require much higher analysis.

In the second volume he begins with the consideration of waves. The theory of wave-motion, both in deep and shallow water, he gives first, mostly following Airy; while experimental illustrations, and observations on the actual length, height, and speed of waves follow next. Breakers and rollers are then discussed, together with their influence on the abrasion of coast-lines. Earthquake and volcanic waves are illustrated by a self-recorded tidal trace from South Georgia, which will be new to English readers; and the section ends with a capital account of stationary waves, *sèches*, and of the curious tidal phenomena in the Straits of Euripus, which so puzzled the ancient Greeks.

The author then turns to tides, dealing mostly with the theories of Laplace, Whewell, Ferrel, Airy, Thomson, &c., but very wisely ending with a chapter on "unsolved problems." The difficult subject of the vertical circulation of the ocean is next discussed, and an admirable account given of the cold *aufreibwasser*—up-rubbed water—of tropical weather coasts. This uneuphonious term is applied to the cold water that is found

close to many tropical shores off which the wind blows steadily. For instance, near Cape Guardafui, when the south-west monsoon blows off shore, cold water is found near the land, but when the north-east monsoon blows on shore nothing but warm water can be discovered. The theory is that an off-shore wind blows, or rubs, the sun-heated surface water to leeward, and that the proper level of the sea is maintained by cold water welling up from below. Mr. J. Murray, of the *Challenger*, has discovered a similar effect in the long, narrow, deep waters of Loch Ness. With a south-west wind the coldest water is at the south-west end of the lake, but when the wind changes to north-east the lowest temperature is found at the north-east extremity.

The remainder of the work is taken up by a description of the currents of the ocean. The theory, of course, is fully given, and we may note that the author uses Ferrel's formula for the deflection of a moving particle to the right, through the influence of the earth's rotation, which has been accepted in every country except England. The long detail of the currents in different oceans of course contains little novelty, but is illustrated by an excellent map in blue and red of the direction and velocity of these well-known cold and hot streams.

Both of these volumes are to a certain extent uncritical compilations, for the results of various experiments and observations are merely recorded, without any comment on the varying quality of the work. We have already commented on the absence of maps; and the instruments used in oceanic research might well have been much more copiously illustrated. Still this work is a most valuable addition to the literature of the subject, and we wish that it could be translated into English.

There is no text-book of the subject in England, beyond School-Board primers, except the work of Maury; and this, in spite of a fascinating style, is too fanciful, and too much out of date, to be of any use.

Though the volumes now under review can never be popular in the ordinary sense of the word, still they would be invaluable to scientific men and others, who though not specialists wish to study in a compact and available form the present state of knowledge of one of the most interesting branches of modern research.

RALPH ABERCROMBY.

BULLETIN OF THE UNITED STATES FISH COMMISSION.

Bulletin of the United States Fish Commission. Vol. VI., for 1886. (Washington: Government Printing Office, 1887.)

THE immense number of short articles in this volume are as usual classified in a topical synopsis of the contents. The largest class is that of articles concerning the fisheries, the next in size contains those concerning aquiculture, the next those concerning natural history, and there are two other classes headed U.S. Fish Commission—General, and Miscellaneous. Of the biological articles Mr. John A. Ryder contributes only three, and the reader regrets there are not more from his hand. One is on the early development of the toad-fish, *Batrachus tau*, whose eggs are described as adherent, being fixed to the under surface of submerged boulders. The young toad-fish have this unique peculiarity, that when the egg-membrane bursts they are not set free but the lower surface of the yolk-sac remains firmly fixed to the adherent portion of the membrane, and this adhesion continues until the yolk-sac has become almost entirely intra-abdominal. The second of Mr. Ryder's papers is on the cleavage of the blasto-disk in the ovum of *Raja erinacea*; and the third on the intra-ovarian gestation of the viviparous *Sebastes marinus*: this last is based upon the examination of a gravid specimen obtained by the *Albatross*.

The few articles on the reproduction and generative organs of eels are of little value, as the information contained in them is not up to date. One, for instance, is a translation of a paper by Prof. Pavesi, published in 1880, and therefore of course treating as probabilities propositions concerning the testes which were proved in 1881 by Otto Hermes.

Among the aquicultural articles there are a great many on the shad-hatching work of the Commission, most of them detailing statistics of the operations of 1886. In one of these Marshall McDonald announces that for the entire period of the Commission's work up to and including 1882, 200,000,000 of young shad were produced, while for 1886 alone the total was 90,000,000, and this last number was fifteen times as great as the number of adult shad captured for market in one season. In another report by the same writer we find that the cost of production of shad-fry was $127·66, or about £25, per million. The exact effect of the artificial production of shad-fry on the supply of the adult fish is not estimated, but in one place we find that the catch in the Potomac was much larger in 1886 than in 1885; and in another that in Connecticut pollutions and sewage are diminishing the number of shad in the rivers.

Evidence is given that shad are now fairly abundant on the whole coast of California, apparently from plantings in the River Sacramento, but no regular run of shad seems to have been produced in that river; and we find statistics of plantings in 1886, in the Columbia and Colorado, from which a better result is expected. But of course the Bulletin is not the place to look for a connected and logical discussion of the operations carried out and their results. The publication contains occasional notes and statistics which are interesting to those who are familiar with the matters to which they belong, and which place on record facts which form materials for a connected study.

Of the very large amount of information comprised under the heading Fisheries, we cannot say more here than that it includes details and statistics not only of American fisheries but of those of all parts of the world.

OUR BOOK SHELF.

Flour Manufacture: a Treatise on Milling Science and Practice. By Friedrich Kick; translated by H. H. P. Powles. (London: Crosby Lockwood and Son, 1888.)

THE art of flour-milling, which of late years has undergone changes in its method of the most marked character, has at no time been productive of anything like a copious technology; and, in the attempt to supply this deficiency, it was natural that Mr. Powles should turn his regard towards Austria, where the manufacture of flour had engaged the attention of scientific experts long before the

necessity for systematic inquiry into its processes became obvious in this country. The publication of Dr. F Kick's supplement to his treatise "Die Mehlfabrication," which tabulated the improvements in machinery for preparing and grinding cereals introduced up to the year 1883, placed at the disposal of the translator a manual complete in its investigations into the nature of grain from the miller's technical standpoint, and into the best means of reducing it to flour. It is true that the book does not concern itself with the construction of the mill building nor with the motive power to be employed; but, from this point onward, the leading principles which should guide the milling engineer are carefully and accurately related, and their application justified when necessary by mathematical demonstration; the *rationale* at the same time being within the comprehension of the practical miller. Of this method the chapters on "balancing millstones" (p. 113), and on "disintegrators" (supplement, p. 25), afford admirable examples. The various operations of grain preparation, grinding, and of bolting, sifting, and dressing the meal, with descriptions and plates of the best known machines employed, are fully detailed, whilst the controversy between the advocates of "high" milling and "low" milling is discreetly adjusted by the author in the incidental remark that "which of these methods is to be used can only be settled by the local demand, if, as is generally the case, the mill works for the home market."

It is, however, to those portions of the work which relate to roller-mills that the reader at the present time will probably turn in the first instance. He will find here, not only information as to the various kinds in use and as to the manner in which they have been found to perform their work, but an intelligible account of the operations involved in the reduction of cereals by rollers, and good reason shown why the time honoured millstones have become almost entirely discarded in the manufacture of wheaten flour.

The book is very fully illustrated by woodcuts throughout the text, and by some thirty supplementary sheets of diagrams; whilst a preliminary chapter contributed by Dr. August Vogel, of Vienna, on the histology of farinaceous grains, adds completeness to the work.

We congratulate the translator on his introducing to the English reader a volume of the utmost value to millers and engineers, and of great interest to many other persons more or less concerned with this important industry.

Elements of Chemistry: a Text-book for Beginners. By Ira Remsen. (London: Macmillan and Co., 1887.)

OPINIONS no doubt differ much as to what is simple enough for a beginner. A good deal depends on the age of the beginner. We hold, in opposition to the author in his preface, that the present production is not well adapted for very young pupils.

There is a good deal of promise in the book which might be better fulfilled, and there is an attempt to cover far too large a field, with the result—not intended by the author—that it reads more like a book on general chemical information than an elementary introduction to chemistry.

Metals and non-metals are dealt with under "family" groups, and most of their common, and many uncommon, compounds described, generally with formulæ, and this in cases and with equations which cannot be termed simple; for instance, technical processes like soda-making, or bleaching powder, or potassium chlorate, or nitro-benzene, &c. Otherwise the order and arrangement of matter and the questions attached to each section are most excellent, and the book would be most useful even for general reading, exercise, and information on the chemistry of common things to the great mass of partially informed, ordinarily well educated, people of any age. To the senior boys of public schools, who have already had a little instruction in science, this book would be really useful, as taking them in a different manner over ground already partially covered, widening their general knowledge, and cultivating the main thing, "*thinking.*"

A Primary Geometry, with Simple and Practical Examples in Plane and Projection Drawing, and suited to all Beginners. By S. E. Warren, C.E. (New York: Wiley and Sons; London: Trübner, 1887.)

THIS work bears as motto, "Geometry should be begun as early and as simply in behalf of industrial life as arithmetic is in behalf of business life"; and its object is, accordingly, to contribute to a general earlier beginning of the study of geometry. "The truths of *form*, as needed in *drawing*, have been made prominent, while not neglecting elementary ones of *measure*."

The text treats of straight lines, triangles, regular figures, areas, lines and planes in space, the elementary bodies, and projections of elementary solids, the subject being considered in a common-sense fashion without much use of purely geometrical proofs. Having perused a very large portion of his book without detecting any flaw, we consider the author competent for the task he has undertaken, but we do not take kindly to such presentments of geometry. We believe, however, the book to be well adapted to junior pupils as an introduction to the study, and also to artisans and others who are likely to be able to grasp the illustrations given better than they would purely geometrical proofs for which their antecedents have not prepared them.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

The Duke of Argyll's Charges against Men of Science.

I REGRET to find that the Duke of Argyll has once more evaded the point at issue. The question is one not of *formulas* but of *facts*. If the statements upon which his Grace bases the severe strictures of his "Great Lesson" were *true*, I for one should take no exception to any "metaphorical or rhetorical expression" by which he chose to enforce his lesson.

Three months have elapsed since the Duke's attention was directed to the discussions which during the last seven years have taken place upon the subject of Mr. Murray's theory of coral reefs—and especially to that one in which the Director-General of our Geological Survey, and the most eminent of American geologists, Prof. J. D. Dana, bore the leading parts; the Duke has been referred to the scientific journals in which this and the other discussions have been carried on; and the fact has been pointed out to him that all the principal text-books of geology, foreign as well as British, which have been published since the theory was announced, have given it a prominent position in their pages. In the face of these facts, is the Duke of Argyll still prepared to maintain that, with respect to the theory in question, there has been "a grudging silence as far as public discussion is concerned"; that there has been "a silence of any effective criticism"; and that "no serious reply has ever been attempted"? If his Grace admits that he was mistaken in making these assertions, is he prepared to withdraw them and also the comments which he has based upon them?

Instead of doing anything of the kind up to the present, the Duke of Argyll has fathered two stories about the wrong-doings of geologists—both of which stories have as little foundation in fact as his statements in "the Great Lesson."

The first of these stories is related in very circumstantial terms, but without any authority being given for it. It is said that a Fellow of the Geological Society offered a certain paper, which the authorities of the Society refused; and it is asserted that the reason of their refusal was that the paper "was not orthodox," and "they probably smelt heresy." Now the Duke of Argyll is well aware that every Fellow of the Geological Society has the right to present papers for reading, and that the responsibility for accepting or refusing papers rests in the first instance with the President; but he, in the case of exercising his veto, is bound to report the fact, and the reasons for his action, at the next meeting of the Council. The records of the Society show that no such paper was ever offered to it; that the President never exercised his right of veto; and that the Council never discussed the grounds of the supposed refusal. The Duke of Argyll has been informed of these facts, but he has not yet retracted the very serious charge which he has made affecting the honour and good faith of the President and the other twenty-two members of the Council of the Geological Society.

In the case of the second story circulated by the Duke of Argyll, the authority is given. The complaint is made that since 1862 "advanced geologists" have "ignored" views which "tend to dethrone" their own "pet theories." Anyone who chooses to refer to the *Philosophical Magazine* for 1862 will see that the "pet theories" in question are those relating to the antiquity of man; that the "advanced geologists" implicated in the charge must have been the late Sir Charles Lyell, Prof. Prestwich, and those who have followed up their researches and arguments; and that the "views" which they "ignored" were the suggestions which I described in my last letter!

JOHN W. JUDD.

The Total Eclipse of the Moon of 1888 January 28, as observed at Birr Castle Observatory, Parsonstown.

THE total eclipse of the moon on Saturday last was, like its predecessor in 1884 (see NATURE, vol. xxx. p. 589, and Trans. Royal Dublin Society for October 1885), favoured by a very clear sky during the whole time of its progress, so that very extensive observations of the changes of the moon's heat in consequence of the passing over of the earth shadow could be made. The apparatus used was essentially the same as that used before; yet the two old thermopiles had been replaced by two new ones especially made for this occasion by the Earl of Rosse.

The observations began at 7h. 19m. M.T. Greenwich, and were, as much as possible, uninterruptedly continued till 15h. 45m.

During this time 638 distinct readings of the galvanometer were obtained, which, when fully reduced, will enable a very satisfactory heat-curve to be drawn. A few preliminary results, reduced to zenith, I communicate at once.

Galvanometer.	
739·4	... 1h. 10m. before first contact with penumbra.
663·4	... 24m. ,, ,, ,,
624·1	... First contact with penumbra.
252·1	... ,, shadow.
34·9	... 22m. before beginning of total phase.
30·2	... 22m. after ,, ,,
231·9	... Last contact with shadow.
545·6	... ,, penumbra.
540·8	... 1h. 34m. after last contact with penumbra.

From these figures it will be seen—

(1) That the heat radiated by the moon begins to decrease a considerable time before the first contact with the penumbra.

(2) That 22m. before the beginning of totality the heat is only 4·7 per cent. of the value obtained 1h. 10m. before the first contact with the penumbra. Unfortunately an unforeseen stoppage of the driving-clock prevented the observations from being carried on closer up to and during the total phase.

(3) That in spite of the rapid fall on approach to totality, the heat, after the last contact with the penumbra, does not at once increase to anything like the value observed at corresponding times before the first contact.

It is worth remarking that points 2 and 3 are confirmatory of the results arrived at in 1884. OTTO BOEDICKER.

Birr Castle Observatory, Parsonstown, January 30.

"Elementary Chemistry," and "Practical Chemistry."

I CRAVE leave from the Editor for space in which to reply, on my own behalf and on that of my fellow-authors Messrs. Slater and Carnegie, to the charges brought by "H. E. A." in NATURE of January 19 (p. 265) against our method of teaching chemistry. At the outset I thank "H. E. A." for the patience which, as he publicly announces, he has shown in waiting for the publication of these books, and I condole with him in his disappointment. Like him, I too am waiting patiently; I trust my disappointment will be less bitter.

One of the important points in our plan of chemical teaching is the connection of the work in the laboratory with the student's reading and lecture-work. To emphasize this connection, and to make our course run fairly smoothly, we have published two books, one to be used in the laboratory, the other to be used in the lecture-room and in reading in connection with the whole work of the student. "H. E. A." acknowledges the advantages of this division, but throughout his review he ignores the statement distinctly made by us, that one book is complementary to the other and that both must be used together. He confines his remarks almost wholly to one of our books, viz. the "Practical Chemistry"; and yet he condemns our system of teaching. On this ground alone I claim that his review is misleading and unfair. I go further, and assert that "H. E. A." has condemned our system without acquainting himself with its essential features. He says that "in the earlier part of the 'Practical Chemistry' Messrs. Muir and Carnegie do not sufficiently bear in mind their own intention, and that much of the matter would find a more fitting place in the companion volume." No one reading this would suppose that almost every experiment used in Chaps. I. to VIII. of the "Practical Chemistry" is also used in Chaps. I. to IX. of the "Elementary Chemistry." Yet this is the case. In one book the experiments are described, along with others, in such terms as allow attention to be concentrated on their results and on the reasoning on these results; in the other book the experiments are described in detail in order that the student may repeat them in the laboratory. In another part of his review "H. E. A." says that most of the subjects dealt with in the third part of the "Elementary Chemistry" "ought never to have been introduced into an 'Elementary Chemistry.'" He has here made a slip: it is the third part of the "Practical Chemistry" which includes subjects not touched on in the other book. This correction involves a point of some importance. Although the preface to our "Practical Chemistry" states that the book forms part of a course of elementary chemistry, yet the student who uses both books will see that the course of work laid down in the practical book carries him much beyond the limits of treatment adopted in the other volume. There are numerous direct and indirect indications of this in the book itself, which those for whom the work is intended will not fail to notice. One cannot put the whole of one's book into the preface. I admit that it would have been better had we indicated in the preface to the "Practical Chemistry" that many experiments in Parts II. and III. are difficult to perform, and require skill and training; but I assert that the nature of the experiments themselves, the references to the original papers to be read before conducting these experiments, and the suggestions as to other work to be done preparatory to Parts II. and III. respectively, suffice to indicate to the student, although not necessarily to the reviewer, the character of the work described in the later chapters of the "Practical Chemistry."

Chapter I. of Part III. of the "Practical Chemistry" involves a repetition of some of Stas's determinations of the atomic weight of silver. "H. E. A." says that this chapter should have been included in Part I., and he adds, "the remaining chapters ought never to have been introduced into an 'Elementary Chemistry,'" kindly informing his readers that these chapters are included "because of the senior author's well-known tendency to worship physical constants." I venture to remind "H. E. A." that no election has taken place to the office of supreme pontiff of chemistry. Were that official in existence, I feel inclined to think he would admit that accurate determinations of atomic weights—and "H. E. A." allows these in the most elementary part of the course—are determinations of constants which have physical as well as chemical meanings.

"H. E. A." says that in the "Practical Chemistry" there is an "entire absence of anything approaching to a *systematic* arrangement." The boldness and baldness of the assertions made by the reviewer encourage me to meet this statement with

a direct denial. There *is* a systematic arrangement in the whole book, or rather in the whole scheme embodied in both books. Because "H. E. A." fails to discover that plan which finds favour with him, it does not follow that systematic arrangement is absent. To say that the arrangement is not that which one would like to see adopted is fair criticism; but to imply that there is no alternative between one's own system and chaos is to expose one's own ignorance. And what is the feature of that system of practical chemistry in which alone the reviewer thinks the student can find salvation? He says, "in a properly chosen series of experiments everything should be proved; no assumption should be necessary." In another part of the review he tells us that "air and the phenomena of combustion should be first studied: the composition of air should be determined, and oxygen should be *discovered* by the student . . . The composition of water should next be qualitatively ascertained." I should be deeply indebted to "H. E. A." if he would kindly describe experiments on these subjects, suited to beginners in chemistry, in which no assumptions are made, and which convey sound teaching. He must not get over the difficulty by cleverly hiding the assumptions made, and so appearing to make none; everything must be proved, and proved by experiments which the beginner can satisfactorily conduct. I hold, and I am convinced that the history of science bears me out, that all scientific reasoning starts with certain assumptions, and that in every particular train of reasoning assumptions are made. If the beginner can be taught to recognize the assumptions which are involved in his reasoning on experimental data, he will do well. In the "Practical Chemistry" we have tried to emphasize the assumptions which the beginner must make. In our opinion the fatal thing is to cover over and hide away the assumptions; by doing this, the student acquires a habit of confounding hypotheses with facts, and so unconsciously he slides into loose methods of reasoning. I fancy I can detect the effects of such a method in the whole review: has not "H. E. A." tacitly, probably unconsciously, assumed that chemical truth abides with him and with him only?

We thank "H. E. A." for indicating some points in the descriptions of certain experiments which might be improved, and also for reminding us that the drawings of apparatus are not as good as they might be. These things can and will be improved. The mistake in the description of the diffusion-experiment, on p. 30 of the "Elementary Chemistry," to which "H. E. A." alludes, has been already pointed out to us, and a slip has been inserted in all copies except the first few hundred correcting this mistake. We cannot congratulate the reviewer, nor do we think he will be inclined on second thoughts to congratulate himself, on the trifling quibbles in which he has indulged regarding one of our experiments on the electrolysis of water.

Cambridge, January 23. M. M. PATTISON MUIR.

"Physical Science and the Woolwich Examinations."

I AM afraid that the moderation of your article on the regulations for admission to the military colleges may give some readers the impression that science is merely being discouraged more or less seriously in their examinations. The fact is, however, that it is being ousted with absolute certainty, for hardly anyone can afford to take up an optional subject which is at a disadvantage of 1000 marks. Severity of competition has within the last few years quite doubled the number of marks qualifying for admission to Sandhurst, and it will soon be impossible, even if it is not so at present, for a candidate to gain a place if he takes up any subject other than Latin, French, German, or mathematics.

This making all the men fit square holes whether they are round or not can hardly be for the advantage of the service, and one's curiosity is aroused as to the reason for such retrogressive changes—whether it is due, as has been asserted, to the action of head masters who do not desire to accumulate or encourage new-fashioned lore; or whether the military authorities really opine that to an officer who may have to deal with telegraphy, to choose a camping-ground, or perhaps direct a search for water, Latin is half as important again as electricity or physical geology.

Is it really too much to expect that they might insist first on a thorough knowledge of those parts of an ordinary education which are specially necessary or helpful to an officer, and then treat the unessential subjects on an equality as far as possible, and let a boy do in his preparation as he will when a man—adequately fulfil the duties of his position, and then follow his own bent? W. A.

January 30.

"The Art of Computation for the Purposes of Science."

HAVING read with much interest Mr. Sydney Lupton's second article on this subject, I think it right to draw his attention, and that of your readers, to Table III. of my book of five-figure and other logarithms published by Messrs. C. and E. Layton in 1870.

This table was framed by me for the purpose of enabling computers who occasionally require to use logarithms to ten places to get same with as little trouble as possible, and without shifting to any other book. In fact, I believe results can be got from my table almost as quickly as from the voluminous and beautiful volume of George Vega.

For instance, referring to Mr. Lupton's example, I find from my table and the instructions that $\log 1{\cdot}0542482375 = \log 1{\cdot}05 + \log 1{\cdot}0040459405$—this by simple division; then—

$$\begin{aligned} &\text{By part A } \log 1{\cdot}05 &&= 0{\cdot}0211892991 \\ &\text{By part B } \log 1{\cdot}0040459405 &&= 0{\cdot}0017535845 \\ \hline &\log 1{\cdot}0542482375 &&= 0{\cdot}0229428836 \end{aligned}$$

correct by Mr. Lupton's solution from Vega.

My whole table is contained in eight octavo pages, and I believe is in as narrow a compass as is consistent with utility.

I may add that in the preliminary part of my book will be found a method of finding the logarithms of all numbers by nothing more than simple multiplication.

The late Prof. Augustus De Morgan, when I showed him this Table No. III., I well remember, replied: "It is very good indeed, but you will get no one to look at it," showing how rarely logarithms are ever required for any practical use beyond five, or at the most seven, figures. E. ERSKINE SCOTT.

6 Bond Court, Walbrook, London, E.C.,
January 18.

THE articles of Mr. Sydney Lupton on the above subject, which have appeared in recent numbers of your paper, do not profess to be complete; still, as their declared object is to assist those who are not mathematicians to work sums by the aid of tables, it seems to me that the best methods should not be passed over in silence, while others that are practically obsolete are discussed at length.

I beg of you therefore to allow me to call attention to the labours of the late Peter Gray, F.R.A.S., in the direction of supplying facilities for computing logarithms and antilogarithms. He contributed papers on the subject to various magazines; notably a series (with a table for formation of logarithms and antilogarithms to twelve places) to the Journal of the Institute of Actuaries in 1865. His most important work on this subject was, however, published as an independent volume in 1876. It is entitled "Tables for the Formation of Logarithms and Antilogarithms to Twenty-four or any less number of places"; and it contains, besides the tables, an explanatory introduction and an exhaustive historical preface. The published price is only 7*s.* 6*d.*, and it is therefore not beyond the reach of those who require such tools.

Weddle's method, the last mentioned by Mr. Lupton, consists in multiplying the given number down to unity, by means of a series of factors of the form $1 - (\cdot 1)_n \times r$, where r may take any integral value from 1 to 9. The logarithms of the factors are then obtained from a previously prepared table, and the complement of the sum of these logarithms is the logarithm of the given number. Weddle also used his method conversely, to calculate antilogarithms.

Hearn, of the Royal Military College, Sandhurst, improved upon Weddle's method, by substituting factors of the form $1 + (\cdot 1)^n \times r$ for the computation of antilogarithms, r, as before, ranging in value from 1 to 9; but he retained the factors $1 - (\cdot 1)^n \times r$ for computing logarithms.

Gray's improvements on Hearn were twofold. In the first place, he gave r the range from 1 to 999, taking for factors $1 + (\cdot 001)^n \times r$, and he thereby brought within narrow compass the arithmetical work involved. In the second place, by a simple arrangement of the calculations, he showed how to use factors of the form $1 + (\cdot 001)^n \times r$, instead of $1 - (\cdot 001)^n \times r$,

for computing logarithms as well as antilogarithms; and thus, not only made the operations more convenient, but also caused one set of preparatory tables to be sufficient.

The principal table in Gray's book above-named consists of the logarithms to twenty-four places of all the possible factors $1 + (\cdot 001)^n \times r$, up to that limit. An auxiliary table contains, also to twenty-four places, the logarithms and their complements of the natural numbers 1 to 9, these being frequently required to "prepare" the given number. A smaller table to twelve figures only appeared, as already mentioned, in the Journal of the Institute of Actuaries, and was subsequently published separately by Messrs. C. and E. Layton; but as the twenty-four-figure table can be worked quite easily to any extent up to that limit, there is no particular advantage in the smaller one.

By means of Gray's tables the work of forming logarithms and antilogarithms is reduced to a minimum, and the process is so simple that any arithmetician can perform it, the more especially as many numerical examples are given in the introduction.

London, January 23. GEORGE KING.

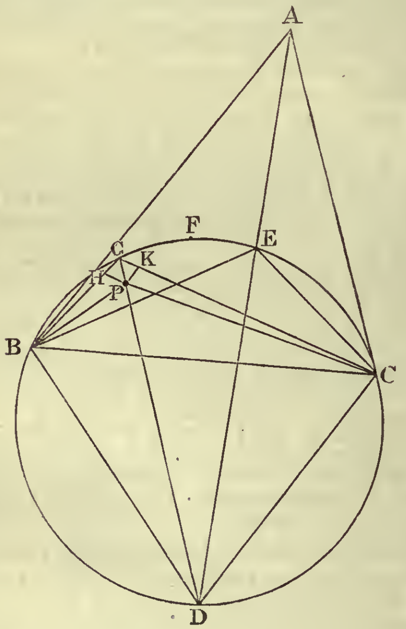
Note on a Problem in Maxima and Minima.

To find a point such that the sum of the straight lines joining it with the angular points of a given triangle shall be a minimum.

This problem was proposed by Fermat to Torricelli, who solved it, and sent it to Vincent Viviani, who also solved it, but called it a problem "quod, ut vera fateor, non nisi iteratis oppugnationibus tunc nobis vincere datum fuit."

The solution is given in Gregory's "Examples of the Differential and Integral Calculus," and in Todhunter's "Differential Calculus," pp. 240–42.

Yet it can be solved in the most elementary manner.



Let ABC be the triangle. Describe an equilateral triangle on BC on the side remote from A. Describe a circle round the triangle BCD. Join AD. Then E is the point required. Join BE, CE.

(1) It follows, from Euc. vi. D, that

$$BE + EC = ED,$$
$$\therefore BE + EC + AE = AD,$$

and evidently $\angle BEC = BEA = AEC = 120^\circ$.

(2) Let F be a point on the circumference BC.

$$BF + FC = FD \quad \text{(Euc. vi. D)},$$
$$\therefore BF + FC + FA = FD + FA > AD.$$

(3) Let P be a point not on the circumference. Join DP, and produce it to the circumference at G. Let fall the perpendiculars PH and PK, on GB and GC respectively.

$$\text{By Euc. i. 26, } GH = GK = \tfrac{1}{2}GP.$$
$$\text{Since } \angle GPH = 30^\circ = GPK,$$
$$\therefore BH + KC = PD,$$
$$\therefore BP + PC > PD,$$
$$\therefore BP + PC + PA > PD + PA > AD.$$

(4) It also follows from the above that if $\angle A = 120^\circ$, then the point required is $A \equiv E$.

If $\angle A > 120^\circ$, the point A will be within the circle, and A itself will be the point required. R. CHARTRES.

Note on the Dimensions and Meaning of J, usually called the Mechanical Equivalent of Heat.

THE title "mechanical equivalent of heat" tends to make one consider that J means the ratio of a quantity of mechanical energy to an equivalent quantity of heat; but since heat *is* mechanical energy (in a molecular form) it follows that J on this supposition is equal to *unity*, and therefore unnecessary.

Another way in which J is sometimes regarded is as the ratio between the ordinary *units* of heat and work; that is to say, in England, it is the ratio of the British thermal unit to a foot-pound, viz. the number 772. This definition makes it a simple number, the number of work units in a heat unit, a number which depends on the units of heat and work employed, and is different in France and England.

Now although J generally has one or other of these significations—that is, must be either unity or some pure number—yet people speak of the *dimensions* of J as being, not zero, but $\dfrac{\text{Work}}{\text{Mass} \times \text{Temperature}}$.

It is evident that there must be some confusion here, a confusion arising from the fact that most people when *talking* of quantities *mean* only so many times the units of those quantities, and so are not always sufficiently careful about the definitions of the various quantities which they introduce.

Now if we confine our attention to quantities themselves, independently of any systems of measurement, we shall be led to a perfectly consistent mode of regarding J, a way moreover in which it will have the required dimensions $\dfrac{\text{Work}}{\text{Mass} \times \text{Temperature}}$.

A British thermal unit is the heat required to raise a pound of water at freezing-point through 1° F., and Joule discovered that the mechanical equivalent of that amount of heat was about 772 foot-pounds.

Hence if we wish to consider the work necessary to raise any other mass of water at freezing-point through any small range of temperature, we have only to notice that the quantity $\dfrac{\text{Work}}{\text{Mass} \times \text{Range of Temperature}}$ is constant, and equal to $\dfrac{772 \text{ foot-pounds}}{1 \text{ pound} \times 1^\circ \text{ F.}}$.

This quantity is very fitly denoted by J, and might, if thought convenient, be called *a Joule*.

But this quantity is the *specific heat of water*, according to the definition that specific heat is the heat required to raise a mass through a small range of temperature *divided by* the mass and the range. So that we have arrived at these conclusions: a quantity of heat is the same thing, whether expressed in British thermal units, or in foot-pounds, or in terms of any other standard; and the specific heat of water at 0° C. is denoted by the letter J.

Indeed it may be said that the result of Joule's experiments is the determination of the specific heat of water in absolute measure. Again, if c is the ratio of the specific heat of any substance to that of water, the full expression of its specific heat is cJ; that is, its specific heat is some multiple or fraction of a *Joule*.

The first law of thermo-dynamics will then be expressed as—

$$dQ - p \,.\, dV = cJm \,.\, d\theta + m \,.\, dI,$$

where $dQ - pdV$ is the total energy supplied, $cJmd\theta$ is the amount of new energy evidenced by increase of temperature, and mdI is the increment of the latent energy of the body.

Coopers Hill, Staines, January 19. ALFRED LODGE.

The Temporary Thermo-Current in Iron.

IN the *Philosophical Magazine* for January, Mr. Herbert Tomlinson has proposed an explanation of the remarkable fact that in an iron wire, heated red hot by a burner, an electric current is produced when the flame is shifted along the wire (see Wiedemann's "Galvanismus," ii. 453).

As his explanation is inadequate, perhaps I may be excused again drawing attention to this subject. Briefly his explanation is as follows:—That, as the portion of the wire in the flame rises in temperature, it, thermo-electrically speaking, becomes in fact like a different metal, and that then, on shifting the flame, the junction with the unaltered wire on the side moved towards becomes hotter than before, while the one on the other side falls in temperature, thus presenting the ordinary case of a thermo-couple with junctions at different temperatures. Now this explanation entirely overlooks the fact that, by the first assumption, just as fast as the temperature on one side rises, the wire there changes into the "second state," and correspondingly changes back on the other side as the temperature falls there; so that, as far as this explanation goes, there ought to be no current whatever, for thus both junctions must always be at the same temperature.

When I first noticed this current, which from considerations to follow I have ventured to call the "temporary thermo-current," it appeared to me to be due to the difference in the temperature-slope (or gradient) along the wire in front from that behind the flame, as it heats more rapidly in front than it cools behind, and to the electromotive force being a function of the slope, *i.e.* of $\frac{d\theta}{dx}$. But this hypothesis did not stand the test of experiment, as I have shown in a paper published in the Proceedings of the Royal Dublin Society, July 1886. So that as there *is* a current, we must suppose the "second state" to be not only a function of the temperature, but also of the time, *i.e.* that the wire changes into (or from) the "second state" more slowly than it is possible for it to change in temperature. So that the electromotive force at any point depends on the rate of change of the temperature slope, or equals $\phi\left(\frac{d}{dt}\frac{d\theta}{dx}\right)$. In support of this it will be found that if the flame be steadily moved along very slowly no current is produced—at all events less than would be otherwise expected; and, secondly, that the maximum current is got by moving the flame the fastest consistent with the condition of keeping the wire red hot.

It is with the view of emphasizing this dependence on the time that the term "temporary thermo-current" seems appropriate. FRED. T. TROUTON.

Physical Laboratory, Trinity College, Dublin.

Causes influencing the Bathymetrical Range of Deep-Sea Fishes.

YOU refer (p. 219) to the fact that Dr. Günther has adopted the 100-fathom line as the boundary at which with the extinction of sunlight the bathybial fauna commences. This selection of 100 fathoms as the limiting horizon is of much interest in connection with the theory that the shallow-water marine fauna is greatly influenced by wave-currents. In a letter you published in 1885 (NATURE, vol. xxxii. p. 390) I indicated 100 fathoms as the depth to which wave-action of some sort must extend, as evidenced by the character of the deposits at the mouth of the English Channel. Dr. Günther now shows that the deep-sea fishes do not rise above that horizon. But, although the 100-fathom horizon agrees very well with the apparent limit of wave-action, it does not seem to agree with the most recent experiments on the penetration of sunlight in water.

So recently as November last you recorded the fact that during the past year Prof. Forel found that the greatest "depth-limit of absolute darkness" from March to July in the Lake of Geneva was 100 metres (NATURE, vol. xxxvii. p. 88). If experiments in a fresh-water lake may be taken as a guide to light-penetration in the ocean, 50 fathoms will be nearer the limit than 100. In this case the bathymetrical range of the bathybial fauna cannot be much influenced, if at all, by the presence or absence of sunlight. This view is moreover fortified by the fact that, though the deep-sea forms do not usually ascend above the 100-fathom line, the shallow-water forms go far below it; and there is no reason why they should not do so; for, although a form unfitted to withstand wave-currents cannot face them, there is nothing to prevent a flat-fish, fully equipped in this respect, from passing at will from the disturbed to the tranquil horizon, and *vice versâ*. A. R. HUNT.

Torquay, January 10.

Wind Force at Sea.

IN reference to a letter on the above subject in NATURE (p. 274), I beg to acquaint your readers that Capt. Barker's wish that anemometers should be used more on board ship has been endeavoured to be met by an instrument designed by myself on the sail principle. It has now been in use on some ships at sea for long voyages for five years, and daily observations have been obtained and sent home of the data observed, of pressure, direction, and velocity of the winds met with.

Regarding the further inquiry of ascertaining the rainfall at sea, this has now been carried on for about ten years by means of a rain-gauge designed by myself on the pivot principle, and it has been used by many vessels in all the great seas.

The daily observations have been sent home and are now on hand, and about five years of the returns have already been announced, and a further compilation of the data may be prepared when the materials become sufficient.

It may be added that the late Capt. Symington, of the s.s. *Hankow*, amongst his meteorological observations took the rainfall by rain-gauge on his ship for twenty years or more. The marine anemometer and rain-gauge above mentioned were exhibited at the Liverpool Exhibition in 1886, and at the Meteorological Exhibition of last year. W. G. BLACK.

Edinburgh, January 21.

Untimely Insect Development.

SOME of your readers may be interested in a case of untimely insect development, caused no doubt by the phenomenal mildness of the weather in this part of the country during the last few days. Last evening a perfect imago of the common tortoise-shell butterfly (*Vanessa urtica*) was found inside my house on the wall of my nursery. It is fully developed in every way, and the only thing in its appearance at all abnormal is that the antennæ are bent back and lie between the wings, which are in the erect position usual in repose. The insect has evidently only just emerged from the pupa, and is in a torpid condition, only just flapping its wings when touched. The nursery is a warm room looking to the south, and has a fire in it all day.

St. Albans, January 10. JOHN MORISON.

Weasels killing Frogs.

SEEING a note in NATURE (December 29, 1887, p. 208), about weasels killing frogs, I thought that the following fact would be a further confirmation.

I was walking near the village of Clifton Hampden in August last, when I saw a weasel, carrying a good-sized frog in its mouth, come cautiously out of the rank grass by the road-side; directly the weasel perceived me, it dropped its prey on the road and retreated to the cover of the grass. The frog was dead. I kept silence, and the weasel left its hiding-place, and advanced a few steps, but again retreated. Soon, after several advances and retreats, it rushed out, seized the frog with its teeth, and running across the road disappeared in the long grass on the other side.

January 20. M. S. PEMBREY.

"British and Irish Salmonidæ."

THE author of "British and Irish Salmonidæ" calls in question the justice of three criticisms in my review of that book. In reply to his first objection, I have to point out that my quotation of the sentence referred to was, as Mr. Day has himself noted, made to draw attention to its grammatical errors, and therefore the omission of a few words which affected the sense but not the construction was of no consequence at all. I omitted the words intentionally, to shorten the quotation, and gave no opinion on the statement contained in the sentence: the statement which is implied rather than expressed is perfectly correct.

With regard to the second point, the statement in the text of the book which I questioned is as follows:—"The main principle is to employ thin layers of well-packed and pressed moss in trays with perforated bottoms, *the eggs being separated from the moss by muslin, mosquito-netting, swans' down, calico, or butter-cloth*, and that each tray contains two or three layers." In all the methods of packing salmonoid eggs in which moss is employed, the descriptions I have read state that the eggs are placed in direct contact with the moss, and Mr. Day does not justify the statement above quoted by referring to another statement in his notes, that for shorter journeys eggs are thrown off the frames on to swans' down. I doubted, and still doubt, if there is any method practised in which layers of moss are used, and are separated from the eggs by muslin or similar material.

With regard to the third point, it is true that on p. 249 of his book, in the chapter on *S. fontinalis*, Mr. Day refers to Brown Goode's "Game Fishes of the United States," and to the statement in that work that *S. namaycush* has, as its nearest relative, *S. fontinalis*. But I think a more direct reference to a speciegraphical description of *S. namaycush* might have been expected in a footnote referring to errors in the descriptions of this fish by certain writers. My remark about the omission of such reference was not made under the impression that S. *namaycush* was not a char, for I am aware that it is described as such in recent American reports on pisciculture, and have no doubt that such description is correct. But reference to speciegraphical determinations are rare in such reports, and I think readers of Mr. Day's book would have been glad of the references which he now supplies in his letter. YOUR REVIEWER.

MODERN VIEWS OF ELECTRICITY.[1]

PART III. (*continued*).

VI.

LET us now pass in review the various facts and experiences which have led us to a dual view of electricity; a kind of two-fluid theory, but in a very modified form.

First, there are the old experiments which vaguely suggest the separate existence of negative electricity, such as:—

(1) The wind from a point whether positive or negative; so that a candle gets blown always away from it, whether the point be on the prime conductor and the candle held in hand, or whether the point be held in the hand and presented to the candle or prime conductor; so, also, that a point whirligig turns the same way, whether supported on the prime conductor, or whether attached to the earth and placed near it.

(2) Phenomena connected with the spark discharge, such as Wheatstone's old experiment on what he called the velocity of electricity, with the three pair of knobs; and the double burr produced in cardboard when pierced with a spark, suggesting that something has pierced it both ways at once.

Then there are the more recently observed facts; as, for instance:—

(3) The fact that an electrostatic strain scarcely affects the volume of a dielectric; thereby at once suggesting something of the nature of a shearing or distorting stress, which alters shape but not size; a displacement of positive outwards and simultaneous negative inwards.

(4) The facts of electrolysis, and the double procession of atoms past each other in opposite directions.

(5) The phenomena of self-induction, and the behaviour of a thick wire to an alternating current. The delay also in magnetizing iron, and especially the possibility of permanent magnetism; combined with

(6) The absence of momentum in an electric current, or moment of momentum in an electro-magnet, as tested by all mechanical means yet tried.

I admit at once that many of these are mere superficial suggestions which may hardly bear examination and criticism. Only (3), (4), (5), and (6) can be at all seriously appealed to; but (5) and (6), in conjunction, seem to me to afford a sort of provisional and hypothetical proof, which (3) greatly strengthens.

At this point we must for the present again leave the question.

[1] Continued from p. 110.

Representation of a Magnetic Field.

The disturbance called magnetism, which we have shown to be something of the nature of a spin—a rotation about an axis—is conspicuously not limited to the steel or iron of the magnet: it spreads out through all adjacent space, and constitutes what is called the magnetic field. A map of the field is afforded by the use of iron filings, which cling end to end and point out the direction of the force at every point.

These lines of force so mapped are to be regarded as the axes of molecular whirls. They are continuous with similar lines in the substance of the steel, and every line really forms a closed curve, of which a portion is in the steel and a portion in the air. In a wire helix, such as Figs. 16 or 29, the lines are wholly in the air, but in one part of their course they thread the helix, and in another part they spread out more or less between its faces.

But according to Ampère's theory of molecular currents there is no *essential* difference between such a helix and a steel magnet; directly the currents in the molecules of the magnet are considered, everything resolves itself into chains of molecular currents, threading themselves along a common closed curve or axis. Each atom, whether in the steel or in the air, is the seat of a whirl of electricity, more or less faced round so as on the average to have its plane at right angles to the lines of force. The simplest plan of avoiding having to consider those only partially faced round, is to imagine the whole number divided into a set which face accurately in the right direction, and a set which look any way at perfect random, and to neglect this latter set.

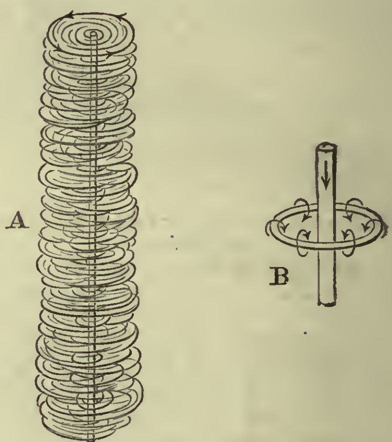


FIG. 30.—A, an element of a magnetic line of force with the electric whirl round it; B, a bit of an electric circuit with one of its magnetic lines of force shown round it, and the electric whirl round this; each magnetic line of force round a current being an electric vortex ring.

Well now try and picture a chain of whirls like beads spinning on a wire threading them all, and think of the effect of a material fluid thus rotating. Obviously it would tend to whirl itself fatter, and to shorten its length. An assemblage of such parallel straight whirls would thus squeeze each other laterally, or cause a lateral pressure, and would tend to drag their free ends together, causing a longitudinal tension.

Such whirls cannot in truth have free ends except at the boundary of a medium—as at the free surface of a liquid. Magnetic whirls are in reality all closed curves;

but inasmuch as part of them may be in a mobile fluid like air, and part of them in a solid like iron or steel, it is convenient to distinguish between their two portions; and one may think of the air whirls alone as reaching from one piece of iron to another and by their shortening tendency or centrifugal force pulling the two pieces together.

The arrangement shown in Fig. 31 illustrates the kind

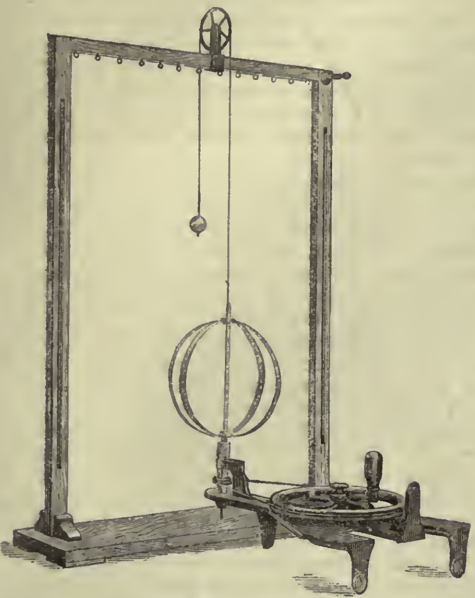


Fig. 31.—A "shape of the earth" model which, when whirled, exerts a tension along its axis, pulling up the weight attached to it, and a pressure at right angles, by reason of its bulging out.

of force exerted by a spinning elastic framework along and perpendicular to its axis of rotation.

One can easily see this effect of a whirl in a tea-cup or inverted bell-jar full of liquid. Stir it vigorously, and leave it. It presses against the walls harder than before, so that if they were elastic they would bulge out with the lateral pressure; and it sucks down the top or free end of its axis of rotation, producing quite a depression or

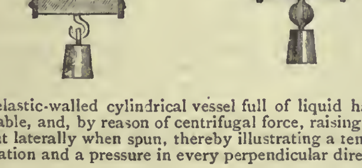


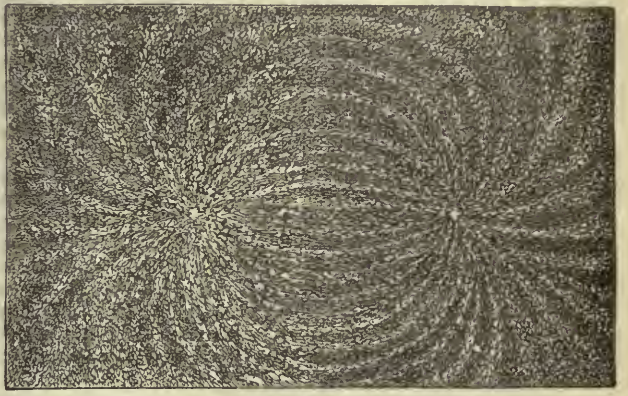
Fig. 32.—An elastic-walled cylindrical vessel full of liquid hanging from a whirling table, and, by reason of centrifugal force, raising a weight and bulging out laterally when spun, thereby illustrating a tension along the axis of rotation and a pressure in every perpendicular direction.

hollow against the force of gravity. Or, as a more striking illustration, make the following apparatus.

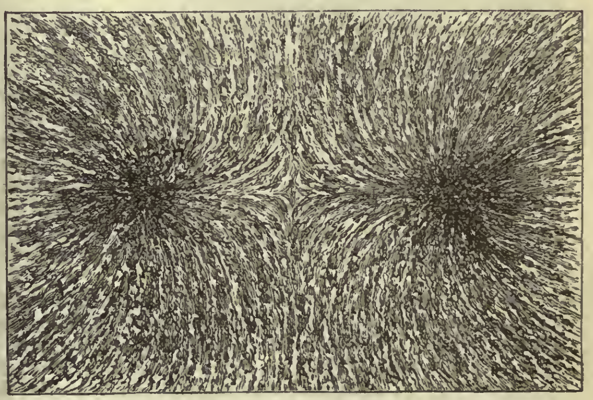
Two circular boards joined by a short wide elastic tube or drum: a weight hung to the lower board, the top board hung from a horizontal whirling table, the drum filled with water, and the whole spun round. The weight is raised by the longitudinal tension; the sides bulge out with the lateral pressure.

There is no need for the whole vessel to rotate. If the liquid inside rotates, the same effect is produced.

Imagine now a medium composed of a multitude of such cells with rotating liquid inside: let the cells be either very long, or else be joined end to end so as to make a chain—a series of chains side by side—and you have a picture of a magnetic medium traversed by a field of force. End-boundaries of the field will be dragged together, thus representing magnetic attraction; while, sideways, the lines of force (axes of whirl) squeeze each



Attraction.



Repulsion.

Fig. 33.—Attraction and repulsion. The tension along the lines of force or axes of rotation drags the one pair of poles together; and the pressure in directions perpendicular to the axis of rotation due to the centrifugal force of the whirls drives the other pair apart.

other apart, thus illustrating repulsion. This is Clerk-Maxwell's view of an electro-magnetic medium, and of the mode in which magnetic stress, and magnetic attractions and repulsions between bodies, arise.

Wherever lines of force reach across from one body to another, those bodies are dragged together as if pulled by so many elastics (Fig. 33); but wherever lines of force from one body present their *sides* to those proceeding from another body, then those bodies are driven apart.

Oliver J. Lodge.

(*To be continued.*)

LANGUAGE-REASON.

The inclosed letter on "Reason-Language" was written to an American friend, and has been published in an American paper in Chicago. I thought it might possibly interest the readers of Nature.

Oxford, January 22. F. Max Müller.

"You tell me that my book on the 'Science of Thought' is thoroughly revolutionary, and that I have all recognized authorities in philosophy against me. I doubt it. My book is, if you like, evolutionary, but not revolutionary; I mean it is the natural outcome of that philosophical and historical study of language which began with Leibniz, and which during our century has so widely spread and ramified as to overshadow nearly all sciences, not excepting what I call the science of thought.

"If you mean by revolutionary a violent breaking with the past, I hold on the contrary that a full appreciation of the true nature of language and a recognition of its inseparableness from thought will prove the best means of recovering that unbroken thread which binds our modern schools of thought most closely together with those of the Middle Ages and of ancient Greece. It alone will help us to reconcile systems of philosophy hitherto supposed to be entirely antagonistic. If I am right—and I must confess that with regard to the fundamental principle of the identity of reason and language I share the common weakness of all philosophers, that I cannot doubt its truth—then what we call the history of philosophy will assume a totally new aspect. It will reveal itself before our eyes as the natural growth of language, though at the same time as a constant struggle of old against new language—in fact, as a dialectic process in the true sense of the word.

"The very tenet that language is identical with thought —what is it but a correction of language, a repentance, a return of language upon itself?

"We have two words, and therefore it requires with us a strong effort to perceive that behind these two words there is but one essence. To a Greek this effort would be comparatively easy, because his word *logos* continued to mean the undivided essence of language and thought. In our modern languages we shall find it difficult to coin a word that could take the place of *logos*. Neither *discours* in French, nor *Rede* in German, which meant originally the same as *ratio*, will help us. We shall have to be satisfied with such compounds as thought-word or word-thought. At least, I can think of no better expedient.

"You strongly object to my saying that there is no such thing as reason. But let us see whether we came honestly by that word. Because we reason—that is, because we reckon, because we add and subtract—therefore we say that we have reason; and thus it has happened that reason was raised into something which we have or possess, into a faculty, or power, or something, whatever it may be, that deserves to be written with a capital R. And yet we have only to look into the workshop of language in order to see that there is nothing substantial corresponding to this substantive, and that neither the heart nor the brain, neither the breath nor the spirit, of man discloses its original whereabouts. It may sound violent and revolutionary to you when I say that there is no such thing as reason; and yet no philosopher, not even Kant, has ever in his definition of reason told us what it is really made of. But remember, I am far from saying that reason is a mere word. That expression, 'a mere word,' seems to me the most objectionable expression in the whole of our philosophical dictionary.

"Reason is something—namely, language—not simply as we now hear it and use it, but as it has been slowly elaborated by man through all the ages of his existence on earth. Reason is the growth of centuries, it is the work of man, and at the same time an instrument brought to higher and higher perfection by the leading thinkers and speakers of the world. No reason without language—no language without reason. Try to reckon without numbers, whether spoken, written, or otherwise marked; and if you succeed in that I shall admit that it is possible to reason or reckon without words, and that there is in us such a thing or such a power or faculty as reason, apart from words.

"You say I shall never live to see it admitted that man cannot reason without words. This does not discourage me. Through the whole of my life I have cared for truth, not for success. And truth is not our own. We may seek truth, serve truth, love truth; but truth takes care of herself, and she inspires her true lovers with the same feeling of perfect trust. Those who cannot believe in themselves, unless they are believed in by others, have never known what truth is. Those who have found truth know best how little it is their work, and how small the merit which they can claim for themselves. They were blind before, and now they can see. That is all.

"But even if I thought that truth depended on majorities, I believe I might boldly say that the majority of philosophers of all ages and countries is really on my side (see 'Science of Thought,' pp. 31 *et seq.*), though few only have asserted the identity of reason and language without some timorous reserve, still fewer have seen all the consequences that flow from it.

"Some people seem to resent it almost as a personal insult that what we call our divine reason should be no more than human language, and that the whole of this human language should have been derived from no more than 800 roots, which can be reduced to about 120 concepts. But if I had wished to startle my readers I could easily have shown that out of these 800 roots one-half could really have been dispensed with, and has been dispensed with in modern languages (see 'Science of Thought,' p. 417), while among the 120 concepts not a few are clearly secondary, and owe their place in my list (*ib.* p. 619) merely to the fact that in Sanskrit they cannot be reduced to any more primitive concept. To dance, for, instance, cannot be called a primitive concept; perhaps not even to hunger, to thirst, to cook, to roast, &c. Only it so happens that in Sanskrit, to which my statistical remarks were restricted, we cannot go behind such roots as NÆT, KSHUDH, TÆSH, PAK, &c. It is in that limited sense only that such roots and such concepts can be called primitive. The number of really primitive concepts would be so alarmingly small that for the present it seemed wiser to say nothing about it. But so far from being ashamed of our modest beginnings, we ought really to glory rather in having raised our small patrimony to the immense wealth now hoarded in our dictionaries.

"When we once know what our small original patrimony consisted in, the question how we came in possession of it may seem of less importance. Yet it is well to remember that the theory of the origin of roots and concepts, as propounded by Noiré, differs, not in degree, but *toto cœlo* from the old attempts to derive roots from interjections and imitations of natural sounds. That a certain number of words in every language has been derived from interjections and imitations no one has ever denied. But such words are not conceptual words, and they become possible only after language had become possible—that is, after man had realized his power of forming concepts. No one who has not himself grappled with that problem can appreciate the complete change that has come over it by the recognition of the fact that roots are the phonetic expressions of the consciousness of our own acts. Nothing but this, our consciousness of our own repeated acts, could possibly have given us our first concepts. Nothing else answers the necessary requirements of a concept, that it should be the consciousness of something manifold, yet necessarily realized as one. After the genesis of the first concept, everything else becomes intelligible. The results of our acts become the first objects of our conceptual thought; and with

conceptual thought, language, which is nothing if not conceptual, begins. Roots are afterwards localized, and made the signs of our objects by means of local exponents, whether suffixes, prefixes, or infixes. What has been scraped and shaped again and again becomes as it were 'shape-her',' *i.e.* a shaft; what has been dug and hollowed out by repeated blows becomes 'dig-her',' *i.e.* a hole. And from the concept of a hole dug, or of an empty cave, there is an uninterrupted progress to the most abstract concepts, such as empty space, or even nothing. No doubt, when we hear the sound of cuckoo, we may by one jump arrive at the word cuckoo. This may be called a word, but it is not a conceptual word, and we deal with conceptual words only. Before we can get at a single conceptual word, we have to pass through at least five stages:—

"(1) Consciousness of our own repeated acts.
"(2) *Clamor concomitans* of these acts.
"(3) Consciousness of that *clamor* as concomitant of the act.
"(4) Repetition of that *clamor* to recall the act.
"(5) *Clamor* (root) defined by prefixes, suffixes, &c., to recall the act as localized in its results, its instruments, its agents, &c.

"You can see from my preface to the 'Science of Thought' that I was quite prepared for fierce attacks, whether they came from theologians, from philosophers, or from a certain class of scholars. So far from being discouraged, I am really delighted by the opposition which my book has roused, though you would be surprised to hear what strong support also I have received from quarters where I least expected it. I have never felt called upon to write a book to which everybody should say *Amen*. When I write a book, I expect the world to say *tamen*, as I have always said *tamen* to the world in writing my books. I have been called very audacious for daring to interfere with philosophy, as if the study of language, to which I have devoted the whole of my life, could be separated from a study of philosophy. I have listened very patiently for many years to the old story that grammar is one thing and logic another; that the former deals with such laws of thought as are observed, the latter with such as ought to be observed. No, no. True philosophy teaches us another lesson—namely, that in the long run nothing is except what ought to be, and that in the evolution of the mind, as well as in that of Nature, natural selection is rational selection; or, in reality, the triumph of reason, the triumph of what is reasonable and right; or, as people now say, of what is fittest. We must learn to recognize in language the true evolution of reason. In that evolution nothing is real or remains real except what is right; nay, in it even the apparently irrational and anomalous has its reason and justification. Towards the end of the last century, what used to be called *Grammaire Générale* formed a very favourite subject for academic discussions; it has now been replaced by what may be called *Grammaire Historique*. In the same manner, *Formal Logic*, or the study of the general laws of thought, will have to make room for *Historical Logic*, or a study of the historical growth of thought. Delbrück's essays on comparative syntax show what can be done in this direction. For practical purposes, for teaching the art of reasoning, formal logic will always retain its separate existence; but the best study of the real laws of thought will be hereafter the study of the real laws of language. If it was really so audacious to make the identity of language and reason the foundation of a new system of philosophy, may I make the modest request that some philosopher by profession should give us a definition of what language is without reason, or reason without language? "F. M. M."

FERDINAND VANDEVEER HAYDEN.

WE reprint from the American journal *Science* (January 6) the following article on Dr. Hayden, whose death we lately announced:—Prof. Ferdinand Vandeveer Hayden, M.D., Ph.D., LL.D., who died in Philadelphia on the morning of December 22, was born in Westfield, Mass., September 7, 1829. Early in life he went to Ohio. In 1850 he was graduated from Oberlin College, and soon afterward read medicine at Albany, N.Y., receiving his degree from the Albany Medical College in 1853. He did not begin the practice of medicine, but in the spring of the year of his graduation was sent by Prof. James Hall of Albany, with Mr. F. B. Meek, to visit the Bad Lands of White River, to make collections of the Cretaceous and Tertiary fossils of that region. This was the beginning of his explorations of the West, which continued with little interruption for more than thirty years.

In the spring of 1854, Dr. Hayden returned to the Upper Missouri region, and spent two years in exploring it, mainly at his own expense, although he was aided a portion of the time by gentlemen connected with the American Fur Company. During these two years he traversed the Missouri River to Fort Benton, and the Yellowstone to the mouth of the Big Horn River, and explored considerable portions of the Bad Lands of White River and other districts not immediately bordering upon the Missouri. The large collections of fossils he made were given partly to the Academy of Sciences in St. Louis, and partly to the Academy of Natural Sciences of Philadelphia.

As one of the members of the Geological Survey has recently said, these collections and researches mark the commencement of the epoch of true geologic investigation of the Great West. The collections attracted the attention of the officers of the Smithsonian Institution; and in February 1856, Dr. Hayden was employed by Lieut. G. K. Warren, of the United States Topographical Engineers, to make a report upon the region he had explored; so that the results of his labours during the three previous years were utilized by the Government. This report was made in March of the same year, and in May following he was appointed geologist on the staff of Lieut. Warren, who was then engaged in making a reconnaissance of the North-West. He continued in this position until 1859, when he was appointed naturalist and surgeon to the Expedition for the exploration of the Yellowstone and Missouri Rivers, by Capt. William F. Raynolds of the Corps of Engineers of the United States Army, with whom he remained until 1862. The results of his work while with Liutenant Warren were published in a preliminary report of the War Department, and in several articles in the Proceedings of the Academy of Natural Sciences of Philadelphia for the Years 1857 and 1858, and more fully in a memoir on the geology and natural history of the Upper Missouri, published in the Transactions of the American Philosophical Society, Philadelphia, 1862. This paper also included chapters on the mammals, birds, reptiles, fishes, and recent mollusca of the region in which his geological investigations were carried on. During this period also he found time to make notes upon the languages and customs of the Indian tribes with which he came in contact. These notes were embodied in "Contributions to the Ethnography and Philology of the Indian Tribes of the Missouri River," published in the Transactions of the American Philosophical Society, Philadelphia, 1862; in a "Sketch of the Mandan Indians, with some Observations illustrating the Grammatical Structure of their Language," published in the *American Journal of Science* in 1862; and in "Brief Notes on the Pawnee, Winnebago, and Omaha Languages," published

in the Proceedings of the American Philosophical Society, Philadelphia, 1869.

In May 1862, Dr. Hayden was appointed acting-assistant surgeon of volunteers by the Surgeon-General of the United States Army, and was sent to Satterlee Hospital in Philadelphia. He was confirmed by the United States Senate as assistant-surgeon and full surgeon of volunteers on the same day (February 16, 1863), and sent to Beaufort, S.C., as chief medical officer, where he remained for one year, when he was ordered to Washington as assistant medical inspector of the Department of Washington. On February 19, 1864, he was sent to Winchester, Va., as chief medical officer of the army in the Shenandoah valley. Here he remained until May 1865, when he resigned, and was brevetted lieutenant-colonel for meritorious services during the war. During the remainder of the year 1865 he was employed in work at the Smithsonian Institution. It was during this year that he was elected Professor of Geology and Mineralogy in the University of Pennsylvania,—a position he held until 1872, when the increased executive duties in connection with the Geological Survey of the Territories induced him to resign it.

In the summer of 1866 he undertook another expedition to the Bad Lands of Dakota, under the auspices of the Academy of Natural Sciences of Philadelphia, for the purpose of clearing up some doubtful points in the geology of that region, and returned with large and valuable collections of vertebrate fossils, which were described in a memoir published by the Academy of Natural Sciences of Philadelphia in 1869. From 1867 to 1879 the history of Dr. Hayden is the history of the United States Geological and Geographical Survey of the Territories, of which he was geologist-in-charge, and to the success of which he devoted all his energies during the twelve years of its existence. In this time more than fifty volumes, together with numerous maps, were issued under his supervision. One of the results of his surveys, and the one in which he probably took the greatest interest, was the setting aside by Congress of the Yellowstone National Park. The idea of reserving this region as a park or pleasure-ground for the people originated with Dr. Hayden, and the law setting it apart was prepared under his direction. The work of the Geological Survey of the Territories had its consummation in the Atlas of Colorado, which increased greatly our knowledge of one of the most interesting portions of the Great West. In 1879, after the disbanding of the Survey of the Territories, Dr. Hayden received an appointment as geologist on the newly organized United States Geological Survey. For about three years he was occupied in the completing of the business of the Geological and Geographical Survey of the Territories, and the preparation of the final results of that survey. His health had already begun to fail, but early in 1883 he asked to be relieved from the supervision of the printing of the reports, and during the three following seasons he undertook field work in Montana. By the latter part of the year 1886 his health had become so poor that he was confined most of the time to his bed. He then resigned his position as geologist, closing an honourable connection with the Government that included twenty-eight years of actual service as naturalist, surgeon, and geologist. To the general interest in science excited by the enthusiastic labours of Dr. Hayden in his geologic explorations, is due in a great degree the existence and continuance of the present United States Geological Survey.

In 1876 the degree of LL.D. was conferred upon him by the University of Rochester, and in June 1886 the same degree was conferred upon him by the University of Pennsylvania. Dr. Hayden was a member of the National Academy of Sciences and of many other Societies scattered throughout the country. He was also honorary and corresponding member of a large number of foreign Societies.

As to Dr. Hayden's personal character, those who were personally associated with him know best how genial he was, and how sincere and enthusiastic his desire to forward the cause of science. Although impulsive at times, he was generous to a fault. His subordinates all knew that each one stood upon his own merits, and that due credit would be awarded to his successful efforts. The same spirit actuated him in respect to those not immediately connected with him. His views are expressed as follows in one of his earliest reports, when speaking of those who had preceded him: "Any man who regards the permanency or endurance of his own reputation will not ignore any of these frontier men who made their early explorations under circumstances of great danger and hardship."

His ideas were broad and liberal. He aimed to make a thorough astronomical, topographical, geological, and botanical survey of the Great West, with a view to the development of its mining and agricultural resources. The greater part of his work for the Government and for science was a labour of love.

To the foregoing notice some token of recognition and regret on the part of brother geologists on this side of the Atlantic may perhaps be fittingly appended by one who knew Dr. Hayden personally, was familiar with his writings, and had wandered in his footsteps among the solitudes of the Far West. The first impression which the late geologist made on those who came to know him was one of gentleness, almost of timidity. They could hardly help asking themselves, "Can this be the man who has so successfully won over the blustering Congressmen to grant him year after year such large appropriations for his western surveys; who has organized such wonderful expeditions; who has gone through such hardships, and in an incredibly short space of time has made such excellent reconnaissances and published such voluminous Reports and admirable maps?" It was some time before one could see the real underlying secret of his success. This was undoubtedly a quiet enthusiasm for science, supported by an undemonstrative but indomitable courage, and a determination to gain the proposed end, cost what it might in bodily and mental endurance. No one who has not been in some measure admitted behind the scenes of political wire-pulling in the States, can realize what had to be undertaken by the man of science who would obtain and retain an annual subsidy from Congress for scientific investigation in the days when Hayden carried on his explorations. There were other rival claimants for Parliamentary aid who were doing similar work, under other Government Departments. There was likewise the wide outside circle of scientific men who had no State employment, and some of whom thought themselves at least as deserving of it as those who fortunately had gained it. Then there were the Gallios of Congress, who cared nothing about science of any kind, those who grudged money spent out of their own States, those who required to see on their drawing-room table a well got up Annual Report with pictures and maps before they could be made to believe that the money was well bestowed. And the weeks and months of early summer, so precious for field work, had to be passed in the lobbies of the Capitol, making sure that there would be no failure in the granting of the appropriation. The most wearisome and profitless part of his year was this "lobbying" at Washington. But Hayden had no choice in the matter. He must either go through with that part of his work or abandon his western surveys altogether. This alternative has not always been borne in mind by those who have judged of him.

There can be no doubt that among the names of those who have pioneered into the marvellous geology of Western North America, that of F. V. Hayden will

always hold a high and honoured place. This place will be his due, not only because of his own personal achievements in original exploration. His earlier work exhibits much of that instinctive capacity for grasping geological structure which is the main requisite for a field geologist. He had a keen "eye for a country." But he likewise possessed the art of choosing the best men for his assistants, and the tact of attaching them to himself and his corps. In this way he accomplished much excellent work, keeping himself latterly rather in the background so far as actual personal geological investigations were concerned, and contenting himself with the laborious task of organization and supervision, while he encouraged and pushed forward his coadjutors.

The abolition of his Survey and the appointment of one of his rivals to the post of Director of the reconstituted Geological Survey of the United States, was a blow from which he does not seem ever to have recovered. He was treated, however, with great generosity by the new Director, and had a share of the large annual appropriation to enable him to complete his Reports. He was urged to condense these voluminous works, and to present a concise and readable account of what he and his fellow-workers had done for the geology of the far West. But he had no literary proclivities, and in the end gladly surrendered the task of writing for the more congenial employment of renewing his personal acquaintance with the geology of the Western Territories. Perhaps among those, and there must be many, who personally knew and esteemed him, there may be one competent and willing to compile or complete the summary which he never completed, and thus to erect to his memory a more fitting and lasting monument than one of brass or marble. A. G.

NOTES.

We regret to announce the death of Dr. Asa Gray, the most eminent of American botanists. He died at Cambridge, Massachusetts, on Monday, aged seventy-seven. Next week we shall give some account of his services to science.

Mr. George Godwin, F.R.S., well known as the editor of the *Builder*, died on January 27. He was seventy-three years of age. Among his writings were several works in which, with great earnestness, he pressed upon the attention of the public the evil consequences springing from the neglect of sanitary laws.

Mr. George Robert Waterhouse, late Keeper of the Department of Geology in the British Museum, died at his residence, Curton Lodge, Putney, on January 21, in his seventy-eighth year.

We have also to record the death of the well-known botanist, Dr. J. T. I. Boswell, who was for many years Curator to the Botanical Society in London, and a Lecturer at the Charing Cross and Middlesex Schools of Medicine.

The Medals and Funds to be given at the annual meeting of the Geological Society on February 17 have been awarded as follows:—Wollaston Medal to H. B. Medlicott, F.R.S.; Murchison Medal to Prof. J. S. Newberry, M.D., of New York; Lyell Medal to Prof. H. Alleyne Nicholson, M.D., D.Sc.; Wollaston Fund to John Horne, F.R.S.E.; Murchison Fund to E. Wilson of the Bristol Museum; Lyell Fund to Arthur H. Foord, and T. Roberts, B.A.

The Academy of Sciences at Turin has awarded the great Bressa prize of 12,000 francs (£480) to M. Pasteur.

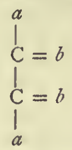
The eighteenth International Congress of Orientalists will meet in Stockholm on September 2 next, and be opened by King Oscar in person, attended by the whole of the Royal family. The Congress will sit till September 6, when the members will visit Christiania as guests of the King, in whose name they will be entertained in the Norwegian capital for two days. They will then proceed to Gothenburg, where the Congress will be dissolved. In honour of the Congress a bibliography is to be issued, containing the portraits in heliography of all living Orientalists, and a *résumé* of the works published by each. The work is to be most sumptuously got up. The editor is Count Carlo Landberg.

The following arrangements have been made for the Penny Science Lectures at the Royal Victoria Hall: February 7, by Dr. Percy Frankland, "Germs in the Air, and what they do for us"; February 14, by E. Wethered, "Earthquakes and Volcanoes"; February 21, by F. R. Cheshire, "Insects as Florists and Fruit-makers"; February 21, by E. Hodder, "Incidents in the Life of Lord Shaftesbury."

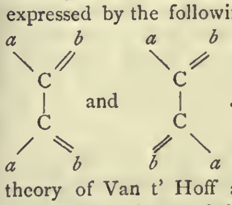
Information has been received of the arrival of H.M. surveying-ship *Egeria*, Capt. P. Aldrich, at King George's Sound, after a very successful deep-sea sounding cruise across the Indian Ocean. Between latitudes 10° and 35° S.,—a belt 1500 miles wide,—not a single sounding has heretofore been obtained in this ocean, and it is therefore satisfactory to learn that forty-three soundings, all of them accompanied with several sets of temperature observations, have been now obtained. The *Egeria's* track was from Sunda Strait to Mauritius, thence south to latitude 38°·5, and thence to Western Australia.

Memorials are being sent from various public bodies in Hampshire to the Lord President of the Council, requesting that the proposed Forestry School for England may be established in that county. It is pointed out that the extensive Crown lands of Hampshire are peculiarly well fitted for scientific and practical forestry.

A paper of exceptional interest was read by Prof. Victor Meyer at the meeting of the Chemical Society of Göttingen held on January 24 In it were embodied some remarkable speculations upon the shape of the ultimate atoms of carbon. These ideas are the outcome of his recent work upon the oxims of benzil and certain other complicated organic compounds, and may be briefly summed up as follows. Certain compounds of the type



(where *a* represents a monad and *b* a dyad radicle) exist in two isomeric modifications which can only be expressed by the following different geometrical arrangement:



. This necessitates an expansion of the theory of Van t' Hoff and Wislicenus, according to which, by rotation of one of the carbon atoms in the first case, the latter would be the only stable form; there are cases in which this rotation is free to occur, and cases like the present where it is prevented. From a consideration of the geometrical isomers of the benzyl cyanides, Prof. Meyer further shows that the valencies of carbon may be displaced out of their normal positions at the corners of a regular tetrahedron by the unequal attractions of unlike radicles. Finally, as the only means of accounting for all these varied phenomena, Prof. Meyer expresses his conviction that the atoms of carbon are spheres, each surrounded by an ether-shell, which forms the seat of the four valencies. On account of their probable electrical connection he terms these valencies "electrules," and considers that the electrules of the same atom are in isochronous oscillation, and

therefore, in accordance with the law, repel each other but are attracted by the dissimilar oscillations of other atoms. In consequence of this repulsion they must take up the regular tetrahedral position, from which, however, under the circumstances mentioned above, they may be slightly displaced. In course of the discussion which ensued, Prof. Riecke followed up the subject with a preliminary notice of his own researches upon the shape of odd-valent atoms, and showed that, in case of nitrogen, phosphorus, and other pentad elements, the ether-shell is probably not spherical, but an ellipsoid; three valencies being situate upon the great circle at the angles of an equilateral triangle, whilst the other two are located at the poles. If this indeed be the case, the tri- and penta-valency of the nitrogen group will be completely and very simply explained.

In a paper contributed to the Royal Society of Edinburgh, and to be published in a forthcoming volume of the Society's *Transactions*, Mr. A. Crichton Mitchell gives the results of an experimental inquiry, made at the Edinburgh University Physical Laboratory, into the thermal conductivity of iron, copper, and German silver. The method employed was that of Forbes. The experiments were, for the most part, a repetition of those made by Prof. Tait about ten years ago; the main difference being that the bars used were nickel-plated in order to prevent oxidation, and thus render the estimation of the amount of surface loss of heat a matter of greater certainty than hitherto. The results arrived at confirm, in the main, those of the previous work, on the same bars, already mentioned, and are of some importance in deciding the question as to whether thermal conductivity increases or decreases with increase of temperature. The principal conclusion in the paper is, that in iron, thermal conductivity increases with increased temperature, and that therefore iron can no longer be looked upon as an exception to the rule followed by the other metals, viz. that their conductivity increases with temperature.

The first rack-railway in France was opened lately at Langres (in Haute-Marne), which is perched on a hill 1460 feet high, and is the principal seat of the French manufacture of fine cutlery. The two railway-stations which have hitherto served it are several kilometres from the town, and much inconvenience has been felt. The new line is made on the type of the Righi railway, and rises to the heart of the town. A company has recently been formed to construct a funicular railway at Hong Kong, for ascent of the hill overlooking the town, and much frequented by the inhabitants. This line will rise to a height of 500 metres, and will be 1600 metres in length. The formation of the ground is exclusively granite, affording a fine solid base. There will be three viaducts, the largest 30 metres in length. Two compound engines of forty horse-power each will be installed at the upper station.

One of the most important questions in optics is whether the velocity of propagation, and therewith the wave-length of light, depend on its intensity or not. A determination of wave-length from prismatic decomposition is not capable of great accuracy. Dr. Ebert, of Erlangen, in studying the subject, preferred a more accurate method based on interference phenomena. Using eight variously coloured light sources (lithium, hydrogen, sodium, &c.), varied in intensity between the values 1 and 250, he established the constancy of the wave-length and velocity of propagation to nearly a millionth within those limits of intensity. Considering, with these results, that the great brightness of the sun does not destroy the coincidences of the Fraunhofer lines with lines of our terrestrial light sources, even with the greatest dispersions, Dr. Ebert thinks his affirmation (of independence of intensity) is generally valid within ordinarily occurring limits of brightness.

Mr. M. W. Harrington has contributed to the *American Meteorological Journal* for December the results of an interesting inquiry as to whether the rainfall is increasing on the plains. The subject is one of importance, as an annual increase of the rainfall would increase the agricultural capacity of a large territory. In order to come to a definite conclusion he has used two long series of observations representing the average conditions at the epochs of 1850 and 1880. The author compares the lines of equal rainfall for the two periods, and shows that, if there had been an increase, any one line should have travelled westwards in the interval between the two epochs. The result of the inquiry shows an apparent advance along the zone included between the parallels of 35° and 45°, and a regression above and below these latitudes, in other words that there has been apparently a consistent increase of rainfall toward the plains.

M. A. Lancaster, Meteorologist at the Royal Observatory of Brussels, and Inspector of Meteorological Stations in Belgium, has published a paper on the climate of that country in 1887, based on the observations at Brussels, and three other stations at the west, north, and east limits of the kingdom, the stations selected being typical of all other points of observation. The weather for each month is discussed at considerable length, and the results are compared with the normal values. The observations show that the mean barometer is highest in December and lowest in October; the absolute maximum temperature during the year at Brussels was 91°·0, and the minimum 15°·4. The prevalent winds are from west-south-west to south-south-west. The rainfall during the year amounted to 24·42 inches, being considerably below the average, viz. 28·78 inches. The rainfall diminishes with the distance from the sea, excepting in the neighbourhood of the forests; the amount increases notably in the Ardennes.

Dr. Fines has published, in the fifteenth *Bulletin Météorologique* of the Department of the Pyrénées-Orientales, the results of experiments carried on at the Perpignan Observatory:—(1) To test the theory of M. Kammermann, of Geneva, for the prediction of spring frosts by comparison of the readings of the wet-bulb and minimum thermometers (explained in the *Archives des Sciences physiques et naturelles*, vol. xiv. p. 425); the result being that the data, so far as Perpignan is concerned, do not bear out the character of accuracy attributed to them elsewhere. (2) To compare the results of wind-velocity given at each instant by a Bourdon's anemometer (presented to the Academy of Sciences, July 30, 1882) with the records of Robinson's velocity anemometer. The result of this inquiry shows a mean increase in the maximum velocities of over 21 per cent. by the use of Bourdon's instrument as compared with the means of the greatest velocities obtained by Robinson's anemometer.

We have received from the Imperial Observatory at Tokio the monthly summaries and means of the observations of the meteorological service in Japan for the year 1886, accompanied by charts showing the tracks of central areas of high and low barometer and by synoptic weather charts for each month. The stations are supplied with good instruments (in some cases with self-recording apparatus), and thee obsrvations which have now been regularly taken for about eleven years under the present organization compare favourably with those of European systems. The climate is generally of the oceanic type—the highest mean temperature at Tokio occurring in August. The winds are governed by the monsoon seasons; during the first three months of the year the prevalent directions at Tokio are north and north-west. From April to June the winds shift round to the south, through east. In July and August southerly winds predominate, but in September a sudden change to the north occurs, and continues generally until December. Warn-

ings of wind and weather are issued to various stations; the general percentage of success for both elements during the year 1886 was 80. Rainfall maps for each month and for the year are also given.

WE have received the *Annuaire* for the year 1888, published by the Bureau des Longitudes with Messrs. Gauthier-Villars, Paris. It contains, besides the tables usually expected in works of this class, much useful information as to the monetary systems of the various nations of the world, mineralogy, meteorology, and other subjects. We may especially note an excellent account, by Admiral Mouchez, of the International Astronomical Congress which met in Paris in April 1887, to prepare the way for the execution of a photographic chart of the heavens.

THE Syndics of the Cambridge University Press have undertaken the publication of a collected edition of the mathematical papers of Prof. Cayley. These papers, originally contributed to the Royal and other Societies and to various mathematical journals, will be arranged for publication by Prof. Cayley himself, who will add notes containing references to the writings of other mathematicians on allied subjects. It is expected that the edition will extend to ten quarto volumes; it is intended to publish two volumes each year until the completion of the work.

"MY TELESCOPE; a Simple Introduction to the Glories of the Heavens," is the title of a little half-crown work on astronomy by "A Quekett Club Man," whose kindred volumes on "The Microscope" have been so successful. It will be issued in a few days by Messrs. Roper and Drowley.

A NEW geological map of the Government of Kutais, by MM. Simonovitch and Sorokin, has been published at Tiflis by the Mining Department.

AT a recent meeting of the Asiatic Society of Japan (reported in the *Japan Weekly Mail* of November 19), Mr. Batchelor read a paper on the *Kamui*, or gods of the Ainos of Yezo. He enumerated under thirteen heads these deities as they appear to be arranged in the Aino mind. These are: (1) the chief of all the deities, the possessor of heaven and the maker of worlds and places; (2) the progenitor of the Aino race, and presider over the affairs of men, who is the only human being worshipped by the people; (3) the sun and moon (the stars are not worshipped); (4) the fire-god, worshipped because of its general usefulness in cooking, healing, purifying, &c.,—sometimes spoken of as the "messenger" or mediator between gods and men; (5) the goddesses who preside over springs, lakes, rivers, and waterfalls,—they are worshipped as benefactors of mankind, particularly in alluring fish to ascend and descend the rivers; (6) the sea-gods, two in number, one being good and one evil,—the latter is the originator of all storms, and the direct cause of shipwrecks and death from drowning at sea; (7) bears, the most powerful animals known to the Ainos, as well as the most useful, supplying them at once with food and clothing; (8) the autumn salmon, the largest fish ascending the rivers,—it is not worshipped, but the term *Kamui* or deity is applied to it; (9) many birds, some of good, others of ill, omen, though not worshipped, are called deities. The same term is applied to beautiful localities, to high mountains, to regions full of bears or rivers full of fish, to large trees, to cool breezes on a warm day, to men of official rank, to devils, evil spirits, and reptiles. When applied to anything good, the term *Kamui* expresses the quality of usefulness, beneficence, divinity; when applied to anything evil, it implies dread, hatefulness, and such like. Applied to animals, it represents the greatest, fiercest, or most useful; to men, it is a mere title of respect. Subsequently, in the course of the discussion, Mr. Batchelor said that the facts of the Aino religion were very simply stated. They had one chief god, and all the others were officers or messengers of this supreme being; there was no lightning- or thunder-god. These were the facts, but he could not explain them. The Ainos, he said, regarded the sun as a body in which a deity resides, "distinguishing, so to speak, between a body and a soul."

THE fossil head of a mammoth has just been unearthed in the Montmartre cemetery in Paris. The distance between the tusks is nearly 2 feet. Further excavations are being made in the hope that the remainder of the skeleton may be discovered.

ABOUT 20 cwt. of bones of prehistoric animals have been found in the bear cave near Rübeland, in the Harz Mountains. Only a part of the cave has yet been explored.

THE University of Upsala has recently been presented with the fossil skeleton of a whale, found in a layer of marl at a depth of 10 feet in the province of Halland, in the south of Sweden. The skeleton, which is almost perfect, is that of a whale which has been called *Eubalæna svedenborgii*, from some portions of a whale skeleton found last century in the province of West Gothia, and now also in the Museum at Upsala. The skeleton is the only one complete ever found. It is that of a young whale.

ON January 8, about 4 p.m., a magnificent meteor was seen at Porsgrund, in the south-east of Norway. It moved rapidly in an easterly direction towards the constellation Taurus. It was square in appearance, but the corners were rounded, the colours being intense green and violet, increasing in strength as the meteor disappeared behind a hill. The size was about that of the full moon. No sound was heard, nor did it leave a train. The passage occupied about two seconds.

THE fog which lately prevailed over our islands and the North Sea extended far into the heart of Norway.

ON December 27, about 11.30 p.m., a severe shock of earthquake was felt at Solum, in the province of Bratsberg, in the south-east of Norway. The shock was so severe that beds seemed lifted from the floor, and the occupants fled in terror into the open. The shock lasted several seconds, and was accompanied by several deep detonations. Large cracks were afterwards seen in the earth. The motion was from east to west.

A SEVERE earthquake occurred at Algiers on January 8. It was noticed throughout the whole province. In one village a house fell in, and the church and the school-house were damaged.

FROM the Consular Report on the Trade of France for 1886–87, it is apparent that the desire for technical education is not at present widespread throughout that country. M. Lockroy, thinking that one of the great causes of the depression in trade, from which France, like England, has been suffering for some years, was the almost total absence of technical education in France, and that the usual remedies were of little or no avail unless aided by a sounder education of tradesmen and merchants, founded a new department in the Ministry of Commerce to supervise the carrying out of his plans. By his help and that of other supporters of his scheme, technical schools have so increased and multiplied, that there are at present ninety of these institutions in Paris and the provinces subsidized by the State. Very few of them are self-supporting, and the number of students in attendance is lamentably small. Students now, it is said, are not anxious to attend, but it is thought that if technical schools received the power of conferring degrees equivalent to the lower degrees in a University, the students would come more readily. It is also urged that these schools should be freed from Governmental control, and be

handed over to the mercantile bodies that in France correspond to our Chambers of Commerce, who understand local needs and local industries better than any department of State. Most of these institutions are behind the age, and the collections at the Conservatoire des Arts et Métiers, Paris, are not so full as those in other countries, and the building itself is in a half-ruinous condition. If the other establishments are inferior to this, as the Report seems to imply, perhaps it is not so difficult to account for the paucity of students and their lack of interest as the Ministry of Commerce seems to think it is.

THE additions to the Zoological Society's Gardens during the past week include two Poë Honey-eaters (*Prosthemadera novæ-zealandiæ*) from New Zealand, presented by Capt. Brabazon J. Barlow, s.s. *Tamui*; a Brazilian Hangnest (*Icterus jamaicai*) from Brazil, presented by Mr. Geo. D. Morce; a White-bellied Sea Eagle (*Haliaetus leucocephalus*) from Newfoundland, presented by Mr. Geo. M. Johnson; three Egyptian Cobras (*Naia haje*), three Cerastes Vipers (*Vipera cerastes*), two Hissing Sand Snakes (*Psammophis sibilans*), a Clifford's Snake (*Zamenis cliffordi*), an Egyptian Eryx (*Eryx jaculus*), a Blunt-nosed Snake (*Dipsas obtusa*) from Egypt, presented by Capt. W. G. Burrows; twenty-one Horrid Rattlesnakes (*Crotalus horridus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

AMERICAN OBSERVATORIES.—The January number of the *Sidereal Messenger* states that the University of California has allotted $19,000 for the current expenses of the Lick Observatory during the present year. The Observatory has received an accession to its staff in Mr. Charles B. Hill, formerly of Chabot Observatory. The equipment of the Observatory has also been furthered by the arrival of the 36-inch photographic corrector and the micrometer for the great telescope. The micrometer is by Fauth and Co.

A new Observatory has been opened in connection with the Syracuse University, New York. This Observatory, erected in memory of Mr. C. D. Holden, a former graduate of the University, was dedicated on November 18, 1887, Prof. Newcomb pronouncing the inaugural address. The new institution possesses a transit instrument by Troughton and Simms, of 3 inches aperture, a chronometer by Dent and Co., a chronograph by Fauth and Co., and an 8-inch equatorial by the Alvan Clarks. Prof. John R. French is the Director.

At the Washburn Observatory, Prof. Brown, the new Director, who was formerly at the Naval Observatory, Washington, is engaged at Prof. Auwers' request in the determination of the fundamental star-places of the Zusatz-sterne in Auwers' system.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 FEBRUARY 5-11.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 5

Sun rises, 7h. 35m.; souths, 12h. 14m. 13·6s.; sets, 16h. 53m.: right asc. on meridian, 21h. 14·6m.; decl. 16° 0′ S. Sidereal Time at Sunset, 1h. 54m.

Moon (between Last Quarter and New) rises, 1h. 20m.; souths, 6h. 24m.; sets, 11h. 19m.: right asc. on meridian, 15h. 23·5m.; decl. 13° 16′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	8 10	13 6	18 2	22 6·3	12 58 S.
Venus.....	5 30	9 32	13 34	18 31·8	21 58 S.
Mars	23 17*	4 41	10 5	13 40·0	7 44 S.
Jupiter....	2 50	7 6	11 22	16 5·6	19 55 S.
Saturn....	15 20	23 15	7 10*	8 17·5	20 14 N.
Uranus...	22 33*	4 5	9 37	13 4·2	6 7 S.
Neptune..	11 1	18 40	2 19*	3 41·6	17 54 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Feb.	h.	
6	1	Jupiter in conjunction with and 4° 2′ south of the Moon.
7	6	Neptune stationary.
8	21	Venus in conjunction with and 1° 24′ south of the Moon

February 11-12.—A partial eclipse of the Sun: not visible in Europe.

Saturn, February 5.—Outer major axis of outer ring = 46″·1; outer minor axis of outer ring = 16″·0; southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Feb. 9,	20 19	*m*
Algol	3 0·9	40 31 N.	,, 10,	1 30	*m*
R Canis Majoris	7 14·5	16 12 S.	,, 5,	22 43	*m*
			,, 7,	1 59	*m*
S Hydræ	8 47·7	3 30 N.	,, 9,		*M*
T Virginis	12 8·9	5 24 S.	,, 7,		*M*
δ Libræ	14 55·0	8 4 S.	,, 8,	2 50	*m*
U Coronæ	15 13·6	32 3 N.	,, 8,	2 26	*m*
V Coronæ	15 45·5	39 55 N.	,, 5,		*m*
U Ophiuchi	17 10·9	1 20 N.	,, 9,	1 29	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·5	27 47 S.	Feb. 9,	5 0	*M*
Z Sagittarii	18 14·8	18 55 S.	,, 6,	0 0	*m*
β Lyræ	18 46·0	33 14 N.	,, 7,	21 0	*m*
			,, 11,	2 0	*M*
U Aquilæ	19 23·3	7 16 S.	,, 11,	5 0	*m*
η Aquilæ	19 46·8	0 43 N.	,, 11,	21 0	*M*
S Sagittæ	19 50·9	16 20 N.	,, 6,	4 0	*m*
			,, 9,	4 0	*M*
Y Cygni	20 47·6	34 14 N.	,, 6,	20 9	*m*
			,, 9,	20 3	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near Capella	74	43 N.	
,, λ Draconis	165	73 N.	
,, θ Draconis	240	62 N.	February 6.

GEOGRAPHICAL NOTES.

AT Monday's meeting of the Royal Geographical Society, Admiral Mayne gave an account of recent explorations in British North Borneo. The paper of most original interest was, however, that of Mr. Maurice Portman, on the exploration and surveys of the Little Andaman. As an official on the Andaman Islands, Mr. Portman made it his business to conciliate the natives of the Little Andaman, who were regarded as quite intractable, and had been severely punished several times for murdering shipwrecked sailors. After a great deal of trouble and much risk, Mr. Portman succeeded in making friends with the natives, with the result that he and those who accompanied him could visit the island with impunity. He has thus been able to collect much welcome information both concerning the island and its highly interesting inhabitants. He completely surveyed the island, and has thus been able to make important corrections on our maps. At the north end the island consists of mangrove swamp and low belts of sandy soil, on which the aborigines have their huts. On the west and south-west the land rises into low hills of a coarse sandstone, running more or less north and south. The timber appears to be much the same as that of the South Andaman, though Mr. Portman saw no padouk and very few bamboos. The rocks are chiefly lime and sandstone, with a good deal of actual coral rock on the east and south coasts. In one place, south of Daogulé Bay, Mr. Portman noticed an outcrop of igneous rock. He found no minerals of importance. This island is about 27 miles long by 15 miles broad, and is encircled by a fringing coral reef. The products of the sea are the same as at the Great Andaman; but the Tubiporine family of coral, particularly *Tubipora musica*, occur in profusion. Dugong and turtle are very plentiful. On the South Sentinel Island, about 12 miles west of the Little Andaman, the turtle appear to have their breeding-station. This island, which is composed entirely of coral rock, is infested by large iguanas, and the *Birgus latro*, or cocoa-nut-stealing crab

(which certainly does not live on cocoa nuts there, as there are none). In rough weather landing is almost impossible on the coast of the Little Andaman, and even in fine weather there are heavy ground-swells and tide-rips. On the north coast large isolated reefs and ledges exist, which make navigation dangerous. With regard to the aborgines of the island, Mr. Portman is of opinion that the whole of the Little Andaman Island is peopled by one race, calling themselves Öngés. These people are subdivided into tribes, who adhere more or less to their own villages, and who quarrel and fight with each other considerably. They appear healthy; their principal diseases being chest complaints, colds, fever, and itch. In physique they compare favourably with the inhabitants of the Great Andaman. Their manners and customs differ somewhat from those of the Great Andaman people, the principal differences being the following:—Instead of small lean-to's, they build large circular huts, some measuring as much as 35 feet in height, and 60 feet in diameter. In these huts the various families sleep on charpoys of wood and cane matting, raised from 6 to 18 inches off the ground, and about 2 feet 6 inches square. Their habits are more cleanly, particularly as regards their huts, and the manner of preparing their food, which is invariably cooked. They cook, dry, and store in baskets, a small fish like a sprat, and this, with the boiled seed of the mangrove, seems to be their principal food, which they supplement with what they can. Their canoes, utensils, ornaments, and bows, are different from those of other Andamanese, and the women wear a tassel of yellow fibre in place of the leaf. They do not smear their bodies over with red ochre, or tattoo themselves, nor do the women keep their heads clean shaved. They are by no means expert in the use of a canoe in rough water, and do not harpoon turtle or dugong, though very fond of the former. They have no religion of any kind, and Mr. Portman learnt nothing of their traditions or superstitions, from which they seem even more free than their neighbours.

Mr. C. M. Woodford has recently returned from a two years' visit to the Solomon Islands, with extensive collections of mammals, birds, reptiles, Lepidoptera, &c. Nearly six months were spent on Guadalcanar, an island the interior of which has never been previously explored. Ascents were made of several rivers, the furthest point reached being about fifteen miles from the coast; but the hostility of the bushmen prevented the ascent of Mount Lammas.

OUR ELECTRICAL COLUMN.

Mr. Willard Case, of Auburn, N.Y., U.S.A., whose extremely interesting paper on a thermic voltaic cell was read before the Royal Society on May 6, 1886, is systematically pursuing his studies to obtain electric energy direct from carbon without passing through the intermediate stage of heat. A paper read on January 10, 1888, in New York, narrates his latest experiments. Jablochkoff tried to do it by immersing plates of carbon and iron in fused nitre. Mr. Case has been using chlorate of potassium and chlorine peroxide (perchloric acid), and with the latter has obtained an E.M.F. with certain forms of carbon varying from 0·3 to 1·24 volt.

In 1869, Dr. Gore proposed a thermo-magnetic generator of electricity (Proc. R.S. 1868–69, p. 261), in which an increasing or decreasing magnetic field was produced by heating and cooling an iron wire placed as a core to a coil of wire. Mr. Edison has recently endeavoured to make this principle practical, but M. Menges, of the Hague, has been more successful. The difficulties to overcome are waste of heat, energy, and consumption of time, in heating and cooling. The results obtained at present are, however, poor, though encouraging.

M. Tereschin, following Quincke's examples and directions, has found with water, methyl and ethylic alcohol, bisulphide of carbon, ether, oil of turpentine, and rape oil, a considerable transport of mass in capillary tubes in the direction of the positive current (*Beiblätter* No. 10, 1887); and Prof. Horace Lamb, in the *Phil. Mag.* for January, prints the admirable paper on the subject which was read before Section A of the British Association at Manchester last September, in which he criticizes the work of Wiedemann and Helmholtz, and explains the phenomenon on the assumption of Quincke, that there is a contact potential difference between the fluid and its solid boundaries, and his own conclusion that there is a sliding coefficient for a fluid in contact with a solid. This transport of mass, due to currents, and the electromotive forces produced by the passage of liquids through capillary tubes, and porous diaphragms are facts undeveloped and unapplied at present.

Considerable attention is being devoted to the heating and fusing of wires by currents. Short lengths of fine wire are used in nearly all electric light equipments as safety valves or cut outs; but the law determining the behaviour of these fuses was little known. Mr. Preece has written two papers for the Royal Society. Profs. Ayrton and Perry introduced the subject in a recent paper read at the Society of Electrical Engineers, and Mr. Cockburn has brought the whole subject before that Society, where it has been well threshed out. For fine wires, viz. those under ·010 in., the fusing current varies with the diameter; but for wires over ·010 in., is given by the equation—

$$C = ad^3/_2.$$

The constant a has been determined for all metals. The behaviour of tin, which is very commonly used, is peculiar. When it approaches the temperature of fusion, its surface oxidizes and coats the wire with a thin skin, which acquires a higher temperature and allows a greater current to flow before fusing. Mr. Cockburn breaks through this skin with a weight—a pellet of lead; while Mr. Preece prevents the skin forming by covering the wire with shell-lac, which acts as a flux and prevents oxidation.

Major King, U.S.A., has recently made a mammoth electro-magnet out of two Rodman guns, weighing about 60 tons. It was excited by a powerful dynamo, and the armature resisted a pull of nearly 10 tons. The field was felt and watches were stopped at very great distances.

Von Bernardo's system of welding by directing an arc itself along the crack, fissure, or edge of the metal to be welded is attracting great attention on the Continent. Prof. Rühlmann, of Chemnitz, has read a very interesting paper before the Electrotechnical Society of Berlin. A carbon rod is the positive and the metal to be fused the negative pole of the arc. The arc acts like a blow-pipe flame. It is eminently adapted to repair cracks and leaks in boilers, heaters, and condensers, to repair tools and generally to cover the ground of soldering and welding.

A novel mode of forming electrolytically deposited copper tubes is attracting considerable attention. The copper is slowly deposited in a thin coating on an iron mandril kept constantly rotating in the bath. As the copper forms it is pressed by an agate burnisher, which compresses the molecular structure into a hard and solid mass of great tensile strength. Such copper has reached a breaking strain of 40 tons on the square inch. The process is due to Mr. W. Elmore.

A curious experiment is mentioned by the *Electrician* (January 27). A disk of soft iron has a spindle put through it so that it can be spun like a top. When at rest or moving slowly the disk is attracted by the poles of a magnet; but when it turns with sufficient velocity it is repelled by the magnet. The reaction of the induced currents in the mass of the metal is greater than the magnetic attraction.

H. F. Weber has cast doubts on the dull red rays being the first luminous rays to appear. He says that the carbon filaments, platinum, gold, and iron give a "gray glow," which is evident at temperatures much below that of dull red, viz. 525° C. Gold gives this gray effect at 417°, iron at 377°, and platinum at 390°.

THE PROPOSED TEACHING UNIVERSITY FOR LONDON.

The following is the text of the petition which has been drawn up by the Association for Promoting a Teaching University for London:—

To the Queen's Most Excellent Majesty in Council.

The Humble Petition of the Association for Promoting a Teaching University for London

Showeth—

1. That the Association for Promoting a Teaching University for London was formed in 1884, and has enrolled up to the present time about 250 members, each of whom was specially invited to join on the ground of eminence, or of experience in matters affecting University teaching in London, or of being

actively engaged in educational or administrative work in one of the institutions in which such teaching is given.

2. That your petitioners have been engaged for the last three years in examining the state and requirements of University education in London, and in conferring with the persons responsible for the teaching and administration of the institutions in which such education is carried on. They have thus been led to the conclusion that there exists in the metropolis and its suburban districts a general and growing demand for the development of University education. They are convinced that this demand cannot be met while higher education in London remains in its present unorganized state, and while the various institutions giving University instruction are deprived of the means of common discussion and concerted action. For the teaching given in these institutions their respective governing bodies are alone responsible, and each of these bodies for the most part acts in educational matters on the advice of its own teachers; but there is no common centre, such as a University would supply, where these governing bodies and their teachers could meet for purposes of conference, and wherein measures for the better organization of teaching could be discussed and settled. It is matter of experience to those who have taken part in the administration of such institutions that they suffer from the want of public recognition and support—a want due, not to defects in their work, but mainly to the anomaly of their position as institutions performing some of the functions without having the *status* of a University.

3. That the severance from the work of teaching of the work of examination for degrees, and the assignment of such examination to the existing University for London as its sole function, has had an injurious effect upon University education in London. The restraint exercised over efficient institutions through examinations held by a body which is neither responsible for their teaching nor in communication with their teachers acts as a fetter upon education, and gives undue consequence to examinations and their results. Examinations so arranged are less efficient than they might be made as a test of real merit, and tend to encourage dissipation of intellectual energy. In the Faculty of Medicine, although a systematic course of study in a recognized school is now required by the existing University of London, the want of due relations between the examining institution on the one hand and the teaching bodies and professional authorities on the other has led to unsatisfactory results.

4. That the evils above mentioned cannot be fully remedied but by the establishment in London of a Teaching University—that is to say, a University which (1) provides for the student in all the subjects included in its Faculties the best attainable teaching with the necessary aids and appliances; (2) requires a regular course of attendance on such teaching as a preliminary to graduation; and (3) secures to the teacher a direct and adequate representation in its councils, and a due share in its administration.

5. That such a University may be formed without trenching upon the province of the existing University of London, the functions of which are entirely different, and without superseding any institution now giving genuine University instruction in the metropolis. A Teaching University for London would incorporate or associate such institutions without injury to their individual life, as the Victoria University has incorporated Colleges in the North of England.

6. That the metropolis, regarded as the seat of a Teaching University, possesses for students in every Faculty, but especially in the Faculties of Laws and Medicine, advantages which cannot be equalled in any other place in the United Kingdom. Such a University, once established, would supply the motive power for various amendments in the University education of London, which are generally admitted to be needful, such as the greater concentration of the teaching of particular subjects in the earlier scientific stages of medical education; the foundation of additional chairs, attached either to particular institutions or to the University, for the further prosecution of special studies; the promotion of new Faculties; the encouragement of general education as a preliminary to the training for all professions; and, finally, such a presentation to the public of the needs of higher education in London as might secure from the corporate or private munificence of the metropolis the endowments necessary to enable it to keep pace with the growth of population and with the progress of learning.

7. That your petitioners, wishing to promote the foundation of a Teaching University in London, have held conferences with representative London teachers of University rank in the Faculties of Arts, Science, and Medicine, and have submitted to them the following statement of the objects to be aimed at in the foundation of such a University:—

(1) The organization of University teaching in and for London, in the form of a Teaching University, with Faculties of Arts, Science, Laws, and Medicine.

(2) The association of University examinations with University teaching, and the direction of both by the same authorities.

(3) The conferring of a substantive voice in the government of the University upon those engaged in the work of University teaching and examination.

(4) Existing institutions in London of University rank not to be abolished or ignored, but to be taken as the basis or component parts of the University, and either partially or completely incorporated, with the minimum of internal change.

(5) An alliance to be established between the University and the professional societies or corporations, the Council of Legal Education as representing the Inns of Court, the Royal College of Physicians of London, and the Royal College of Surgeons of England.

8. That these conferences have resulted in three reports, each embodying a substantial approval of the objects above stated and of the proposals of your petitioners for the purpose of effecting them.

9. That your petitioners have also held conferences with committees of the Senate and Convocation of the existing University of London, and that, simultaneously with the action taken by your petitioners, and, as they believe, in consequence thereof, the questions at issue have, it is understood, been under the consideration of the Senate and Convocation. Their deliberations have resulted in a report, which has been communicated to your petitioners, and recommends various changes in the constitution of the Senate as the governing body of the University, the establishment of Faculties and Boards of Studies, and the introduction on the governing body of representatives of such Faculties.

10. That such proposals differ from the proposals of your petitioners in the following, among other, respects:—

(1) In not requiring, as a preliminary to graduation in the Faculties of Arts and Science, a regular course of instruction in some recognized teaching institution. Such a course of instruction, however, continues to be required by the existing University as a preliminary to graduation in the Faculty of Medicine.

(2) In admitting Colleges not situated within the London district, the effect of this provision being that the suggested Faculties and Boards of Studies could meet but seldom, and thus would not afford to the teaching institutions of the metropolis adequate means of common discussion and concerted action.

(3) In the absence of any sufficient conditions for securing that the associated Colleges shall be doing effective University work.

(4) In not providing for the direct representation upon the governing body of the associated institutions, or of University teachers.

(5) In granting an unduly large representation to the graduates of the University.

11. That it appears to be difficult, if not impossible, for the University of London, consistently with its relations towards institutions situated elsewhere, and towards private students, to accept modifications which would enable it to fulfil the objects above mentioned.

12. That your petitioners have also held conferences with committees of the Councils of University College, London, and King's College, London, respectively, and have submitted to them the above statement of the objects of the Association. The subject having been subsequently brought by the committees before their respective Councils, these Councils adopted resolutions expressing their approval of the objects above stated, and have since determined to petition Your Majesty to the same general effect as is set forth in this petition.

13. That an alliance between the Teaching University above described and the chief professional societies and corporations of the metropolis, such as the Inns of Court, the Royal College of Physicians of London, and the Royal College of Surgeons of England, would be desirable as securing professional interests in

the arrangements for graduation, and in simplifying and re-arranging examinations in the Faculties of Laws and Medicine. Your petitioners have accordingly opened communications with the above-named bodies regarding this subject. They understand, however, that the Royal College of Physicians and the Royal College of Surgeons are disposed to seek conjointly for independent powers of granting degrees in a Faculty of Medicine. Your petitioners deprecate any severance of the machinery for granting degrees in London from academic influences. Many serious defects of University education in London are due to such a severance.

14. That, with a view to avoid multiplication of bodies conferring a diploma or a licence to practise, it is expedient that the possession of the conjoint diploma of the two Royal Colleges above named should be a preliminary condition for obtaining a medical degree in the University, the conferring of such diploma remaining, as at present, the function of the said Royal Colleges.

15. That the objects above set forth would, in the opinion of your petitioners, be most readily accomplished by the issue of a charter to a body of persons suitably constituted to be the governing body of a new University in and for London; such body to consist of the following persons:—

(1) The Chancellor of the University; the first Chancellor to be appointed by Your Majesty, and named in the charter.

(2) Members to be named by Your Majesty in the charter. Vacancies to be filled by the Lord President.

(3) Members chosen by the governing bodies of University College, London; King's College, London; and such other Colleges as may be associated with the University.

(4) Members chosen by the governing bodies of the professional societies and corporations hereinbefore referred to, if associated with the University.

(5) Members chosen by the professors or teaching staff of associated institutions doing University work, and assembled in the Faculties, whether of Arts, Science, Laws, or Medicine, to which they respectively belong, such members to be in number not less than one-third of the whole governing body.

16. That power should be given to the governing body of the new University to accept the application for association with the University of any teaching institution in the metropolis, the conditions of such association to be—(*a*) that the institution is giving instruction of a University character; (*b*) that it has established a complete curriculum, and possesses a sufficient teaching staff in at least one of the recognized Faculties; (*c*) and that it has furnished proofs of its means and appliances for teaching being established on a satisfactory basis.

Your petitioners therefore humbly pray Your Majesty to be pleased to grant a charter to a body of persons appointed as is described in this petition, or to such other person as Your Majesty may be pleased to select, constituting a University in and for London upon the principles and for the purposes hereinbefore stated, and having power to grant its own degrees in the Faculties of Arts, Science, Laws, and Medicine, and that Your Majesty will be pleased to make such orders in the premises as to Your Majesty, in your Royal wisdom and justice, may seem meet.

And your petitioners will ever pray, &c.

Executive Committee of the Association for Promoting a Teaching University for London:—W. Grylls Adams, M.A., F.R.S., J. W. Cunningham, Sec. King's College, J. Curnow, M.D., F.R.C.P., Sir Dyce Duckworth, M.D., F.R.C.P., G. Carey Foster, B.A., F.R.S., M. Berkeley Hill, M.B., F.R.C.S., W. H. H. Hudson, M.A., LL.M., J. Marshall, LL.D., F.R.S. (Chairman), Norman Moore, M.D., F.R.C.P., H. Morley, LL.D., W. M. Ord, M.D., F.R.C.P., F. Pollock, M.A., R. S. Poole, LL.D., W. J. Russell, Ph.D., F.R.S., T. E. Scrutton, M.A., LL.B., Rev. Henry Wace, D.D., G. C. W. Warr, M.A., A. W. Williamson, LL.D., F.R.S., Gerald F. Yeo, M.D., F.R.C.S., Sir George Young, LL.D.; Secretary, F. C. Montague, M.A., 12 New Court, Carey Street, W.C.

THE TOTAL ECLIPSE OF THE MOON, JANUARY 28.

THE weather on the night of January 28 proved decidedly unfavourable for those astronomers in London and the neighbourhood who had prepared to observe the occultations of small stars by the eclipsed moon. The sky, which had been beautifully clear in the morning, but which had become partially clouded towards evening, had cleared again a little before the commencement of the eclipse, thus raising hopes which were destined to be disappointed, for the clouds returned, and, with the exception of two or three short breaks, the moon was enveloped in cloud more or less dense during the entire duration of the total phase. Very full preparations for the observation of the occultations had been made at the Royal Observatory, Greenwich, but only the observers at the four largest telescopes were able to see even one of the predicted phenomena. At the Cambridge Observatory a similar disheartening experience was recorded, and at Mr. Crossley's Observatory, Halifax, it was quite cloudy, but in the west and south of England, and in Ireland at Dublin and Belfast, the conditions for observing were very favourable. On the Continent, at Vienna no observations could be made, the moon being enveloped in thick haze; at Paris and Berlin the sky had been overcast, and there had been a fall of snow before the eclipse, but the latter half of the eclipse was well observed at the former station, and some good results were obtained at the latter during a clear interval about a quarter of an hour after the commencement of totality. At Moscow the eclipse was seen in a very clear sky, and at Madrid it was partially clear.

The following table gives the number of observations obtained at those Observatories from which accounts have been received up to the present time:—

Observatory.	Aperture of telescope, inches	No. of Stars observed. Dis.	Reap.
Royal Observatory, Greenwich	24	3	7
,, ,, ,,	$12\frac{3}{4}$	1	4
,, ,, ,,	6	3	1
,, ,, ,,	$6\frac{1}{2}$	0	3
Col. Tupman's—Harrow	18	3	2
Mr. Penrose's—Wimbledon	6	3	2
Mr. Brodie's—Fernhill, I. of W.	$8\frac{1}{2}$	9	5
Mr. Stothert's—Bath	6	11	13
Cambridge Observatory	12	0	2
Miss Petrie—Bradford	6	1	1
Mr. Backhouse's—Sunderland	—	1	0
Glasgow Observatory	9	6	7
Mr. Heath—Edinburgh	$3\frac{1}{4}$	3	1
Dunsink Observatory	—	17	18

		No. of Stars observed.
Stonyhurst Observatory	8	1
,, ,,	$5\frac{1}{2}$	12
,, ,,	4	4

The 8-inch refractor at Stonyhurst was devoted to spectroscopic observations during the greater part of the eclipse. It had been in the programme to make similar observations at Greenwich had the night proved favourable, and also to take a series of photographs showing the progress of the eclipse. Three photographs were secured, but the clouds prevented all spectroscopic work. Dr. Copeland also at Dun Echt had intended to make spectroscopic observations, but was almost completely thwarted by snow-squalls.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 19.—"On the Secondary Carpals, Metacarpals, and Digital Rays in the Wings of existing Carinate Birds." By W. K. Parker, F.R.S.

In a paper "On the Morphology of Birds," already sent in to the Royal Society, but not yet published, I have described certain additional parts in the wings of Gallinaceous birds.

One of these lies on the radial side of the first metacarpal; the other two are on the ulnar side of the second and third metacarpals.

These parts, which at first caused me considerable surprise, being wholly unexpected by me, are only part of what I have since found in other families.

During the past year I have worked out the development of the skeleton in the Duck tribe ("Anatidæ"), in the Auk tribe ("Alcidæ"), and in the Gull tribe ("Laridæ"), and to some degree in some other families. The subject appears to me to be of great interest, and I have, through various English and American friends, obtained many scores of embryos and young birds, &c., that I may be able to trace these parts in every main group of the class. Normally, both the existing Carinatæ and Ratitæ,

and such extinct forms as have been worked out—*Archæopteryx*, *Hesperornis*, *Ichthyornis*—show that the primary form of the bird's wing is simply *tri-digitate.* In this I agree with Baur, who has helped me greatly in this matter, both by his valuable papers and also by personal discussion with me.

The normal "manus" of a Carinate bird contains two permanently distinct carpals: three carpals that lose their independence by anchylosis with the metacarpals, and three digital rays extending from the three fused metacarpals.

In some birds, *e.g.* the Passerinæ, the *pollex* of the first digit has only one phalanx attached to its short metacarpal, the second only two, and the third only one, phalanx. In others, Plovers, Gulls, Cormorants, &c., an additional or *ungual* phalanx is found on the first and second digit; and in some birds, *e.g. Numenius*, during their embryonic state, a small semi-distinct nucleus is seen on the end of the aborted phalanx of the third digit.

In my as yet unpublished paper I have mentioned a sub-distinct tract of very solid fibro-cartilage, which evidently corresponds with what has been called "præ-pollex" by Kehrer and others.[1]

I am satisfied, now, that this very notable part is the remnant of the skeleton of the *spur*, so remarkably developed in the Palamedidæ, certain Geese, Plovers, and Jacanas.

This part therefore need not interfere with the consideration of the *true secondary digital* parts.

Among the last communications received by me from Dr. Baur, I find in print what I had already learned from him orally.

In some "General Notes" published in the *American Naturalist*, September 1887, p. 839, I find the following paragraph:—"The oldest Ichthyopterygia had few phalanges and not more than five digits; the radius and ulna were longer than broad, and separated by a space. Later, through the adaptation to the water, more phalanges were developed, more digits appeared, mostly by division of the former, or by new formation on the ulnar side. I have never found a new digit developed on the radial side."

These are most important facts, some of which—namely, the bifurcation of the digital rays—I had received some light upon before, both from Dr. Gadow and from Prof. D'Arcy W. Thompson.[2]

I find that the *carpus*, *metacarpus*, and digital rays are all apt to increase in number beyond what is normal.

Long ago I found, in one of the Palamedidæ, *e.g. Chauna chavaria, two ulnar carpals*, apparently an "ulnare" proper, and "centrale." More recently, in the embryo of a more normal Chenomorph—the Falkland Island Goose (*Chloëphaga poliocephala*)—I found the ulnare nearly divided into two segments.

On the other side of the carpus in an embryo Kestrel (*Falco tinnunculus*) and in a young Sparrow-hawk (*Accipiter nisus*), I found a "radiale" in two pieces, the outer of which in the latter was degenerating into the large "*os prominens*" which is found in the tendon of the "tensor patagii" muscle of rapacious birds.

In the embryos of Gulls, Auks, Guillemots, &c., the large "distal carpal" of the index or second digit sends forward a long wedge of cartilage towards an additional metacarpal nucleus. Evidently this is the rudiment of another carpal seeking to be attached to its own intercalary metacarpal.

Further on, on the large second digit, the flat dilated part of the proximal phalanx, on its ulnar side, also, is developed from a distinct tract of true cartilage, but soon loses its independence; it forms the plate on which some of the *primary quills* are fixed.

Still further on, on the ulnar side, near the small well-developed ungual phalanx of the embryo, but later, after hatching, a small oval cartilage appears, and *is ossified independently.*

A similar tract of cartilage is formed on the pollex or first digit, also, but is somewhat smaller than that on the second; it is on the ulnar side and near the ungual phalanx.

In the feeble third digit I only find a rudimentary secondary metacarpal, on the ulnar side; this is very constant throughout the *Carinatæ;* and sometimes, as I have already mentioned, there is a small rudiment of a second phalanx on that digit which, in the Lizard, has *four* phalanges.[3]

[1] "Beiträge zur Kentniss des Carpus und Tarsus der Amphibien, Reptilien, und Saüger," *Berichte der Naturforschenden Gesellschaft zu Freiburg i. B.*, vol. i, 1886 (Heft 4 und Taf. 4).

[2] See his paper on the hind limbs of Ichthyosaurus, &c., *Journ. Anat. Physiol.*, vol. xx. pp. 1–4 (reprint).

[3] The figures of these parts, and also of the rest of the developing skeleton in these birds—Ducks, Auks, Guillemots, &c.—are ready for publication.

In seeking for evidence of the manner in which these high and noble hot-blooded feathered forms arose from among the Archaic Reptilia, I think that something has been gained in what I have stated above.

The skull brings evidence of the same sort during its development, and it is to ancient long-beaked forms, and not to modern short-faced types of Reptilia, that we must look for any near relationship of the Reptiles in the Birds.

In the Guillemot (*Uria troile*) I have satisfied myself that there has been a considerable amount of *secular* shortening of the beak (rostrum and fore-part of mandibles), and if we look at Dr. Marsh's figures of *Hesperornis* and *Ichthyornis* we shall see what long bills these toothed birds possessed.

But there is no part of a developing bird's skeleton that is not rich with suggestive facts of this kind, as I propose to show in due time.

January 26.—"On the Emigration of Amœboid Corpuscles in the Star-fish (*Asterias rubens*)." By Herbert E. Durham, B.A., lately Vintner Exhibitioner, King's College, Cambridge. Communicated by Dr. P. H. Carpenter, F.R.S., F.L.S.

When small particles (*e.g.* Indian ink) are introduced into the body cavity of a specimen of *Asterias rubens*, they are soon ingested by the amœboid corpuscles of the cœlomic fluid; the latter are carried in various directions by the currents set up by the cilia of the cœlomic epithelium. In the dermal branchiæ these granule-laden corpuscles were observed to adhere to the wall of the branchia, and migrate by amœboid movement to the exterior. [Where such migration is proceeding very actively, a perforation filled by a plug of the corpuscles is formed.] Arrived at the exterior, the corpuscles retain an irregular shape for a while, then they become spherical, swell up, and disintegrate.

Besides corpuscles thus laden with foreign granules, corpuscles containing refringent sphærules (sphæruliferous corpuscles, "Plasma-Wanderzellen") were observed in the extruded material: emigration of such corpuscles was also noted to take place in specimens kept in captivity in glass vessels. Hamann's observation that "Plasma-Wanderzellen" occur in the branchiæ of Echinids helps to confirm the view that this a normal process: further observations are necessary to elucidate its significance. [Dr. Hartog's statements as regards the outward current in the water-tube (stone-canal), were confirmed by the presence of corpuscles in the pore-canals of the madreporite.]

With regard to the other point, it seems clear that minute foreign bodies introduced into the system of a star-fish can be, and are, got rid of by scavenging corpuscles.

"Note on the Madreporite of *Cribrella ocellata.*" By the same.

The dogma laid down by Ludwig is that the cavity of the water-vascular system is isolated from other cavities. In a series of sections carried through the madreporite, &c., of *Cribrella ocellata*, it was seen that a few pore-canals of the madreporic plate open directly into the "Schlauchförmiger Kanal"; the ampulla into which the water-tube (stone-canal) dilates being also connected by an opening with the "Schlauchförmiger Kanal": this latter space being enterocœlic in origin, it is interesting to compare the arrangement in Crinoids.

Zoological Society, January 17.—Dr. A. Günther, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of December 1887, and called attention to a small Fox from Afghanistan, presented by Lieut.-Col. Sir O. B. C. St. John, which should probably be referred to the species shortly noticed by Blyth as *Vulpes griffithi.* It was, however, somewhat doubtful whether the species was really distinct from *Vulpes leucopus*, Blyth, the small Desert Fox of Western India.—Mr. Francis Day exhibited and made remarks on some hybrid fishes from Howietoun, and on a British specimen of the Spined Loche.—Mr. Oldfield Thomas read a report on a collection of Mammals obtained by Emin Pasha in Central Africa, and presented by him to the Natural History Museum. The collection contained 115 specimens belonging to 39 species. The great mass of the collection had been obtained in a district called Monbuttu, just within the Congo Basin. A new Flying Squirrel, of small size, was named *Anomalurus pusillus*, and a new Tree-Hyrax, *Dendrohyrax emini*, after its discoverer.—Capt. G. E. Shelley. read a paper on a collection of birds made by Emin Pasha in Equatorial Africa. The series had been formed partly in the Upper Nile district and partly in the Monbuttu country in the

Congo Basin, and contained examples of four species new to science, proposed to be called *Indicator emini*, *Spermospiza ruficapilla*, *Ploceus castanops*, and *Glareola emini*.—Dr. A. Günther, F.R.S., read a report on a collection of Reptiles and Batrachians from Monbuttu, sent by Emin Pasha. The author enumerated seventeen specimens, of which nine were almost generally distributed over the African region; of the remainder, seven were known from various parts of West Africa. One Tree-Snake was described as new, and called, after its discoverer, *Ahætulla emini*.—Mr. Edgar A. Smith, read an account of the Shells collected by Dr. Emin Pasha on the Albert Nyanza, Central Africa. Of the five species of which examples were obtained, three were referred to new species. It was stated that fifteen species of shells were now known from Lake Albert, of which seven were peculiar to it.—Mr. Arthur G. Butler gave an account of the Lepidoptera received from Dr. Emin Pasha. The collection contained examples of 155 species, of which thirteen Butterflies, and two Moths were new to science.—A communication was read from Mr. Charles O. Waterhouse, containing an account of the Coleoptera from Eastern Equatorial Africa received from Emin Pasha. One of the species was new to science, and six of them had previously been received at the British Museum from West Africa only.

Geological Society, January 11.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—On the law that governs the action of flowing streams, by R. D. Oldham.—Supplementary notes on the stratigraphy of the Bagshot Beds of the London Basin, by the Rev. A. Irving. This paper contained the results of field-work during the year 1887. Additional notes on the stratigraphy of the Bracknell and Ascot Hills were given, justifying the reading of the country as shown in Figs. 1 and 2 of the author's last paper (Q.J.G.S., August 1887), the examination of this line of country having been extended as far as Englefield Green. Sections of the beds of the Middle Group as they crop out at Cæsar's Camp, Swinley Park, Ascot, and Sunningdale, were described and correlated with the 76 feet of beds which constitute that group in the Well-section at Wellington College. The stratigraphy of the hills known as Finchampstead Ridges has been worked out from numerous sections on their flanks; and the strata of the Bearwood Hills were correlated directly with them. All along the northern margin a general attenuation of (*a*) the Lower (fluviatile) Sands, and of (*b*) the Middle (green earthy) Sands was shown to occur, and in some places on the northern margin they are found to have entirely thinned away, admitting of distinct overlap at more than one horizon. The second part of the paper dealt with the Highclere district, where the author believes he has established the full succession of the three stages of the Bagshot formation, a section being given across the valley south of Highclere Station, showing the succession of the whole Eocene series (with the *Ostrea bellovacina* bed for its base) as it is developed there. Some important conclusions were drawn as to the Tertiary physiography of the South of England; and the revised tabulation of the Tertiaries put forward by Prof. Prestwich at the Society's last meeting was referred to as supporting some of the main points for which the author has contended. The reading of this paper was followed by a discussion in which the President, Mr. Monckton, Mr. Herries, and Mr. Drew took part.—The red-rock series of the Devon coast section, by the Rev. A. Irving.

Chemical Society, January 19.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—Morindon, by T. E. Thorpe, F.R.S., and W. J. Smith. Morindin, the active colouring-matter of A'l, the root-bark of *Morinda citrifolia*, yields 48·4 per cent. of morindon on hydrolysis. This latter substance is a methylanthracene-derivative of the composition $C_{15}H_{10}O_5$, and differs from all of the eight known compounds of the same formula.—Manganese trioxide, by T. E. Thorpe, F.R.S., and F. J. Hambly. The authors have repeated Franke's experiments (*Journ. für prakt. Chem.*, 1887) on the so-called volatile oxides of manganese, and have been unable to obtain any evidence of the existence of the blue gaseous manganese tetroxide. They find, however, that manganese trioxide exists, and can be formed by the action of a solution of potassium permanganate in sulphuric acid on dry sodium carbonate.—Note on Chatard's process for the estimation of small quantities of manganese, by the same.—Contributions to the theory of the vitriol-chamber process, by G. Lunge. The theory has been recently advanced by Raschig (*Liebig's Annalen*, 241, 161) that the vitriol-chamber process consists in the formation in the first instance from nitrous acid and sulphurous acid of dihydroxylaminesulphonic acid, which, being acted on by nitrous acid, yields sulphuric acid and nitric oxide, the latter being reconverted into nitrous acid. This theory is regarded by the author as untenable on all points, since it completely ignores the existence of nitrosyl sulphate (chamber-crystals), whilst nitric oxide, oxygen in excess, and water do not yield nitrous acid, but nitric acid. In the author's view it is not NO, but N_2O_3 which acts as a carrier of the oxygen in the vitriol-chamber, and the principal reactions are: $2SO_2 + N_2O_3 + O_2 + H_2O = 2SO_2(OH)(ONO)$; $2SO_2(OH)(ONO) + H_2O = 2SO_2(OH)_2 + N_2O_3$. Much NO is present in the front chambers along with N_2O_3; it is formed by a secondary reaction from nitrosyl sulphate, $2SO_2(OH)(ONO) + SO_2 + 2H_2O = 3SOH_2SO_4 + 2NO$, and is principally absorbed by the direct reaction, $4SO_2 + 4NO + 3O_2 + 2H_2O = 4SO_2(OH)(ONO)$; none of it can pass into NO_2 (which does not occur at all in normally working chambers), but some of it may pass into HNO_3, which is at once acted on by SO_2;—$SO + HNO_3 = SO_2(OH)(ONO)$. Thus the normal vitriol-chamber process is not as hitherto understood an alteration of reductions and oxidations, but it is a *condensation* of nitrous acid, or of NO with SO_2 and O_2 to nitrosyl sulphate, and a splitting up of the latter into N_2O_3 and sulphuric acid.

EDINBURGH.

Royal Society, January 6.—Sir W. Thomson, President, in the chair.—Mr. J. T. Bottomley described and exhibited a practical constant-volume air thermometer. This instrument has been designed by Mr. Bottomley so as to be more sensitive, more accurate, and, at the same time, of much greater range than air thermometers hitherto in use. Mr. Bottomley also exhibited some glass globes with internal cavities produced by cooling.—Prof. Tait communicated a paper by Dr. G. Plarr on the roots of $\epsilon^2 = -1$; and a paper by Prof. Burnside on a simplified proof of Maxwell's theorem.—Prof. Tait also read a paper on the Thomson effect in iron.—Dr. Thomas Muir read a paper on vanishing aggregates of determinants. He has obtained the general theorem of which a particular case was discovered lately by Kronecker, and attracted much attention in Germany.—Prof. Crum Brown communicated a paper by Dr. Griffiths on the Malpighian tubes and the "hepatic cells" of the *araneina* and the diverticula of the *asteridea*.

PARIS.

Academy of Sciences, January 23.—M. Janssen, President, in the chair.—Remarks in reference to M. J. Bertrand's recent note on the law of probability of error, by M. F. Tisserand. A solution is given of the problem, "To determine the function $\psi(x_1 - x_2, x_1 - x_3, \ldots . x_1 - x_n)$, where $x_1, x_2, \ldots . x_n$ indicate n arbitrary quantities independent one of the other, in such a way that this function is symmetrical in relation to $x_1, x_2, \ldots . x_n$."—The paper is followed by a communication from M. F. Tisserand on the law of probability as applied to target-firing.—On some notions, principles, and formulas, which come into play in several questions connected with algebraic curves and surfaces, by M. de Jonquières. A rapid summary is given of these principles, &c., some of which have been established by the author himself, some by other mathematicians.—Note on the second volume of the "Annales de l'Observatoire de Bordeaux," by M. M. Lœwy. This volume is largely occupied with the important observations undertaken for the purpose of revising the positions of the stars in Argelander-Oeltzen's catalogue. It contains the precise coordinates of about 3500 stars belonging to the southernmost region of the northern hemisphere.—Contributions to the history of the problematical organisms of old marine basins, by M Stanislas Meunier. The paper deals with the so-called Bilobites, regarded by some palæontologists as mere physical tracings, by others as real organic remains. Several arguments are advanced in favour of the latter opinion, which is regarded as fairly well established, although not yet rigorously demonstrated.—On the rapidity with which the report of fire-arms is propagated, by M. Journée. All the facts here described tend to show that a projectile possessing a greater velocity than that of the report produces, during its passage through the air, a continuous sound analogous to the explosion of gunpowder.—On the mean distances of the planets from the sun, by M. Roger. It is shown that, apart from certain deviations within a defined

limit, these distances form a geometric progression modified by a periodic irregularity. In a future communication the connection will be pointed out between this law and the theoretic views advanced by the author on the formation of the planetary system.—Summary of the solar observations made at Rome during the last quarter of the year 1887, by M. P. Tacchini. The diminution of spots already noted in September was continued during the two ensuing months, so that the mean was even less than in the previous quarter. The protuberances also were less frequent.—On the phases of Jupiter, by Dom E. Siffert. Most of these observations, which were taken at the Observatory of Grignon, are tabulated for the period from March to December, 1885.—On the application of the Cremonian quadratic substitutions to the integration of the differential equation of the first order, by M. Léon Autonne. In this paper the author develops, for the integration of the differential equation of the first order, the method based on the employment of the Cremonian quadratic substitutions, and applies this method to some special cases of a simple and comprehensive character.—Electric solution of algebraic equations, by M. Félix Lucas. It is shown how, by means of electricity, the solution of equations of any degree p, whose real or imaginary coefficients are given numerically, may be reduced to that of equations of degrees lower than p.—Action of vanadic acid on the alkaline fluorides, by M. A. Ditte. The present paper deals with the fluorides of sodium and ammonium, whose composition is shown to be analogous to that of the fluoride of potassium.—Action of hydrochloric acid on cupric chloride, by M. Engel. The results are tabulated of a series of experiments with the hydrochlorate of cupric chloride. In this substance the chloride of copper appears to be in the anhydrous state, all the water being ultimately combined with the hydrochloric acid.—On the alcoholic fermentation of galactose, by M. Em. Bourquelot. From these experiments, undertaken to determine the true character of the action of the yeast of beer on galactose, the author concludes that pure galactose does not ferment in the presence of the yeast at 15° to 16° C., but that it undergoes alcoholic fermentation when glucose, lævulose, or maltose are added.—On two new genera of Epicarides, by MM. A. Giard and J. Bonnier. The specimens here described live in the fresh waters of the Dutch East Indies, and are regarded as the types of two new genera, Probopyrus and Palegyge, whence are respectively derived the genera Bopyrus and Gyge. They are here named *Probopyrus ascendens*, Semper, and *Palegyge borrei*, G. and B.

BERLIN.

Physiological Society, January 13.—Prof. du Bois Reymond, President, in the chair.—Prof. Fritsch described the detent-joint of a Sheat-fish (Siluridæ). In this fish, as found in the Nile, the adjusting and fixing of the dorsal and pectoral fins is provided for by the various shape and arrangement of the surfaces of the joints, which take the form of hooks and detents. The speaker explained the above arrangements by means of drawings and preparations, by means of which it was easily seen that when once the dorsal spine is fixed it will withstand a very considerable force. These spines constitute a protective mechanism against other predatory fishes, and accounts for the numerical development of these fishes in the Nile. The speaker stated his inability to accept Sörensen's view that the detent-joints of these fishes are organs for the production of sound.—Dr. Joseph had studied the minute structure of the axis-cylinder in the nerves of the electric organ of *Torpedo marmorata* treating them with osmic acid and various staining reagents. By making a careful series of transverse sections he has become convinced that not only the medullary sheath, but also the axis cylinder, possesses a fan-like structure, and that the longitudinal fibrils run in the meshes of the radiating fibres, and are the true conducting tissue of the nerve. The diameter of the axis is six or seven times as great as that of the sheath.—Dr. Weyl had subjected silk to a thoroughly chemical examination, and obtained values for its percentage composition, after purification by treatment with caustic soda, which corresponded with those given twenty-five years ago by Cramer; according to these, silk may be taken as belonging to the proteid class of bodies. Raw silk, and to a greater degree that which has been purified by soda, is soluble in fuming hydrochloric acid; if this solution is poured into alcohol, a white cloudiness is produced, which speedily increases in intensity, and on cooling gives rise to a solid white gelatinous mass. The percentage composition of this new substance obtained from silk, and called by the speaker seroin, is, as regards its carbon and hydrogen, the same as that of silk, but it contains less nitrogen. It possessed in all cases the same composition, so that it is undoubtedly a distinctly characterized chemical substance, and is neither pure silk nor some closely related proteid formed by a splitting-off of ammonia. When treated with dilute acids, seroin yields the same products of decomposition as does fibroin—namely, large quantities of leucin and tyrosin, by which it is characterized as being a proteid. Dr. Weyl hoped shortly to resume this investigation in the direction of a general consideration of the proteid group.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Voltaic Electricity: T. P. Treglohan (Longmans).—Practical Physics for Schools and the Junior Students of Colleges, vol. i. Electricity and Magnetism: Stewart and Gee (Macmillan).—Behind the Tides: C. B. Radcliffe (Macmillan).—Pflanzen-Teratologie: M. T. Masters; German by U. Dammer (Leipzig).—Practical Amateur Photography: C. C. Vevers.

CONTENTS.

THURSDAY, FEBRUARY 9, 1888.

MESSRS. GOSCHEN AND HUXLEY ON ENGLISH CULTURE.

WITHIN the last few days two noteworthy utterances on the subject of our national prospects have been made by men whose opinions deserve and command attention. Prof. Huxley has told us, in the *Nineteenth Century*, that though the restraints imposed by civilization have altered the methods by which the struggle for existence is carried on, they have not made it less real or less bitter.

"In a real, though incomplete, degree we have attained the condition of peace which is the main object of social organization; and it may, for argument's sake, be assumed that we desire nothing but that which is in itself innocent and praiseworthy—namely, the enjoyment of the fruits of honest industry. And lo! in spite of ourselves, we are in reality engaged in an internecine struggle for existence with our presumably no less peaceful and well-meaning neighbours. We seek peace, and we do not ensue it."

This application of Darwin's great theory to commercial competition is more than a parable. It is the scientific explanation of causes which have wrecked civilizations in the past and may wreck them in the future.

The struggle must go on while men are impelled by the desire for a greater profusion of what sustains life or makes it happier. It often has been, and often is, carried on by the sword, but important victories may be won, and disastrous defeats sustained, by more peaceful means. The discovery of the passage round the Cape transferred the trade of the East from the Mediterranean to London and Amsterdam, and most merchants in the City affirm that the cutting of the Suez Canal has once more deprived England of the advantage of situation. The commercial success of Switzerland, however, proves that national characteristics are at least as important as geographical position, and it is well from time to time to ask if we are doing all that in us lies to train those who shall follow us to maintain what our predecessors have won.

It is from this point of view that the second of the two utterances we have referred to is specially interesting. Mr. Goschen is at one with Prof. Huxley as to the severity of the struggle in which we are engaged. "Our position in the race of civilized nations," he told the undergraduates at Aberdeen, "is no longer what it was. We had a great start in industries and commerce, and by virtue of that start we attained to a station of unprecedented and long unchallenged supremacy. That supremacy is no longer unchallenged. Others are pressing on our heels. We require greater efforts than formerly to hold our own." Theory and experience agree. The biologist tells us that a state of struggle is the normal condition of man as of all other living beings, and that it must become keener as our numbers augment. The Chancellor of the Exchequer, with his hand on the pulse of English trade, is witness that the strife is growing in severity.

And this is not all. Mr. Goschen is not satisfied that we have as a nation all the qualifications for success. In a powerful address, which evidently expressed a matured conviction, he insisted that Englishmen lack "intellectual interest" in their work. They regard their business as a necessary evil, from which they delight to sever themselves as often and as completely as possible. They are ignorant of the general principles which underlie the conduct of trade, or at least are careless in noting their application to particular instances. It is quite in accord with this that they regard education not so much as an essential to fit a man for the battle of life as an ornament for his leisure hours. And here again Professor and politician are at one. The highest intellectual ideal of our University men, says Mr. Goschen in effect, is, or at all events until very lately was, perfection of literary form. Our public schools have aimed chiefly at turning out scholars who could write Latin verse. Our educational systems, echoes Prof. Huxley, were fashioned "to meet the wants of a bygone condition of society. There is a widespread and, I think, well justified, complaint that [our system of elementary education] has too much to do with books and too little to do with things."

To discuss the whole question thus opened—an indictment of University and Board-school alike—would be impossible in the limits of space at our disposal, but regarding it from the point of view in which our readers, like ourselves, are specially interested, we cannot but note a sad corroboration of Mr. Goschen's words. In no trades could a genuine intellectual interest be more easily excited than in those which involve a knowledge of science, and in none have Englishmen more conspicuously failed. It is needless to recapitulate stories like that of the discovery of the aniline dyes in an English laboratory, and the wholesale appropriation of the trade to which that discovery gave rise by German manufacturers. The fact is patent and obvious to all who have studied the question. Science can only be successfully cultivated by men who take an "intellectual interest" in their work; and in trades which depend upon a knowledge of science, it is the foreigner who achieves success. Where does the fault lie? For the masters and foremen, the colleges which are springing up all over the country may do much. They are, we believe, slowly creating a class of men who have a sound foundation of scientific knowledge, and a genuine interest in scientific progress. But for the rank and file, for the clerk and artisan, it is upon evening classes that Prof. Huxley thinks we must chiefly rely, and here the main difficulty seems to be to secure good teachers for classes in science and technology. They are, says Prof. Huxley, "not to be made by the processes in vogue at ordinary training colleges." "As regards evening science teaching"—we quote from the Report of the Royal Commissioners on Technical Instruction—"there seems to be nowhere in Europe any organization for systematic evening instruction comparable, as regards the number of subjects taught, and the facilities afforded for the establishment of classes, and for the examination of the students' work, with that undertaken by the Science and Art Department in this country, and recently supplemented, in the application of science to special industries, by the City and Guilds of London Institute.

"At the same time it must be borne in mind that in many towns visited by the Commissioners the evening science teaching was conducted by Professors of higher standing than, and of superior scientific attainments to, the ordinary science teachers who conduct courses in some of the largest and most important of the manufacturing centres of this country."

Here, then, appears to be at all events one weak point.

The Science and Art Department has a flexible system, capable of application to the wants of town and country. Students are examined by the thousand every May, but though the standard of attainment is rising, there is a general opinion—which is supported by the statements of the Royal Commissioners—that the instruction given by the teachers falls far short of an ideal which might be and ought to be reached. And yet this matter of good or bad teaching is vital. "It is absolutely essential," says Prof. Huxley, "that the mind [of the teachers of scientific subjects] should be full of knowledge and not of mere learning, and that what he knows should have been learned in the laboratory rather than in the library." "This," according to Mr. Goschen, "is the first test of the value of an educational system, whatever its curriculum may be. Is it intelligent? Is it thorough? Above all, is it rousing? Does it excite intellectual interest in those who come under its influence? Does it develop in them the temper which always asks for a reason and struggles to arrive at a principle?"

Teachers competent to work an educational system which satisfies these requirements must be themselves highly-finished educational products. They must have risen above the vulgar pocket-filling ambition of passing so many students per annum. Risen above it, not in the sense of ignoring it, for in this prosaic world a livelihood must be earned, but in the sense that the mere drudgery of bread-winning is for them lit up with a glow of the enthusiasm of the student who has knowledge to impart which he himself values for its own sake.

We want as science teachers not men who have crammed just enough to enable them to cram their pupils in turn, but men—and we believe there are many, though far too few of them—who have learnt to regard themselves as members of the great scientific army the advance of which is the most remarkable movement of the age.

How are they to be got? They cannot be obtained in the requisite numbers without a systematic search and preparation. It may be, as Prof. Huxley hints, that additional pecuniary inducements must be held out to secure them. This is a question on which the Chancellor of the Exchequer may have an opportunity of giving practical aid to English science and education. Or, if this is Utopian, let us suggest to Mr. Goschen that it would be well if his great influence were used to urge the Government to make the most of the machinery it already possesses.

Prof. Huxley has been for years the Dean of the Science Schools which are the centre of the system of evening teaching which the Royal Commission on Technical Instruction has pronounced to be in many respects the best in Europe. Among the highest rewards given to the successful candidates in the May Examinations are free passes for more or less prolonged courses of study at South Kensington.

Teachers in training attend the classes, and year by year batches of science teachers are brought together to receive special instruction in the subjects they are engaged in teaching. One of the great difficulties to be encountered by a provincial College is the fact that the calls upon the Professors are too multifarious. Students of all classes—would-be engineers, doctors, electricians, and a dozen similar groups—all desire courses of instruction designed to meet their particular wants. It has been rightly decided that this obstacle shall not impede the progress of the State-aided system of evening instruction. A special institution is provided to meet the special requirements of those who are engaged in it. The union of the Normal School of Science with the Royal School of Mines has not interfered with the attainment of this end, while it has secured the advantages which result from the mingling of students who are studying the same subjects with different aims.

The State, then, has recognized the need for trained science teachers, just as it feels the necessity for providing properly-educated officers for the Navy. It is admitted that both classes can best receive the instruction they need at special institutions. The Royal Naval College at Greenwich has been provided for the one, the Normal School of Science for the other.

The school gives evidence of vitality and success. Within the last five years the number of students has doubled. A very considerable amount of original research is done in its laboratories. Now, however, its very efficiency is a danger. It has outgrown the buildings which have been assigned to it. By permission of the Commissioners of the 1851 Exhibition, classes are carried on in what was the Colonial Exhibition. But duty to the interests with which they are primarily charged will, before long, compel them to withdraw this hospitality. Driven from the holes and corners in which it has been compelled to seek refuge, the Central School for the training of teachers of evening science classes may be compelled to reduce its numbers, and to limit its usefulness at the very moment when Mr. Goschen, Prof. Huxley, and all competent educationalists are agreed that one of our most pressing national wants is the elevation of our teachers, and of their type of teaching.

We have chosen this as a single example which serves to illustrate the wide generalization which we have been discussing. Is the interest of the average Member of Parliament in the dangers which threaten our trade sufficiently intellectual to lead him to sanction the cost of necessary precautions? In these democratic days the fate of the English people is in their own hands. If they choose that the education of their bread-winners shall be conducted on the principles on which the "accomplishments" were taught in an old-fashioned ladies' school—if they choose to send competent Commissioners all over Europe, and, when they tell them that one of the chief defects of their educational system is the comparative inefficiency of their teachers, they nevertheless deliberately half-close the doors of the school specially provided to remedy this defect—there is no help for it, and but little hope for them.

Wars may be caused by race hatreds which have taken centuries to gather, but success or failure often depends on the placing or misplacing of a few thousand men. Commercial competition may be, as Prof. Huxley tells us, due to causes which affect all living things. The progress or decadence of England will depend upon how it adjusts itself to the altering character of the strife; and we confess that we shall watch with interest to see what amount of practical support the Chancellor of the Exchequer is prepared to give to the views of the Lord Rector of Aberdeen. The test will be applied when the Technical Education Bill is again brought forward, and when the particular need which we have chosen as an illustration has to be met.

THE PROPOSED TEACHING UNIVERSITY FOR LONDON.

WE printed last week the petition which has been presented to the Privy Council by the Association for Promoting a Teaching University in London. We have now before us the petition of University College and King's College, to which is appended a proposed draft charter for the University, under the name of the Albert University of London. If, as seems probable, the promoters have been well advised in claiming no less a surname than that of the Metropolitan district for which the University is to serve, the prefixed name of the late Prince Consort, to whom England is undoubtedly indebted for the encouragement his influence gave to educational and scientific work, is perhaps as good a way as could have been hit upon for avoiding confusion with the existing University. For the rest, the charter appears to be an adaptation to the circumstances of that granted to the Victoria University; the principal differences being—the place reserved for the Royal College of Physicians and Royal College of Surgeons in the University, which is one of complete equality with the governing bodies of the University Colleges themselves; the power conferred upon the legally recognized medical schools of London, as such, to claim, as of right, admittance to the University, on equal terms with the Medical Faculties of University College and King's College; and the greater simplicity of the governing body. In the case of the Victoria University a complicated division of authority was resorted to, with the view of obviating mutual jealousies between the various cities and towns in which, in that case, the several Colleges were to be situated. The Senate proposed for the Albert University consists of three members chosen by the governing body of each College associated with the University; the College of Physicians and College of Surgeons being reckoned among associated Colleges, if willing to accept the position, but the twelve medical schools not being so reckoned; of four members representing the assembly of each Faculty, such assemblies being composed of the teaching staffs of all Colleges or medical schools admitted in respect of the Faculty; and of six representatives nominated by the Crown in the first instance, of whom three are eventually to be replaced by representatives of the graduates in Convocation.

Compared with this body, the composition of the Senate of Physicians and Surgeons proposed in the petition of the two Royal Colleges presents an even greater degree of simplicity. No provision is made for the representation of any other interest than that of the petitioners themselves; and the two Colleges divide the representation equally between their respective governing bodies. It is understood, however, that in the case of the College of Surgeons this proposal has not given satisfaction, even within the limits of the College; and that some representation will be claimed for Fellows of the College other than those who constitute the Council which governs it.

From the point of view which is especially our own, the quarrel about degrees, and the interests of rival institutions, occupy a place secondary in importance to considerations affecting the promotion of knowledge and science, and only important in so far as they are concerned. If the proposal of the Royal Colleges is carried into effect, and a committee of eminent physicians and surgeons is intrusted with the power of examining for and giving medical degrees, there can be little doubt that the great building on the Thames Embankment, and the space behind it shortly to be covered with building, will speedily develop into a teaching institution, with provision for research; and thus knowledge will be increased, and science promoted, by the addition of one more to the number of efficient schools for special purposes which are now open in London. The promoters of the Albert University do not offer us any immediate addition of this nature to the resources which are now available. We have examined the draft charter with care, in order to detect, if possible, the traces of a design to check the foundation or perfecting of new institutions, in the interest of those already existing. But the promoters, we are bound to say, appear to have guarded against all objection, by following, in this respect, the charter of the Victoria University. The appeal which is given to the Privy Council, in case of the refusal of the University to admit a new College, is a satisfactory provision against the spirit of monopoly. In the absence of danger from this point of view, the Teaching University promises more than is offered by the Senate of Physicians and Surgeons, in the interests of science. The prospect of establishing, as a qualification for admission to the University, a general standard of efficiency for Colleges professing to do the work of scientific teaching, has greater attractions for us than that of the opening of a single new school of medical and surgical research. Moreover, by the institution of the Teaching University, we shall secure the first, without rendering it less probable that in time the second also may follow.

We notice that the right of admission offered to all the London medical schools, though absolute so far as the Medical Faculty is concerned, is, in regard to the Faculty of Science, made conditional on efficiency. This is as it should be. Probably some of the smaller hospitals will regard with equaminity the extinction of their pretensions to be recognized as efficient schools of science. Others will be incited to render themselves efficient. In both cases the result to science will be a pure gain. One matter of importance appears omitted in the programme of the Albert University: the position to be assigned within the University, if its admission is contemplated, to the Royal School of Mines and Normal College of Science. It would appear proper that this point should be further considered, if the project ever reaches a more definite stage.

Upon the matters in dispute between the University of London and the University Colleges we desire to maintain an attitude of impartiality. On the one hand, it is urged that the credit of a degree will not stand the strain consequent on the creation of a second degree-giving body in London; and that all the reform desirable, in the interests of education, is the introduction of a larger number of teachers on the governing body of the existing University. On the other, stress is laid upon the importance, for educational purposes, of the independence of teachers from irresponsible external control, and upon the necessity of an organization of teaching for London more thorough than can be afforded by any constitution or reconstitution of an examining body. But whether

the desired reforms are carried out by changing the constitution of the University of London, or by instituting a new University, two things appear in any case to be incontestable: that the open examinations conducted by the existing University shall continue to be conducted by an impartial authority; and that the Colleges shall be allowed to organize their work in the manner best suited to promote their own efficiency.

Sir Philip Magnus, in a letter which appeared in the *Times* on Thursday, appears to consider the dispute as one between the efficiency of "lectures" on the one hand, and of "reading" on the other; and he cites the now well-known dictum of Darwin, in favour of reading, and against lectures. But it would be to mis-read, in a strange manner, the lesson of Darwin's life, if from it were to be drawn a conclusion against the existence of Universities for teaching purposes, and in favour of examinations. If Darwin carried from Edinburgh a profound dislike to unintelligent lecturing, of the epideictic sort, he was at Cambridge known as "the man who walks with Henslow." In Sir W. Hamilton's famous analysis of the work of Universities, examination holds the first place only among no less than seven "exercises" by which study, in a teaching institution, can be promoted; the others being "disputation, repetition, written composition, the practice of teaching, conversation with and interrogation of the learned, and social study." To these must be added, by the student of science, the practice of experiment under competent supervision. Some of these appear to us of more value than examinations, some of less; but it is obvious that an institution which is solely concerned with examinations does not cover the whole ground of institutional aid to study; and it is of no avail, as between one institution and another, to exalt the benefits of "reading," which is not peculiar to either. In conclusion, we trust all parties to the controversy will bear constantly in mind that degrees and examinations, lectures and colleges, are, after all, but means to an end. The end is the spread and advancement of knowledge, through educational methods and research.

MANUAL OF BRITISH DISCOMYCETES.

A Manual of the British Discomycetes. By William Phillips, F.L.S. 8vo, 446 pages, 12 plates. International Scientific Series. (London: Kegan Paul, Trench, and Co., 1887.)

IT is by no means an uncommon misfortune to find that text-books are not written by persons the most competent, or with the widest experience; hence the results are very far from satisfactory, and no one expresses much gratification. Now and then notable exceptions to this rather general rule may be discovered, to the delight of all who are interested in that special branch of science to which the book is devoted, and the great edification of the student. It is beyond our province to inquire why the most suitable men are so seldom engaged in the production of "manuals," or why the most skilful manipulator, with a few months' study and much "coaching," cannot compete successfully with the practical hand well steadied with a twenty years' experience. It will be enough to intimate that no one acquainted with British Cryptogamic botany would for a moment hesitate to pronounce that the most suitable person to undertake a manual of the Discomycetes would be Mr. W. Phillips, of Shrewsbury, if practical knowledge, and persistent investigation, extending over at least two decades of years, are to be accepted as qualifications.

With these preliminary observations it will be at once evident that, in general terms, and as a whole, we feel bound to give this little volume our heartiest commendation; and if, in the course of our remarks, we indicate any weak places, it will be with the desire to act with the tenderness of a friend, and to point out how, in our conception, an admirable manual may be rendered more perfect or more useful.

No apology is needed for restricting a book like the present to a small controllable group of some 600 species, especially when the limits are so well defined that a student may devote himself exclusively to it, with advantage to himself, without any special acquaintance with outside groups. It is generally admitted that the entire British Fungi, with its thousands of described species, is become too unwieldy and extensive for any ordinary individual, not content to become a slave to his subject and a martyr to science. The Discomycetes present an admirable group, capable of isolated study; and for this purpose a careful and trustworthy manual, at a moderate price, is now ready at the student's hands.

"The subject of classification," the author says in his preface, "will not fail to awaken some controversy." "To adhere as closely as possible to the long-accepted Friesian system has been the practice of English authors, but this has been carried a little too far, owing to our 'insular prejudices,' and the time has come when a new departure must be made." We are prepared to accept this paragraph—exclusive of "insular prejudices," which we cannot admit—and with it the "new departure." To our mind this is a most moderate concession, and we doubt not that, if controversy there should be, its direction will be in favour of far greater innovation than Mr. Phillips or ourselves would approve. The details of the new arrangement must be subjected to closer examination and the test of experience, but at present we see no reason to take exception to them. We have long been of opinion that some such modification of the old classification was desirable.

There is, nevertheless, one point on which we have always uttered a protest, and repeat it again, since in two or three instances in the present volume the error has been committed. We allude to the addition to, or alteration of, a generic description, and the appending of the original author's name, with the word "amended" after it. We protest against *amended genera*, because they are nobody's genera; they are not the genera of the original author, but a "thing of shreds and patches." A genus should not be altered or amended, in order to fit any subsequent species which a later author may desire to incorporate. He should keep the new species outside, and accommodate it in other ways, rather than modify or "tinker" the work of a predecessor, and assume the change to be an "amendment," whereas it *may* be something very different, and probably *would* be to the old author himself, if he could be resuscitated to gaze on the freaks of his successors.

As for the number of species described in this volume,

we may remark that it is more than double that which the "Hand-book of British Fungi" included in 1871. One of the many valuable features of the book is, that, wherever possible, measurements are given of the sporidia of the various species, in micromillimetres, in addition to the dimensions of the fungus in its entirety. We specially allude to this feature in order to have an opportunity of adding that in our experience we have never met with a more careful or expert hand at microscopical measurement than the author of the present work, an opinion based on hundreds of observations made in concert during a series of years. Yet we must urge that, however useful the micromillimetre undoubtedly is in spore-measurement, it is not so well to use it for larger bodies, such as the cup of a *Peziza*, when the millimetre or its decimal part would appeal more directly to the eye and experience. 500 μ may be equal to half a millimetre, but the mind more quickly and readily conceives the half millimetre than the 500 μ. We observe a lack of uniformity in dimensions appreciable by the naked eye, which is avoided in measurements under the microscope. For instance "¼ to ½ line broad" (p. 249), "cups 200 to 500 μ" (p. 257), "cups 500 to 800 μ" (p. 321). What relation does the "line" bear to the micromillimetre? If half a line is about 500 μ, why use the two units of measurement? Would it not have been better to follow Stevenson in his "British Fungi," and to reduce all measurements to the centimetre, millimetre, and micromillimetre, which would have been much more consistent, and far better than the mysterious "line," and had the merit of being more intelligible to the foreigner than a unit of which he has no knowledge or experience.

It would be useless to assume that the work is absolutely free from errors, but these are mostly of a trivial character, although more numerous than we could have wished. We doubt whether "conidia" would not have been a better term than "spermatia" in such a connection as *Calloria fusarioides;* and we also doubt whether our author accurately appreciates the value of the terminations in such words as *violascens*, *virescens*, *fuscescens*, *nigrescens*, &c.

As for the general scope of the work, we may say that each species begins with the diagnosis, then follows its synonymy, especially in British works, references to figures, and published specimens. If these are in the main accurate, as we have no reason to doubt, they will be exceedingly valuable, but manifestly only experience can prove this, and figures are very liable to become displaced or transposed. The habitat succeeds the synonymy, which is followed by special notes or comments; then the derivation of the specific name, now and then hardly successfully interpreted, as for instance on pp. 291, 325, and 369, where *ater* would have been better rendered "dark" instead of "black"; and finally a list of localities.

At page 358, *Ephelis* is inserted as a genus of Fries's. The same genus is claimed by Saccardo ("Sylloge," iii. p. 691) for a genus of Sphæropsideæ, and we fear that Phillips will have to give way to Saccardo, as both cannot stand, and there is no evidence that Fries regarded his genus as ascigerous.

Forty pages at the end are most useful appendages to the work, consisting of a glossary of terms, full titles of the various works quoted, and an exhaustive index. To the last page Mr. Phillips has spared no trouble to make his work as complete and useful as practicable, and we trust that he may be rewarded for his labour of love (for such it undoubtedly has been) by being called upon speedily to correct the verbal errors in preparation for a new edition.

M. C. C.

OUR BOOK SHELF.

Physiography: an Elementary Text-book. By W. Mawer, F.G.S. (London: Marshall and Co., 1888.)

THIS is another addition to the steadily increasing number of text-books adapted to the elementary stage of physiography. The usual plan of dividing a book into chapters is not adhered to, but probably the author is of opinion that he is working according to the true spirit of physiography in drawing no hard and fast lines.

In the majority of cases the author has succeeded in his endeavours to explain everything in the simplest way, but in a few cases his anxiety to do so has led him astray. The following may be quoted as examples, and the obvious shortcomings need no further comment:—

"Work is the moving of matter" (p. 8); "Energy, when active—when actually doing work—is in the condition called *kinetic*; when it is passive and only ready to do work, it is *potential*" (p. 9).

With a few exceptions of this kind, the book is admirably adapted to the syllabus which it is intended to cover. That it is not a mere cram-book is evidenced by the mass of useful information which is given. A good general outline of the nebular hypothesis is presented, in so far as it concerns the history of our globe, and there is also an outline of the classification of animals and plants. The astronomical portion of the syllabus also receives a fair share of attention. One omission, however, has been made, and that is the use and meaning of the term "stress": the word apparently does not occur even once in the whole book; this is rather unfortunate now that modern physicists are beginning to regard gravitation, magnetism, &c., as stresses.

Apart from its use as a class-book, it can be recommended to the general reader as an outline of science.

A. F.

Early Christian Art in Ireland. By Margaret Stokes. (Published for the Committee of Council on Education, by Chapman and Hall, 1887.)

THIS is one of the South Kensington Museum Art Handbooks, and it deserves to rank among the best of the series. The Christian antiquities of Ireland are in their own way as remarkable as any group of antiquities in the world, and a satisfactory account of them, such as ordinary readers might understand and appreciate, was greatly needed. In undertaking to supply what was wanted, Miss Stokes devoted herself to a task for which she was well equipped by previous study, and she has produced a little book which can hardly fail to excite interest in her subject, and which will be welcomed even by antiquaries to whom the facts of Irish archæology are already well known. A chapter on illumination is followed by one on Irish scribes on the Continent; and then come chapters on metal-work, sculpture, building and architecture, with a chronological table of examples of Irish art the date of which can be approximately fixed. The work is illustrated by upwards of a hundred good woodcuts. In her treatment of all questions relating to early Christian art in Ireland, Miss Stokes displays a thoroughly scientific spirit, and her style has the merit of being always clear, fresh, and unpretending. She rightly claims for her subject that it has a practical as well as an intellectual

interest. If the higher class of workers in Ireland took the trouble to study systematically the objects here so carefully described, an epoch might be marked in the development of Irish technical skill.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

The Duke of Argyll's Charges against Men of Science.

The Duke of Argyll's singular appetite for besmirching the characters of men of science appears to grow by what it feeds on; and, as fast as old misrepresentations are refuted, new ones are evolved out of the inexhaustible inaccuracy of his Grace's imagination.

In the last two letters which the Duke of Argyll has addressed to you, he accuses me of having charged the members of the French Institute with having entered into a "conspiracy of silence" in respect of Mr. Darwin's views. I desire to say that the assertion that I have done anything of the kind is untrue and devoid of foundation.

My words, in the passage of which the Duke has cited as much as suited his purpose, stand as follows: "In France, the influence of Elie de Beaumont and of Flourens—the former of whom is said to have 'damned himself to everlasting fame' by inventing the nickname of 'la science moussante' for evolutionism—to say nothing of the ill-will of other powerful members of the Institute, *produced, for a long time, the effect of a conspiracy of silence.*"[1] I used the words I have italicized advisedly, for the purpose of indicating that, though the members of the Institute did *not* enter into a conspiracy of silence, the notorious antagonism of some of them to evolution produced much the same result as if they had done so.

If the Duke of Argyll were properly informed upon the topics about which he ventures to speak so rashly, he would know that M. Flourens wrote a book in vehement denunciation of evolutionism. As I reviewed that book not very long after its appearance, I could not well be ignorant of its existence. And being aware of its existence, I could not possibly have charged M. Flourens with taking any part in a "conspiracy of silence."

The "effect" of the known repugnance to Mr. Darwin's views of some of the most prominent members of the Institute, to which I refer, is the effect upon the younger generation of French naturalists. Considering the influence of the Institute upon scientific appointments, the chances of a candidate known to be an evolutionist would have been small indeed; and prudence dictated silence.

Mr. Carlyle has celebrated the courage, if not the discretion, of a certain "Rex Sigismundus," who, his Latin being called in question, declared that he was, as a Royal personage, "supra grammaticam." The Duke of Argyll appears to be of King Sigismund's opinion in respect of the obligations which are felt by humbler persons, who have, wittingly or unwittingly, accused their fellows wrongfully; and I do not suppose that he will descend, on my account, from a position which may be sublime or may be ridiculous, according to one's point of view. The readers of Nature will choose their own.

T. H. Huxley.

Bournemouth, February 4.

[1] "Life and Letters of Charles Darwin," vol. ii. pp. 185-86.

An Explanation.

Since the Duke of Argyll's references to myself have been interpreted in a manner likely to convey an erroneous impression to the readers of Nature, it seems to me to be now necessary to give some explanation of the facts in which I am concerned. I intend, however, to go no further than to establish the position his Grace has taken up as regards myself. Such a step, savouring somewhat of presumption on my part, would not have been taken if Prof. Judd had admitted that, although no paper of mine was ever before the Council of the Geological Society, an offer to present such a paper was, doubtless for sufficient reasons, at once declined.

In the spring of 1885, by the advice of Mr. Murray, who had been for some time engaged in examining my recent geological collections from the Solomon Islands, I offered to Prof. Judd, then Secretary of the Geological Society, to present my observations on the upraised coral-reef formations in the form of a paper, in which, as I stated, Mr. Darwin's theory of coral reefs would be brought under consideration. This offer being declined, my observations were taken up by Mr. Murray and were published in the Transactions of the Royal Society of Edinburgh for 1885. As I saw too plainly that the new view of the origin of coral reefs was very far from being generally accepted, I deemed it advisable in preparing my paper to draw no inferences and to allow the facts to speak for themselves. However, six months after the reading of the paper, whilst going over the proofs, having been assured that the theory of Mr. Darwin was rapidly losing ground, I appended some remarks in which I gave the general bearing of my discoveries.

Had I harboured a desire in my mind to record any disappointment in connection with the appreciation of my work, I might have done so in the preface of my small geological volume recently published. The reflection that I had succeeded, and that Mr. Murray's views, as I was told, were being generally received, gave me ample grounds for satisfaction; and there was therefore no reason why I should refer to any difficulties of a personal character. I must confess, however, I was afterwards deeply disappointed on finding that, although the nature of my discoveries was first announced in the columns of this journal in January 1884, whilst the observations themselves had been nearly two years before the world, my name and work were *studiously ignored* in the recent controversy by those who spoke on behalf of English men of science, and particularly on behalf of the Geological Society. Naturally it was there that I looked most for approval. I soon perceived, however, that it could not be in the want of publicity that the reason lay, nor even in the insufficient lapse of time since the publication of my papers. Long abstracts were given in the columns of this journal of the principal paper (Trans. Ed. Roy. Soc., 1885), and of a paper also read before the Royal Society of Edinburgh (Proc., 1886). At the beginning of 1885 (or perhaps earlier) I sent to Prof. Judd a blue pamphlet published in New Zealand, in which I briefly described the discoveries I had made up to the end of 1883. At the beginning of 1886 I sent to him my principal Edinburgh paper of the previous year.

It then occurred to me that since Prof. Dana's last paper, of September 1885, was the chief rallying point of the opponents of Mr. Murray's views, the cue in estimating the value of my work might have been thence derived. I found, however, that Prof. Dana had only before him, when referring to my discoveries, an extract from a private letter of mine to Mr. Murray written in the midst of my work, and published in Nature in January 1884. Rightly enough, he did not consider such a brief account as at all conclusive. My published observations had yet to come before him. It was not, therefore, from the other side of the Atlantic that in estimating the value of my observations Mr. Murray's opponents had taken their cue.

I was forced, therefore, to the conclusion that the reason lay rather in the competency than in the bearing of my observations. I could find no other explanation of the fact that in the succession of replies to the Duke of Argyll's article, entitled "A Great Lesson," no reference whatever was made to the recent important evidence I had adduced—evidence of which at least one of the writers had been previously aware during a period of two if not three years. Under these circumstances, I accepted the decision which the lapse of nearly three years had not affected; and, having naturally some degree of sensitiveness, I withdrew from the Geological Society.[1]

[1] Mr. Guppy was induced afterwards to withdraw his resignation.—Ed. Nature.

In conclusion, I may say that these circumstances do not alter my conviction of fighting on the winning side. The reasons of my faith I hope to give in the next journal of the Royal Scottish Geographical Society. H. B. GUPPY.

17 Woodlane, Falmouth.

Snow Crystals.

YESTERDAY was very favourable for observing the beautiful appearance of sunlight reflected from snow crystals. As one walked across a field, stars appeared to start forth by thousands from amongst the fresh-fallen snow. They were particularly bright and numerous when one walked in the direction of the sun. They appeared almost at all distances, and almost of all sizes, those near at hand being never very large but of great brilliancy and most exquisite colour. The phenomenon was sufficiently striking to induce me to stop and observe it more closely, and the first thing I noticed on stopping was the permanence of each little star of light, although the snow was dropping melted from the hedges and trees by the heat of the sun. A slight movement of the head was sufficient to change the colour of a red star to green or *vice versâ*. It seemed as if the most brilliant colours were seen when looking in a direction nearly but not quite towards the sun. The level surface of the snow appeared as if strewn with gems—and not only near at hand, for even twenty and thirty yards away a large star would shine forth with a subdued but fine colour. I then noticed a peculiar uniformity of shape in these reflections from snow crystals. The shape never varied from that of a blunt arrow-head. This was very striking in the large stars which appeared at a distance; but once noticed, it was obvious enough that even the minute specks at one's feet were all of this form. Nor did the position of the snow vary to any appreciable extent. The inclination seemed always a little to the right, and this occurred no matter in what direction I looked, whether towards the sun, or away from it, or in any other direction. Wishing to know the absolute size of the larger snow crystals, or combination of crystals, I looked for a fine appearance, and estimated as well as I could its magnitude by covering it with a small object held at arm's length. The distance of the spot where the crystal appeared proved to be forty-three paces from where I stood, and its magnitude could not have been much less than three inches in this particular case. Now if, as I suspect, the form of the star which appears so persistently is due to the upper or lower stem only of a complete arrangement of crystals in an hexagonal shape, these combinations must occasionally be six inches or more in diameter. I did not succeed in recognizing any larger than very minute arrangements of crystals in the snow itself, but it is obvious that the sun's rays reflected from a long distance must single out those faces of crystals which happen to be parallel to one another over a certain limited area. Observation of these reflections, therefore, calls in to our aid a power of analysis in the sun's rays to detect symmetrical arrangements of snow crystals quite unrecognizable by mere inspection. Might I ask for some explanation of the phenomena?

Hull, January 30. A. N. S.

"The Mammoth and the Flood."

MR. HOWORTH's letter does little more than travel again over old ground, and two only of the points raised require any notice on my part; the third—the value to be attributed to the opinion of any particular geologist—being immaterial to the main question. As another President of the Geological Society has said: "Science needs no infallible Church, and admits of no Pope."

In regard to the localities in which mammoth remains have been found, I have not "resuscitated" any theory, but have taken my facts from Mr. Howorth's book. His second letter appears to me to ignore a distinction which I was careful to draw in my reply to his former one. That mammoth *bones* should be found at considerable distances from, and elevations above, the existing rivers, offers no difficulty. Indeed, they could not occur, except accidentally, in deltas which are now in course of formation. But, so far as I can ascertain, there is no reason why these "beds of clay and gravel" should not be deposits of rivers which drained the same regions under different climatal conditions, in the distant ages when the mammoth lived in Siberia. The case is precisely similar in England. We should not expect to find mammoth bones in the mud-flats about the mouths of our southern rivers, but in the old valley gravels which occur sometimes even 90 or 100 feet above the present level of the rivers. But the facts most difficult to explain are the occurrences of the *carcasses* of mammoths. It was of these, and of these only, that I was speaking in my letter, as I think would be clear to most readers. No geologist, so far as I know, would deny that the Siberian climate has considerably changed since the mammoth wandered over its tundras, and very likely not seldom got bogged; but the question is, Has it changed suddenly or gradually? The occurrence of the frozen carcass is undoubtedly most simply explained by postulating a sudden change; but when we begin to consider what this means, the remedy, though apparently so simple, seems as heroic as that of the father "who cut off his little boy's head to cure him of squinting." It is then for the best preserved of these frozen carcasses that I suggest the possibility of a drifting and a gradual entombment by the deposits of the ancient rivers. I have again consulted Mr. Howorth's book, and find, between pp. 82 and 89, notices of the discovery of at least ten mammoth carcasses, mostly occurring very far north in Siberia, and nearly all mentioned in connection with rivers: of one it is even said, "like most of the others, it is found on the bank of the river, which had been undermined by floods."

Mr. Howorth further asserts that I cannot have read his book because I charge him with invoking a series of catastrophes when he argues "in favour of one catastrophe only." But, notwithstanding his disclaimer, I would like to know how we are to bring about a deluge to drown the mammoth and a sudden permanent fall in temperature to freeze his carcass (query, one catastrophe, or two?) without "a series of catastrophes." I presume that, as this is a scientific question, we must not invoke a miracle. If continents gambolled like whales—which would be needed for Mr. Howorth's far-reaching flood—would this, unless there were a very special arrangement of continents, so materially alter the climate? and, if they did so disport themselves, what set them dancing? If a number of insular volcanoes exploded with twenty-Krakatão force, this would be a series of catastrophes, but it would probably leave the climate unchanged. If the earth's axis of rotation were suddenly altered materially in position—perhaps the simplest mode of bringing about the two results—would no catastrophic changes be needed to effect this alteration? Mr. Howorth's retort, in fact, indicates better than anything which I can write how completely he has failed to realize the conditions of the problem which he attempts to solve.

But enough. It is impossible for me to continue this correspondence. The reviewer's task is often not a very pleasant one, but a new terror would be added to the work if it involved an interminable controversy with authors on matters of opinion. Dreading this, I deliberately abstained from signing the review, because I knew from past experience that this was my only chance of escape from the flood of Mr. Howorth's controversial eloquence, which, like the proverbial river, *Labitur et labetur in omne volubilis ævum.* YOUR REVIEWER.

An Incorrect Footnote and its Consequences.

IN following up Baltzer's erroneous reference concerning the "Demonstratio eliminationis Cramerianæ," Mr. Muir, as described in his letter on p. 246, seems at first to have been singularly unlucky. For, on referring to the catalogue of Lord Crawford's mathematical library under "Mollweide," although the work itself was not immediately forthcoming, there was a cross-reference to "Prasse, M. von," under whose name the essay was duly catalogued. The Dun Echt copies, for there are two of them bound up in volumes of mathematical pamphlets, are copies of the original "Demonstratio," in 8 folios, with the pages 4 to 15 numbered, and the last blank. In a gap on the title-page of one copy has been written "auct. Mauricius de Prasse," apparently long ago, and in a German hand. But apart from this the last sentence of the first paragraph identifies the author as the writer of "Usus logarithmorum," which bears the same Latin form of the name in print.

The cross-reference is due to the presence in the library of a little book the title of which is worth giving in full, as it contains the names between which Baltzer's mistake arose, and it also gives the German form of von Prasse's name: it is "Logarithmische Tafeln für die Zahlen, Sinus und Tangenten, neu geordnet von Moritz von Prasse ehemals Prof. der Mathematik in Leipzig, revidirt und vermehrt von Karl Brandan Mollweide ordentl. Prof. der Mathematik in Leipzig. Leipzig,

1821," ix. + 110 pp. 16mo. In the preface Mollweide says that von Prasse was his predecessor in the Chair of Mathematics, as stated by Prof. Virchl. This work is entered under both the title-names in Poggendorff's "Biographisch-literarisches Handwörterbuch," a circumstance that might have given a clue to the authorship of the "Demonstratio." Strangely enough, this is not the only instance in which von Prasse omitted his name in essays written by him for academical celebrations. I can only surmise that this was done with a view to republication in his "Commentationes Mathematicæ," and that the name was written on the copies distributed as invitations to the celebrations. Whatever the reason, it has in this instance obviously added greatly to the trouble ordinarily experienced when dealing with this class of academical essay, the bibliography of which is so complicated, and at the same time often so important.

RALPH COPELAND.

Lord Crawford's Observatory, Dun Echt, January 25.

A New Historic Comet?

AT a recent meeting of the Asiatic Society of Japan, a paper was read by Mr. W. G. Aston, H.B.M. Consular Service. This paper will certainly rank high amongst historic papers relating to Japan and Korea. Briefly described, it is a comparison between the ancient records of these two countries and China, and its aim is to establish the relative credibility of these various records. Mr. Aston has so far confined his attention to the period preceding A.D. 500; and his general conclusion is that, as historic writings, the Korean and Chinese chronicles are far superior to the Japanese of the same date.

In the Tongkan, as the ancient records of the Korean kingdoms are called, there is a notice, of which the following is a translation: "Summer, fourth month, Pèkché; comet visible; day-time." The fourth month began on May 14 or 15. At the request of Mr. Aston, I tried to find out if any such comet had been observed elsewhere. The only list of historic comets obtainable in Japan was the list given in Faye's "Astronomy"; and I am not sure if this is meant to be complete. According to Mr. Aston, the Pèkché comet appeared in May or June, A.D. 302. The nearest date in Faye's list is A.D. 295. If this is the same comet, then one *at least* of the dates must be wrong. It is quite possible, however, that both are correct; in which case we shall be indebted to Mr. Aston for having added one more to our list of historic comets. In coming to a conclusion, we must know to what source we owe the knowledge of the 295 comet, and whether this source has greater claims to chronological accuracy than have the Korean records. Not having the references at hand for studying these points, I have written this note to NATURE, in the hope that someone interested in the matter may be able to come to a decision on this question of a possibly new historic comet.

CARGILL G. KNOTT.

Imperial University, Tokio, Japan, December 19, 1887.

"Is Hail so formed?"

UNDER the above heading in NATURE of January 26 (p. 295) there is a short paper by Cecil Carus-Wilson, in which the writer assumes that under certain conditions, drops of water, whilst falling from the upper branches of a tree, become converted into ice before reaching the ground, whilst other drops falling from the same tree, but at 10 feet less altitude, came to the ground in a fluid state. There is, I think, a simpler solution of this question than the one given. Suppose the following conditions—namely, a frost sufficiently severe as to lower the temperature of the leaves and branches of a tree to a few degrees below the freezing-point; after which a very gradual thaw comes on, accompanied by a fine rain or Scotch mist which freezes on the tree.

Where the leaves and smaller branches hang downwards, small beads of ice would form on their points. As the air became warmer the ice would thaw, and fall to the ground either in the liquid form, or the beads at the ends of the leaves and twigs would become detached in their solid state, and reach the ground as ice-pellets.

Sometimes these ice pellets extend in length, and assume the form of small icicles.

J. RAE.

4 Addison Gardens, January 28.

MODERN VIEWS OF ELECTRICITY.[1]

PART III. (*continued*).

VII.

First Representation of the Field due to a Current.

RETURN now to the consideration of a simple circuit, or, say, a linear conductor, and start a current through it; how are we to picture the rise of the lines of force in the medium? how shall we represent the spread of magnetic induction? First think of the current as exciting the field (instead of the field as exciting the current, which may be the truer plan ultimately).

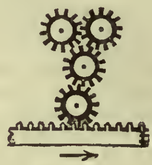


FIG. 34.

If we can think of electricity in the several molecules of the insulating medium connected like so many cog-wheels gearing into one another and also into those of the metal, it is easy to picture a sideways spread of rotation brought about by the current, just as a moving rack will rotate a set of pinions gearing into it and into each other (Fig. 34). But then half the wheels will be rotating one way and half the other way, which is not exactly right.

How is it possible for a set of parallel whirls to be all rotating in the same direction?

FIG. 35.

If there is any sort of connection between them they will stop each other, because they are moving in opposite directions at their nearest points; and yet, if there is no connection, how can the whirl spread through the field?

Well, return to the old models by which we endeavoured to explain electrostatics, and think whether they will help us if we proceed to superpose upon them a magnetic whirl in addition to the properties they already possess. Looking at Figs. 5, 6, and 13, we remember we were led to picture atoms and electricity like beads threaded on a cord. And these cords had to represent, alternately, positive and negative electricity, which always got displaced in different directions.

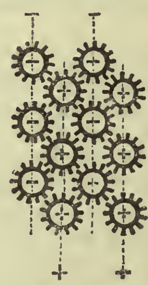


FIG. 36.—Rows of cells alternately positive and negative, geared together; free to turn about fixed axles.

We are forced to a similar sort of notion in respect of the wheels at present under discussion: in order that

[1] Continued from p. 323.

they may co-operate properly, they must represent positive and negative electricity alternately. If they *then* rotate alternately in opposite directions, all is well, and the electrical circulation or rotation in the field is all in one direction. Each wheel gears into and turns the next, and so the spin gets propagated right away through the medium, at a speed depending on the elasticity and density concerned in such disturbances.

It is not convenient at the present stage to ask the question whether the wheels represent atoms of matter or merely electricity. It may be that each atom is electrostatically charged and itself rotates, in which case it would carry its charge round with it, and thereby constitute the desired molecular current.

The apparent inertia of electricity would thus be explained simply enough, as really the inertia of the spinning atoms themselves; and the absence of any moment of momentum in an electro-magnet as tested mechanically would be equally explained by the simultaneous opposite rotation of adjacent atoms. A question may arise as to why the opposite molecules should have exactly equal opposite inertiæ, as they have, else a fluid magnetized medium would bodily rotate; and there may be other difficulties connected with a bodily rotation of electrostatically charged molecules: it is merely a possibility upon which stress must not be laid till it has been proved able to bear it. For our present purpose a spin of the electricity inside each atom, or even independently

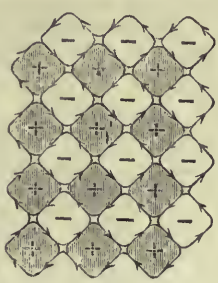


FIG. 37.—Portion of a magnetic field. Another mode of drawing Fig. 36.

of any atoms, is quite sufficient. Besides, since magnetic induction can spread through a vacuum quite easily, the wheel-work has to be largely independent of material atoms.

If any difficulty is felt concerning the void spaces in Fig. 36 it is only necessary to draw it like Fig. 37, which does every bit as well, and reduces the difficulty to any desired minimum.

Representation of an Electric Current.

Now notice that in a medium so constituted and magnetized—that is, with all the wheel-work revolving properly—there is nothing of the nature of an electric current proceeding in any direction whatever. For, at every point of contact of two wheels the positive and negative electricities are going at the same rate in the same direction; and this is no current at all. Only when positive is going one way and negative going the opposite way, or standing still, or at least going at a different rate, can there be any advance of electricity or anything of the nature of a current.

A current is nevertheless easily able to be represented: for it only needs the wheels to gear imperfectly and to work with slip. At any such slipping-place the positive is going faster than the negative, or *vice versâ*, and so there is a current there. A line of slip among the wheels corresponds therefore to a linear current; and, if one thinks of it, it is quite plain that such a line of slip must always have a closed contour. For, if only one wheel slip, then the circuit is limited to its circumference; if a row slip, then the direct and return circuit are on opposite sides of the row. But a large area of any shape with no slip inside it may be inclosed by a line of slip, and this gives us a circuit of any shape, but always closed. Understand; one is not here thinking of a current as analogous to a *locomotion* of the wheels—their axes may be quite stationary,—the slip contemplated is that of one *rim* on another.

Imagine all the wheels inside the empty contour of Fig. 38 to be rotating, the positive clockwise, the negative counter clockwise, and let all those outside the contour be either stationary or rotating at a different rate or in

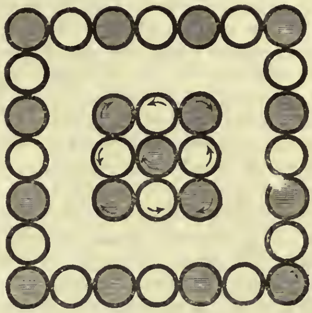


FIG. 38.—Diagram of a peripheral current partitioned off from surrounding medium by a perfect conductor, which transmits no motion, and therefore acts as a perfect magnetic screen.

an opposite direction; then the boundary of the inside region is a line of slip along which the positive rims are all travelling clockwise, and the negative rims the other way, and hence it represents a clockwise positive current.

But it may be said that the spin inside the contour, if maintained, must sooner or later rotate the wheels outside as fast as themselves, and then all slip will cease. Yes, that is so, unless there is a complete breach of connection at the contour, as in Fig. 38 there is. If the outer region has any sort of connection with the inner one the slip at its boundary can only be temporary, lasting during the era of acceleration.

Distinction between a Dielectric and a Metal, as affected by a spreading Magnetic Field.

In a dielectric the connection between the atoms is definite and perfect. If one rotates, the next must rotate too; there is no slip between the geared surfaces; it is a case of cogged wheels. A conduction-current is impossible.

But in a metallic conductor the gearing is imperfect; it is a case of friction-gearing with more or less lubrication and slip, so that turning one wheel only starts the next gradually—it may be very quickly, but not instantaneously—and there is a motion of a positive rim incompletely compensated by an equal similar motion of a negative rim while getting up speed; in other words, there is a momentary electric current, lasting till the wheels have fairly started.

In a perfect conductor the gearing is absent; the lubrication is so perfect that all the atoms are quite free of one another, and accordingly a spin ceases to be transmitted into such a medium at all. The only possible current in a perfect conductor is a skin-deep phenomenon.

A magnetized medium of whatever sort is thus to be regarded as full of spinning wheels, the positive rotating one way and the negative the other way. If the medium is not magnetized, but only magnetic—*i.e.* capable of being magnetized—it may be thought of either as having its wheels stationary, or as having them facing all ways at random; the latter being probably the truer, the former the easier, representation, at least to begin with.

Whether the medium be conducting or insulating makes no difference to the general fact of spinning wheels inside it wherever lines of force penetrate it; but the wheels of a conductor are imperfectly cogged together, and accordingly in the variable stages of a magnetic field, while its spin is either increasing or decreasing, there is a very important distinction to be drawn between insulating and conducting matter. During the accelerating era conducting matter is full of slip, and a certain time elapses before a steady state is reached. A certain time may be necessary for the propagation of spin in a dielectric, but it is excessively short, and the process is unaccompanied by slip, only by slight distortion and recovery. As for a strongly magnetic substance like iron, nickel, or cobalt, one must regard them as constituted in the same sort of way, but with wheels greatly more massive, or very much more numerous, or both.

Phenomena connected with a varying Current. Nature of Self-induction.

Proceed now to think what happens in the region round a conductor in which a current is rising. Without attempting a complete and satisfactory representation of what is going on, we can think of some mechanical arrangements which have some analogy with electrical processes, but do not pretend to imitate them exactly.

Take first a system of wheel-work connected together and moved at some point by a rack. Attend to alternate

FIG. 39.—A provisional representation of a current surrounded by dielectric medium, either propelling or being propelled.

wheels more especially, as representing positive electricity. The intermediate negative wheels are necessary for the transmission of the motion, and they also serve to neutralize all systematic advance of positive electricity in any one direction, except where slip occurs, but they need not otherwise be specially attended to.

Remember that every wheel is endowed with inertia, like a fly-wheel.

Directly the rack begins to move, the wheels begin to rotate, and in a short time they will all be going full speed. Until they are so moving, the motion of the rack is opposed, not by friction or ordinary resistance, but by the inertia of the wheel-work.

This inertia represents what is called self-induction, and the result of it is what has been called the "extra current at make," or, more satisfactorily, the opposing E.M.F. of electro-magnetic inertia or self-induction.

So long as the rack moves steadily forward, the wheel-work has no further effect upon it; but directly it tries to stop, it finds itself unable to stop dead without great violence: its motion is prolonged for a short time by the inertia of the wheel-work, and we have what is known as the "extra current at break."

If the rack is for a moment taken to represent the advancing electricity in a copper wire, then the diagram may be regarded as a section of the complete field: the complete field being obtained from it by rotating it round the axis of the wire. Imagining this done, we see that the axis of each wheel becomes prolonged into a circular core, and each wheel into a circular vortex ring surrounding the rack and rolling down it as it moves forward, as when a stick is pushed through a tight-fitting umbrella-ring held stationary (see Fig. 30 B).

As one goes further and further from the rack the lengths of the vortex cores increase, but there is only a given amount of rotation to be shared among more and more stuff, hence it is not difficult to imagine the rate of spin diminishing as the distance increases, so that at a reasonable distance from the conductor the medium is scarcely disturbed.

To perceive how much rotation of the medium is associated with a given circuit, one must consider the shape of its contour—the position of the return current. Take first a long narrow loop and send a current up one side and down the other. The rotations belonging to each are superposed, and though they agree in direction for the space inclosed by the loop, they oppose each other outside, and so there is barely any disturbance of the medium outside such a looped conductor; very little dielectric is disturbed at all, and accordingly the inertia or self-induction is very small.

If the loop opens out so as to inclose an area, as the centrifugal force of the wheels will tend to make it do,

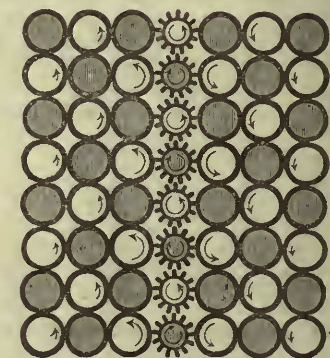


FIG. 40.—Diagram of a direct and return current close together, showing distribution of rotation and of slip in the thickness of the conductor, and in the dielectric between. The dielectric outside is very little disturbed.

then there is a greater amount of rotation, a greater moment of momentum inside it, and accordingly its self-induction is increased. The axis of every wheel is, however, continuous, and must return outside the loop: so the outside region is somewhat affected by rotation, but of a kind opposite to that inside.

Figs. 38 and 41 show the state of things for a closed circuit conveying a current. The free space in Fig. 38 represents a perfect conductor, or perfect breach of connection. Along one side of this space positive electricity is seen streaming in the direction of the arrows, and it may be streaming on the other side also, but nothing happens in its interior—which is therefore not represented.

The corresponding portion in Fig. 41 is intended for an ordinary conductor, full of wheels capable of slip. And slip in this case is a continuous necessity, for the rotation on either side of the conductor is in opposite directions, so the atoms of the conductor have to accommodate themselves as best they can to the conditions; some of them rotating one way, some the other, and some along a certain neutral line of the conductor being stationary. If a conductor is straight and infinitely long, the neutral line of no rotation is in the middle. If it be a loop, the neutral line is nearer the outside than the inside, because the rotation of the medium inside is the strongest. If the

loop be shut up to nothing, the neutral line is its outer boundary or nearly so (Fig. 40). If, again, the circuit is wound round and round a ring, as string might be lapped upon a common curtain-ring to cover it, then the axes of whirl are wholly inclosed by the wire, and there is no rotation outside at all.

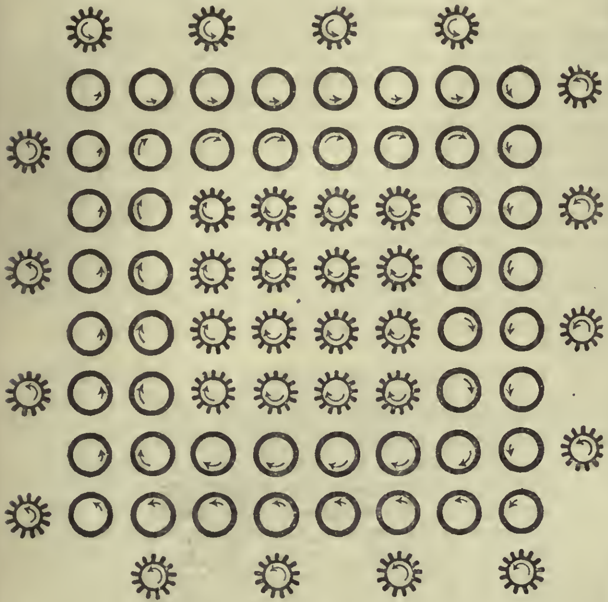


FIG. 41.—Diagram of simple conducting circuit like a galvanometer ring, with the alternate connecting-wheels omitted. The same number of dielectric wheels are drawn outside as inside, to indicate the fact that the total spin is equal inside and out, though the outside is so spread out as to be much less intense.

Fig. 42 shows a section of this last-mentioned condition, and here the wheels of the dielectric outside are not rotating at all. The inside is revolving, it may be furiously, and so between the inner and outer layers of the conductor we have a great amount of slip and dissipation of energy.

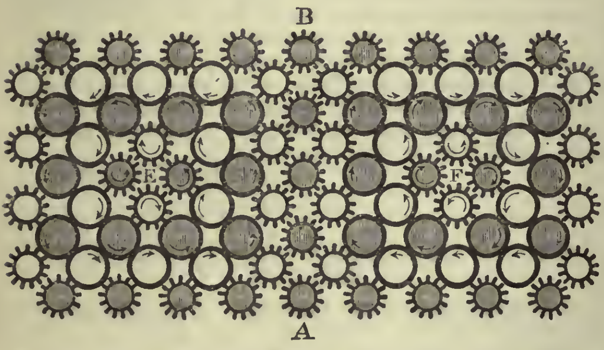


FIG. 42.—Section of a closed magnetic circuit, or electric vortex-ring, or hollow bent solenoid like Fig. 29, inclosing an anchor-ring air-space; the axis of the ring being A B, the sections of the core being E and F. The arrows indicate the intensity of the spin, *i.e.* of the magnetic field, which is a maximum at the middle of each section and nothing at all outside. If the core contains iron instead of air, its wheels have to be from 100 to 300 times as massive: slipping wheels if solid iron, cogged wheels if a bundle of fine varnished iron wires.

The process of slip which we have depicted goes on in all conductors conveying a current, whether steady or variable, and in fact *is* the current. The slip is necessarily accompanied by dissipation of energy and production of heat: only in a perfect conductor can it occur without friction. In a steady current the slip is uniformly distributed throughout the section of the conductor; in the variable stages it is unequally distributed, being then more concentrated near the periphery of the wire.

When a current is started in a wire, the outer layers start first, and it gradually though very quickly penetrates to the axis. Hence the lag or self-induction of a wire upon itself is greater as the wire is thicker, and also as it is made of better conducting substance. If it is of iron, the mass or number of the wheels is so great that the lag is much increased, and the spin of its outer layers is great enough to produce the experimental effects discovered by Prof. Hughes.

One must never confuse the slip with the spin. Slip is current, spin is magnetism. There is no spin at the axis of a straight infinite wire conveying a current, and it increases in opposite directions as you recede from the axis either way; arranging itself in circular vortex cores round the axis. But the slip is uniformly distributed all through the wire as soon as the current has reached the steady state. The slip is wholly in the direction of the wire. The axes of spin are all at right angles to that direction.

Rise of Induced Current in a Secondary Circuit.

To study the way in which a magnetic field excited in any manner spreads itself into and through a conducting medium, look at Fig. 43, and suppose the region inside the contour A B C D to be an ordinary conducting region—that is, full of wheels imperfectly geared together, and capable of slip.

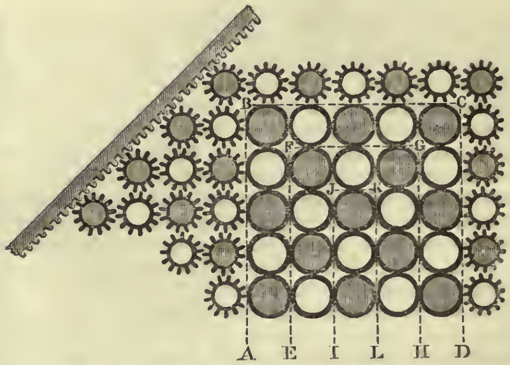


FIG. 43.—Diagram illustrating the way in which an induced current arise in a mass of metal immersed in an increasing magnetic field; also how it decays. The dotted lines A B C D, E F G H, I J K L, are successive lines of slip.

Directly the rack begins to move, all the wheels outside A B C D begin to rotate, and quickly get up full speed. The outer layer of wheels inside the contour likewise begins to rotate, but not at once; there is a slight delay in getting them into full motion. For the next inner layer the delay is rather greater, and so on. But ultimately the motion penetrates everywhere equally, and everything is in a steady state.

But while the process of starting the wheels was going on, a slip took place round the contour A B C D, and round every concentric contour inside it; the periphery of the positive wheels moving in a direction opposite to that of the wheel in contact with the rack, and so suggesting the opposite induced current excited at "make" in the substance of a conductor near a growing current, or generally in an increasing magnetic field.

The penetration of the motion deeper and deeper, and the gradual dying away of all slip, illustrate also the mode in which this induced current arises and gradually dies away, becoming *nil* as soon as the magnetic field (*i.e.* the rotation) has penetrated to the interior of all conductors and become permanently established there as elsewhere.

Suppose the motion of the rack now stopped: all the cogged wheels stop too, though it may be with a jerk and

some violence and oscillation due to their momentum; but those inside the contour A B C D will continue moving for a little longer. The outside layer of this region will slip in such direction as to illustrate the direct induced current at "break," and will begin to stop first; the slip and the stop gradually penetrating inwards, just as happened during the inverse process, until all trace of rotation ceases. This inverse slipping process is the direct induced current at "break."

Through a perfect conductor the disturbance could never pass, for the slip of the dielectric wheels on its outer skin would be perfect, and would never penetrate any deeper. A superficial current lasting for ever, or rather as long as the magnetic field (the rotation of the dielectric wheels) lasts, is all that would be excited, and it would be a perfect magnetic screen to any dielectric beyond and inclosed by it. OLIVER J. LODGE.

(*To be continued.*)

THE BIRDS'-NEST OR ELEPHANT ISLANDS OF THE MERGUI ARCHIPELAGO.

OF the geological structure of this group of islands lying off the coast of British Burmah not much is yet known. Our readers will probably be interested in the following account of a visit to one portion of the archipelago, furnished by Commander Carpenter, R.N., to the Hydrographer of the Admiralty, to whose kindness we are indebted for permission to publish it.

The remarkable group of islands called by the Burmans Ye-ei-gnet-thaik (*lit.* sea-birds' nests) is located on the south-east side of Domel Island, one of the largest of the Mergui Archipelago. It is composed of six marble rocks, the highest and largest of which, 1000 feet in altitude, and about one mile in length, is oval-shaped, and rises very abruptly out of a depth of only 5 fathoms. The islands present a very striking appearance, particularly if the weather is hazy, when they are not seen until within five or six miles, for then they gradually loom out through the mist like some huge misshapen monsters that have strayed away from civilization. Their sides are partly clothed with vegetation wherever a break in the limestone has left a cleft in which moisture and dust can lodge. Conspicuous because of its leaning attitudes is a species of tree-fern which grows at any angle, but only above a height of 200 feet from the water. The face of the rocks is reddish, partly from weathering and partly from soil, and where cliffs exist the most beautiful though uncouth stalactites have been formed, showing grotesque and snake-like patterns varying in hue and shape till one feels as if in some enchanted land. But the great feature of the group is the birds'-nest caverns, which as a rule open into the sea, the entrance being below high-water mark; fortunately I visited them at spring tides, and had plenty of leisure to examine each cavern at low water during two days.

At the south end of the largest island stands a "nine-pin" of gray marble 370 feet high, almost separated from the rest. It is hollow, like a huge extinguisher, and the polished light-blue and yellow sides of the interior seem to point to its having been hollowed by the swell of the sea, which on entering the cave would probably expend its force vertically, the mouth of the cave being open to the direction of the strongest seas. This sea-stack forms the western point of a nearly circular cove, 360 yards in diameter, which runs back into the island, and the sides of the cove rise steeply though not perpendicularly from it. At the head of the cove is a perpendicular wall of rock over which can just be seen the 1000-foot summit in the distance.

At half-tide a tunnel, passable for a canoe, opens under the wall of rock at the head of the cove, but a ship's gig can only enter within an hour of low-water spring tides. This tunnel has a roof covered with large stalactitic knobs except at its narrowest part, where it is apparently scoured smooth by the action of the tidal rush. It is about 250 feet long, and 4 feet deep at low water (the rise and fall of the tide being 16 feet), and is covered with dripping marine life, corallines, small corals, Comatulæ, sponges, and sea-horses. Passing through this submarine passage one emerges into another circular crater-shaped basin with perpendicular sides. This basin is only open to the sky; caves here and there enter it, some of which may perhaps lead by long tunnels to other basins. Water was running freely into it from the foot of the cliffs in several places as the tide fell, showing that water spaces existed, and strange gurgling sounds as of air taking the place of water could be heard now and again. There were hardly any signs of the place being frequented by man except here and there the worn ropes of birds'-nest climbers. It was either not the season for the swallows, or they had deserted the islands, for none were seen. A little reddish guano was noticed in some of the caves. There can be but little traffic through the tunnel by which we entered, for the delicate growth on its sides was hardly injured.

On the west side of the northern large island a lofty cavern is connected at half-tide with another nearly circular basin of about the same size as that we have just described, but in this case the basin also opens into the sea on the east side of the island. After contemplating the cliffs that surround these basins, the general circular contour of the ridges of the islands, the undermining action of the sea at the water-line, which causes in some places an overhang of 20 to 25 feet, and the softening of the marble surface of the cavern roofs by moisture, the conviction gradually forces itself on the mind that these circular basins were themselves at one time the floors of huge caverns; that in days gone by the islands rose far higher, with cavern piled on cavern, and that the work of disintegration by solution and wave-action is slowly going on, pulling down these marble monuments of a giant age. Indeed, here and there a fall of blocks has occurred lately, and, as there is no shoal off the base of the slip, the destructive action is probably rapid.

A small oyster covers the rocks at the water-line. A handsome kingfisher was secured and sent to the British Museum. A few doves and an eagle or two were the only other birds seen, besides a small bat in the caves. By the position of the nest-seekers' ropes, the swallows appear to build only on the roofs of the caves. The islands appeared to be entirely composed of a blue-tinted marble. A vessel could lie alongside them and lower the cut blocks straight into her hold, but it is probably of too poor a quality to be worth shipment.

ALFRED CARPENTER.

PRIZE FOR RESEARCHES IN NATURAL HISTORY.

IN accordance with the intentions of the founder, the Committee of Schnyder of Wartensee's Foundation, Zürich, have decided to offer for the year 1890 a prize for the following researches in natural history:—

"New investigations are desired regarding the relation which the formation of the bones bears to the statics and mechanics of the vertebrate skeleton. The results of the investigations as a whole are to be demonstrated in detail by way of example on the skeleton of a definite species."

The conditions are as follow:—

Art. 1. Competitors for the prize must send in their work in German, French, or English, by September 30, 1890, at the latest, to the address given below in Art. 6.

Art. 2. The award will be made by a Committee consisting of the following gentlemen:—Prof. Hermann von

Meyer, Zürich; Prof. L. Rütimeyer, Basle; Prof. H. Strasser, Berne; Prof. Otto Mohr, Dresden; and Prof. Albert Heim, of Zürich, representing the Committee offering the prize.

Art. 3. The judges are authorized to award a first prize of two thousand francs; and a further sum of one thousand francs is placed at their disposal for distribution in minor prizes according to their discretion.

Art. 4. The work awarded the first prize becomes the property of the Foundation of Schnyder of Wartensee, which will arrange with the author regarding the publication of the same.

Art. 5. Each competing work must bear on the title-page a distinguishing motto, and must be accompanied by a sealed envelope containing the name of the author, and bearing on the outside the same motto.

Art. 6. Competing works are to be sent in by the date named in Art. 1, to the following address: "An das Präsidium des Conventes der Stadtbibliothek in Zürich (betreffend Preisaufgabe der Stiftung von Schnyder von Wartensee für 1890)."

NOTES.

THE death of Sir Henry Maine, F.R.S., has created a great blank in the serious literature of England. He was the first Englishman who applied to the study of law and early institutions the rigid methods of science, and the results at which he arrived marked an epoch in the investigation of these subjects. His literary style, combining as it did extraordinary vigour, lucidity, and grace, was scarcely less remarkable than his grasp of far-reaching principles. He died suddenly, of apoplexy, at Cannes, on Friday evening last. He was in his sixty-sixth year.

IN a letter received from Mr. John Whitehead, dated Labuan December 13, 1887, that gentleman writes:—"To-day or to morrow I start for Kina Balu, and I hope to make this a famous and last expedition into Borneo, for I really am in wonderful health considering everything, but at the same time I am rather tired of Borneo, with its fevers, heat, and mosquitoes. I hope to be back in England in August and September. I do not like to brag of what I hope to do, as things are so uncertain. Natives may refuse to help me, and may perhaps attack me, for the country round this fine mouutain is by no means settled." On the last occasion of his visiting Kina Balu a year ago, Mr. Whitehead was only able to remain a month upon the mountain, but he discovered nineteen new species of birds in that short time, some of them being really wonderful novelties. He now hopes to remain for at least six months, and this he will doubtless be able to do, if he can secure supplies for his hunters, and keep open his communication with Labuan.

MR. WHITEHEAD'S collections from the island of Palawan have now arrived in this country, and a brief account of them will appear in the April number of the *Ibis*. This island has already been visited by Prof. Steere, Mr. Alfred Everett, and Mr. E. Lemprière, all of whom made collections in the neighbourhood of Puerto Princesa. Mr. Whitehead's labours were also confined to the vicinity of this post, as he was prevented from visiting the interior. He has succeeded, however, in procuring specimens of every species met with by the three travellers above-mentioned, and has besides obtained about sixty additional species, several being new to science.

THE Council of the Royal Meteorological Society have arranged to hold, at 25 Great George Street, Westminster, on March 20–23 next, an Exhibition of Apparatus connected with Atmospheric Electricity, including lightning-conductors, photographs of lightning, and damaged objects. The Committee will also be glad to show any new meteorological instruments or apparatus invented or first constructed since last March; as well as photographs and drawings possessing meteorological interest.

A FRIENDLY meeting of employers and working men, to discuss the best means of obtaining technical education, will be held at the Royal Victoria Hall, Waterloo Bridge Road, on Wednesday, February 15, at eight o'clock. The chair will be taken by Sir Douglas Galton. This meeting has been arranged in consequence of the great interest shown in a similar meeting held at the same place on December 14. The speakers will be limited to ten minutes, and those who wish to speak must send in their names the day before the meeting.

THE new American Folk-Lore Society was definitely organized at a meeting held at Harvard College on January 4. The object of the Society is the study of folk-lore in general, and especially of folk-lore in North America. The first President is Prof. F. J. Child, of Harvard, and the acting Secretary is Mr. W. W. Newell, of Cambridge, Mass. It is expected that the first number of the Society's journal will be published in April.

THE Duchess of Albany has become Patroness of the Parkes Museum, of which the Duke of Albany was President until his death.

DURING the coming spring the construction of the North Sea and Baltic Canal will be begun along the whole line. There will be seven camps of workmen, and 4000 men employed.

THE Education Department of Scotland has issued a circular to the various School Boards in that country, in which are embodied the results of the careful inquiries that have recently been made into the existing system of elementary scientific teaching in Scottish schools. Technical instruction is discouraged in primary schools till the boys have reached the higher standards, and even then, the Department thinks, no attempt should be made unless skilled teachers and abundance of scientific apparatus are available. In most instances the thorough teaching of elementary science is beyond the reach of the primary schools; but by various School Boards uniting to employ a trained staff of teachers much of the difficulty will be overcome. School Boards are also recommended to seek the aid of local committees consisting of manufacturers who know what technical education is most needed in the district. The Department also recommends the extension of the system of giving evening lectures, which have been so successful in the past, and the charging of fees low enough to be within the reach of all. Nothing would tend to make technical education more popular than a small rate of charge, combined, as it should always be, with trained help and an abundant supply of scientific instruments.

A STRIKING new experiment, exhibiting the terribly explosive nature of chloride of nitrogen, is described by Prof. Victor Meyer in the current number of the *Berichte*. A few drops of the yellow chloride were prepared in the usual manner by inverting an exceptionally thin flask filled with chlorine gas in a leaden dish containing a solution of ammonium chloride. Instead, however of gently agitating the apparatus so as to cause the drops to fall into a smaller leaden capsule placed beneath the mouth of the flask, they were allowed to float freely upon the surface. The whole apparatus was then inclosed in a cover-box fitted with stout plate-glass sides, through the top of which was passed a bent pipette, turning up below just under the mouth of the flask and connected outside with a dropping funnel containing chloride of ammonium solution and a few drops of turpentine. When sufficient chloride of nitrogen had collected, the tap of the funnel was carefully turned so as to allow a little turpentine to slowly rise in the flask. After a moment or two it reached the surface and mingled with the chloride of nitrogen, causing a brilliant

flash of light and a loud explosion, which Prof. Meyer likens to a thunder-clap, so much more powerful is the detonation in a confined space. The flask of course was shattered, not into powder, but into tolerably large fragments; the plate-glass box, however, even after many repetitions of the experiment, remained intact, a small door on the side away from the observers having been left ajar so as to prevent any notable increase of pressure. Curiously, the chloride of nitrogen never entirely exploded; a part remained in the distorted leaden dish and maintained an incessant fusillade for more than a minute.

AT the last meeting of the Göttingen Chemical Society, Dr. Gattermann read a preliminary note upon his recent researches as to the nature of chloride of nitrogen. From his analyses it appears pretty clear that the yellow liquid is a mixture of at least two distinct chlorides, which he has hopes of being able to separate. During the course of the experiments the reason of its capricious behaviour, the cause of so many painful accidents in the past, was happily discovered. It is decomposed by the actinic rays of light, being rapidly acted upon by sunlight with periodic spontaneous explosion, and is at once fired by exposure to the rays of burning magnesium. Hence further light upon this difficult and dangerous subject can only emanate from the dark room, a paradox the truth of which Dr. Gattermann is endeavouring to demonstrate.

ON the morning of Tuesday, January 31, a distinct shock of earthquake is said to have been felt near Birmingham. In and around Coventry, too, several persons say that they experienced sensible vibrations of their houses and heard rumbling noises. At Hartshill the ceiling of a house was cracked by the shock. On Thursday, February 2, a sharp shock of earthquake was felt over a large part of Scotland. The following details regarding this shock are taken from the *Times* of Friday, February 3:—"The shock was distinctly felt at a quarter past 5 o'clock in Perth. The tremor lasted about one minute, and consisted of five or six slight, wave-like motions from west to east. In the Breadalbane and Grantully districts of Perthshire the shock lasted six seconds. It was also felt very distinctly in Aberfeldy, Acharn, Kenmare, and Strathay. It is twenty years since these districts were similarly affected. In Strathearn two shocks were felt, the first about half past 3 o'clock, and the second about 5 o'clock. Further north, in Inverness-shire and Ross-shire, a shock was felt about 5. It was sharper and seemed to travel from south-west to south-east. The tremor in Dingwall is likened to the vibration caused by a heavy waggon passing along a road, while at Crieff it was like a very heavy body thrown to the ground. In Beauly and Strathglass people were greatly alarmed. Their houses shook, dishes fell, furniture was broken, and numbers of people rushed from their beds and out of houses without dressing. On the west coast the shock was very violent. It was also felt at Mull. From Fort William it is reported that there was a slight shock at 5 a.m., which affected the old Caledonian valley, and extended down to the line of the Moray Firth."

THE February Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, contains a list of such hardy herbaceous annual and perennial plants as have matured seeds under cultivation in the Kew Gardens during the year 1887. "These seeds," it is explained, "are available for exchange with colonial, Indian, and foreign botanic gardens, as well as with regular correspondents of Kew. But the seeds are for the most part only available in moderate quantity, and are not sold to the general public. In the years 1885 and 1886 the list was printed as an independent publication. It has now been thought more convenient to issue it as a number of the Bulletin. Every effort is made to correctly determine the nomenclature of the plants in the list. As far as it goes, it will serve as a record of the herbaceous species cultivated at Kew. It must, however, be remembered that a considerable proportion of herbaceous plants do not mature seeds in the climate of England, and these are necessarily not included in the list."

MUCH inconvenience is caused by the fact that lists of recent additions to public libraries are not always readily accessible to persons who would like to make use of them. Readers at the Darwen Free Public Library may congratulate themselves that in their case this difficulty has been overcome. The other day the *Darwen News* printed the first instalment of a list of books which have been lately added to the collection belonging to that institution, and which are not to be found in the catalogue. Two similar instalments will follow, and afterwards lists will be given as books are purchased. If readers will take the trouble to cut out these lists and place them at the end of their copies of the catalogue, they will know exactly what works have been secured for the library. From the instalment just issued, it is obvious that the managers of the Darwen Free Public Library exercise great discretion in their choice of books, and we are glad to see that among the works selected by them science is very fairly represented.

SEVERAL correspondents have written to us about Mr. John Morison's letter, printed last week (p. 321), on what he supposed to be a case of untimely insect development. Mr. Edward Buckell, of Romsey, writes:—"Surely Mr. Morison has overlooked the fact that *Vanessa urticæ* hibernates in the imago state, selecting for that purpose houses and such other warm quarters as it can find. I have counted nine in one house. During hibernation the insect is naturally in a semi-torpid condition. As to the 'abnormal appearance' of the antennæ, I think that if Mr. Morison observes other specimens, both during the winter months and after sundown in the summer, he will find the position noted by him to be the usual one."

WE referred last week (p. 328) to M. Lancaster's work on the climate of Belgium in 1887; we are also indebted to him for an elaborate discussion of the barometer observations taken at Brussels Observatory during the fifty years 1833–82. That Observatory owes its origin to the efforts of the late L. A. J. Quetelet, President of the International Maritime Conference held at Brussels in 1853, to which Conference our own Meteorological Office owes its origin. The Annals of the Observatory contain one of the most complete series of climatological and phenological observations extant. The barometrical results, which M. Lancaster has carefully resumed, are drawn from the eye observations taken up to June 1841, and since that time from self-recording instruments, one of which is a photographic barograph of the Kew pattern. In addition to the usual monthly and annual means, the tables contain summaries of the days on which the barometer was above or below certain values, the epochs of all remarkable falls and rises, five-day and seasonal means. The mean for the whole period was 29·766 inches (not reduced to sea-level); the greatest height was 30·753 inches on January 17, 1882; and the lowest, 28·367 inches, on December 10, 1872. The mean diurnal range for the year was 0·023 inch. The diurnal and monthly variations are greatest in winter and least in summer; the highest and lowest absolute readings occur generally in the month of January.

MR. R. H. SCOTT delivered a lecture on British and Atlantic weather, at the London Institution, on the 2nd instant. After some interesting remarks on the effect of difference of height upon vapour, the dependence of our weather on the upward or downward movement of the atmosphere in cyclonic and anti-cyclonic systems, and on the cause of fogs, he discussed the utility of the present American reports in forestalling storms, based on a consideration of their movements as shown by the Atlantic Weather Charts lately published by the Meteorological

Council. These charts showed that only a small proportion of storms travelled across the Atlantic. The track of the depressions is determined by the distribution of pressure over the ocean, and of this distribution we are ignorant at the time of despatch of telegrams from America. The lecturer stated that in their present incomplete form the telegrams were of no assistance to the Meteorological Office in issuing storm warnings.

DR. BILLWILLER reports the establishment of a permanent observatory on the summit of the Santis, in October last. This observatory ranks as the third in height in Europe, being at an elevation of 8200 feet, and 108 feet higher than the temporary station at the Gasthaus, on the Santis, where the observations have been taken for the last five years. The results of these observations are published in a *Neujahrsblatt*, by the scientific Society of Zürich. The lowest temperature during the five years was $-9°$ F. on March 13, 1883, and the highest, 69°, on July 21, 1886. The prevalent winds were westerly and south-westerly, which usually occur on high mountains in these atitudes.

ACCORDING to the last annual report on the Dutch East, Indies, rainfall was measured at 183 stations in these possessions. The military portion of the report, the topographical survey of Java, on a scale of 1 : 200,000, is completed, and the members of the Survey have been sent to the west coast of Borneo, where a preliminary survey to join certain points already astronomically determined has been undertaken. The survey on the west coast of Sumatra will also be continued. A considerable part has already been triangulated, and 344 positions have been determined. The definitive calculation of the triangulation work of Java, on which Prof. Oudemans, of Utrecht, has been at work for five years, is not yet completed.

RECENT Java journals give particulars of a remedy for coffee-leaf disease, discovered by Dr. Burck, manager of the Government [Botanic Gardens at Buitenzorg, near Batavia. The specific is said not only to cure the disease, but also to prevent its recurrence. For preventive purposes, he makes use of a highly attenuated solution of chloride of iron applied to the under portion of the leaves by means of a pulverisator. The sticky nature of the solution enables it to adhere two months to the coffee-leaves. It is a powerful antidote to the *Hemileia vastatrix*. To stay the progress of the latter when it has once taken hold, a different method is employed. The coffee-leaves in which the *Hemileia* first manifests itself in the form of orange-coloured spots are at once taken in hand. Holes are pricked in the spots with a needle dipped in a strong solution of sulphuric acid, which kills all the germs of the disease in the leaf. Dr. Burck estimates the cost of the preventive specific at 2½ guilders per 133 lbs., and the healing remedy at 4 guilders. He anticipates that the price of coffee will be enhanced in consequence. The second specific in particular is said to have yielded good results and to be easy to administer. The economic value for Java of the discovery of the remedies, should they prove successful, can scarcely be over-estimated. In Ceylon the disease in the coffee-plant produced a revolution in planting; year after year the coffee crops were failures, many planters were ruined, and ultimately tea-growing took the place of coffee with results which are just now astonishing the world. But the period of transition from one staple to the other was one of economic disaster, from which perhaps Dr. Burck has saved Java.

WE have received the recent issues of the *Journal of the Asiatic Society of Bengal*. Vol. lv. Part 2, No. 5, is wholly occupied by the second instalment (Rhopalocera) of the Lepidopterous insects collected in Tavoy and Siam during 1884–85, under the superintendence of Mr. Pitman, the Chief Superintendent of Telegraphs. Of this list nearly 100 species are quite new. The list is drawn up by Messrs. Elwes and L. de Nicéville. Vol. lvi. Part 2, has numerous and varied contributions. The first paper is by Mr. Blanford, on the influence of Indian forests on the rainfall. The other papers are: the changes in the density of sea-water, by S. R. Elson; notes on Indian Rhynchota, Heteroptera, Part 1, by E. T. Atkinson; new species of Ficus from New Guinea and Sumatra, by G. King, where eleven new species are spoken about; the mammals and birds collected by Captain Yate with the Afghan Boundary Commission, each species briefly described and commented on by J. Scully; the species of Loranthus indigenous to Perak, by G. King; Étude sur les Arachnides de l'Asie meridionale, by M. Eug. Sîmon; the differential equation of a trajectory, by H. Mukhopadhyay.

THE current number of the *Folk-Lore Journal* (vol. vi. part i.) contains a most interesting collection of Aino tales and legends, made by Prof. Chamberlain, the well-known Japanese scholar. We are glad to see that he was encouraged to publish the collection by certain observations contained in a review, in these columns, of his recent monograph on the Ainos. The collection consists of fifty-four tales (a few being omitted as unfit for general publication), classified under the headings—tales accounting for the origin of phenomena, moral tales, tales of the Panaumbe and Penaumbe cycle, miscellaneous tales, and, finally, scraps of folk-lore. These were all taken down from the mouths of Ainos. The other papers in the number include a continuation of one on Irish folk-lore collected from a "Statistical Account or Parochial Survey" of that country published more than seventy years ago; and a collection of the traditions of a race as curious in its way as the Ainos, the Mentra, or aborigines of Malacca and the adjoining States.

THE January number of the *Auk* announces that the affairs of the American Ornithologists' Union have considerably brightened, and that by the public spirit of some of its members the Association begins the new year free from debt. The new part also strikes us as full of vigour, and of more than ordinary interest, and it is evident that the recent exertions of the Americans in getting collections from little-known parts of the Western world, such as Texas and Northern Mexico, are being amply rewarded. The reviews are as good as ever, and some of the original papers are excellent. Mr. Sennett describes a new finch from Arizona, and gives an account of the North American species of *Peucæa*, but not one word is said respecting the "Biologia Centrali-Americana" of Messrs. Salvin and Godman, where all Mr. Sennett's facts were duly set forth a year ago. The new Arizona finch, *Peucæa ruficeps scotti*, of Mr. Sennett, was also described some months ago by Mr. Bowdler Sharpe as *Peucæa homochlamys* in the twelfth volume of the "Catalogue of Birds," but as this work has only just appeared, Mr. Sennett's name secures priority of publication. Mr. Brewster's new species from Mexico seem to rest on somewhat trivial characters, at least in the case of the small owls (*Scops*) and the *Aimophilæ*.

AMONGST the subjects of special interest referred to in the report of the Smithsonian Institution for the past year is the exploration for a collection of skeletons and skins of the now almost extinct American bison or buffalo. The exploraton was very successful, a small herd being found in a wild part of Montana, from which the officers of the Institution secured a series of skins, as well as sixteen complete skeletons, and fifty-one dry skulls. The herd appears to have been completely exterminated by the settlers soon afterwards. An expedition was also despatched to the Swan Islands in the Caribbean Sea, which are said to abound in land birds in great variety, and also in large iguanas and other reptiles. The National Museum

collections are extending so rapidly that the provision of additional and adequate space for their exhibition is becoming a matter of pressing importance. In 1884 the number of specimens was estimated at 1,471,000; the number is now increased by more than a million. At the beginning of the present decade there was only one curator with a few assistants; now there are thirty-one regularly organized departments and sections, under the care of twenty-six curators and numerous assistants. The work of the Ethnological Bureau in all its branches—mound explorations, general field studies, and office work—appears to proceed as vigorously as in previous years. The most important forthcoming work of the Bureau appears to be a report by Mr. E. W. Nelson, on the Eskimo of Northern Alaska. During 1886 the vocabularies of twelve Eskimo dialects were arranged in the form of a dictionary, which will form one part of his report; the other will contain chapters on Eskimo life and customs in Alaska, illustrated by photographs taken on the spot.

AN elaborate review of the mineral industries of the United States during the year 1886 has just been issued by the United States Geological Survey. It is the fourth of a series of volumes entitled "Mineral Resources of the United States." The first three volumes contain the statistics from 1882 to December 31, 1885.

THE U.S. Bureau of Education has issued the first of what promises to be a most interesting series of "Circulars of Information." The present Circular is by Dr. H. B. Adams, who has chosen as his subject the College of William and Mary. This College was founded in 1693 by Royal grant, and was long supported by popular legislation in Virginia. The greater part of its property was destroyed during the Civil War, and since that time the institution has been allowed to decline almost to ruin. Dr. Adams' aim, as explained in a prefatory letter, has been to discover the historical beginnings of the higher education in the South; to trace the causes of the early prosperity of William and Mary College; to show its influence upon Virginia statesmen and the Southern States, its relation to the University ideas of Jefferson and Washington, and its significance to the whole country; to point out the causes of the decline of William and Mary College; to explain the rise of the University of Virginia, and the necessity of popular support for the higher education.

POPULAR editions of the late Dr. Parkin's volumes—"Are Epidemics Contagious?" and "The Volcanic Origin of Epidemics" (Sampson Low)—have been published. Dr. Parkin died nearly two years ago at the age of eighty-five, and it is explained in an editorial note that "his long and strenuous life had been devoted to the study of cholera and similar epidemics." His attention was first specially directed to the subject of cholera more than fifty years ago, when he was visiting India and China. A prolonged series of observations and experiments satisfied him that "the cause of the disease was atmospheric, and that carbonic acid gas was its antidote." The editor of these volumes admits that Dr. Parkin's theories have met with comparatively limited acceptance in England. This fact he attributes in part to the comparative mildness of cholera outbreaks in England, in part to "an erroneous notion that the results of Dr. Parkin's teaching were hostile to sanitation."

MESSRS. LONGMANS have just issued the ninth edition of Mr. William Jago's "Inorganic Chemistry, Theoretical and Practical." In this edition the paragraphs are numbered, and have side-headings. The more important statements and definitions are printed in bolder type. A number of students and teachers have pointed out to Mr. Jago that they have been using his book in preparing for the Matriculation Examination of the London University, and have been inconvenienced by its not covering the whole syllabus of that examination. Chapters have now been added to supply this want.

AT the last meeting of the Society of Science of Christiania Prof. Schöyen exhibited and described four species of Lepidoptera new to the Norwegian fauna; viz. *Agrotis præcox*, L., found as larva in the Hval Islands, in the Christiania Fjord; *Asopia glaucinalis*, L., produced from larva at Christiania; *Tortrix inopiana*, Haw., from southern Aurdal; and *Cerestoma nemorella* from Christiansand. At the same meeting Prof. Blytt read a paper on the alterations of the so-called "Strandlinjer," or shore-lines, in Norway, maintaining that the changes in the division of land and sea might have been caused by an alteration in the length of day and night.

A RESIDENT in the isolated little island of Bornholm, in the Baltic, writes to a Danish journal that a curious Christmas custom is observed in that island. When the so-called "Christmas table" has been spread on Christmas Eve, a large long loaf of rye bread is laid at the upper end of it. In this loaf, before it is baked, two transverse grooves are made about 3 inches from each end. On the top of the loaf a large cheese and various articles of food are laid. This is the so-called "*Julegalt.*" It remains untouched throughout Christmas, and when the table is not in use, the cloth is gathered from the other end and laid over the "*galt.*" This curious custom is believed to have been handed down from Pagan times, the *galt* (pig) having reference to Frey's *galt* or pig "Gyldenbörste" ("Gold bristle"). Frey was the god of rain, sunshine, harvest, and general felicity.

THE acclimatization of the so-called "American" trout in Norwegian waters has been very successful. Attempts are now about to be made to acclimatize black bass obtained from America.

THE Danish Government has decided upon forming an oyster bank in the Limfjord, in Jutland, and has despatched the inspector of the Danish fisheries to Norway to obtain all possible information respecting the artificial banks formed in that country during the last few years.

THE manner in which the spruce and pine forests of Norway are being exterminated, is becoming so serious that the Government is called upon to put a stop, by legislation, to the deforestation of the country. At present there is no law to prevent the purchaser of a forest from felling everything, even down to the tiniest saplings. It is urged by forest officials that trees under a certain diameter should not be permitted to be cut, and that the branches of the trees should not be left in the forest (as is now nearly always done), because they stifle the growth of the young trees. Apart from the wanton exhaustion of this commercial wealth, it is maintained that wholesale felling has the effect of changing the climate in the forest localities.

THE strict preservation of the eider fowl on the south-east coast of Sweden during recent years has had the effect of greatly augmenting the number of these valuable birds. The penalty for killing one is very heavy, and informers receive a considerable reward.

DURING the present winter term there are 26,945 German students at the German Universities. Of this number 5791 study theology, 5769 law, 6650 medicine, and 8735 belong to the Philosophical Faculty. 1644 students are foreign. The Vienna University has 238 theologians, 2569 law students, 1565 medical students, and 634 of the Philosophical Faculty. In Graz there are 1305 students, and in Innsbruck 863. Prague has 3805, Cracow 1234, Lemberg 1112, and Czernowitz 259. At Berne University there are 637 students, 51 theologians, 158 law students, 287 medical students, and 141 physical science students. At Zürich there are 70 female students, 40 being medical.

THE *Bulletin Pharmaceutique* states that a new remedy for Phylloxera has been discovered by M. Laffon, of Capendu, and it has proved successful. It consists of a weak solution of nitrate of mercury.

THE additions to the Zoological Society's Gardens during the past week include a Red-winged Parrakeet (*Aprosmictus erythropterus*); eight Peaceful Doves (*Geopelia tranquilla*) from Australia, presented by the Hon. Stormont Finch-Hatton; a Fulmar Petrel (*Fulmarus glacialis*) from Norfolk, presented by Mr. H. M. Upcher, F.Z.S.; a Jardine's Parrot (*Pæocephalus gulielmi*) from West Africa, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE ROYAL ASTRONOMICAL SOCIETY'S MEMOIRS.—The first part of vol. xlix. of the Memoirs of the Royal Astronomical Society has just been published, and contains a new General Catalogue of nebulæ, by Dr. J. L. E. Dreyer. Sir John Herschel's General Catalogue, published in the Philosophical Transactions for 1864, was almost entirely founded upon his own and his father's observations, and hence, since several observers have devoted themselves to the work of searching for nebulæ since that catalogue was prepared, the number known to us has been very largely increased. D'Arrest's great work on nebulæ, which appeared three years later than the General Catalogue, gave the means of correcting many of its positions, and hence Dr. Dreyer had been induced as early as 1876 to compile a supplement to the General Catalogue, which he published in the Transactions of the Royal Irish Academy in 1878 (vol. xxvi.), containing a list of corrections to it, and a catalogue of recently-discovered nebulæ. In 1886, Dr. Dreyer presented a second similar supplement—in which the later discoveries of Messrs. Stephan, Swift, Ormond Stone, and other observers had been incorporated—to the Council of the Royal Astronomical Society; but the Council, considering that the General Catalogue was practically out of print, and that the use of three catalogues and two lists of corrections would be very inconvenient, proposed to Dr. Dreyer that he should prepare from the whole of his materials a single new General Catalogue. This work he has now carried out, and the present catalogue contains 7840 objects, the positions of which have been as thoroughly corrected and revised as the materials available permitted. The epoch of the first General Catalogue, and of D'Arrest's final positions—1860—has been retained, as it is close to the epochs of the great star-charts of Argelander, Schönfeld, Chacornac, and Peters, and nearly all the modern micrometric observations of nebulæ are referred to an epoch but little later. The precessions have been given for 1880, as done by Sir John Herschel, and the descriptions have been carefully revised. The work also contains an index to published figures of nebulæ and clusters, and an appendix giving the places of several new nebulæ discovered by Prof. Safford and Mr. Swift, but published too late to be incorporated in the catalogue itself. These are added that the volume may contain a complete record of all nebulæ of which the places have been published up to December 1887.

PUBLICATIONS OF DUNSINK OBSERVATORY.—The sixth part of the observations of the Observatory of Trinity College, Dublin, at Dunsink, has just been published, and contains the separate results reduced to 1885·0, and the mean places for 1012 southern stars observed with the transit circle by Dr. Dreyer, the late, and Mr. Rambaut, the present, Assistant Astronomers. These stars are nearly all in the Southern Durchmusterung Belt, between S. Decl. 2° and 23°, and were suggested for observation by Prof. Schönfeld on account either of their proper motion or of discordances between their places as given in different catalogues. A few other stars were observed either at the request of Prof. Peters or Dr. Auwers. The work had been commenced by Dr. Dreyer in September 1881, who continued it until his appointment to the Armagh Observatory in May 1882, and Mr. Rambaut took it up, on succeeding to Dr. Dreyer's position, in November of the same year. Mr. Rambaut gives the probable error of a single observation—most of the stars were observed only once—as ± 0·065s. in R.A., ± 0″·864 in Decl.; the faintness of the objects and their low altitude at meridian passage making observation somewhat difficult. A plate at the end of the volume shows a portion of one of the chronograph sheets, and illustrates a convenient method of making notes whilst at the telescope by sending special signals to the chronograph.

ROUSDON OBSERVATORY.—Astronomical observations have been steadily carried on during the past year at Mr. Peek's private observatory, Rousdon, Lyme Regis. The principal work undertaken, besides transit observations for time, has been the observation of twenty long-period variable stars. It is proposed, so soon as any star has been observed over several complete periods, to publish a memoir with plates showing the variations in the light curve. The record of the Observatory shows that there were 165 good observing nights in 1887, as against 146 in 1886.

β DELPHINI.—Mr. J. E. Gore published two years ago elements for this difficult and interesting binary (NATURE, vol. xxxiii. p. 518), in which he gave the period as 30·91 years, a value fairly corresponding to that found by Dubiago a couple of years earlier, viz. 26·07. Sig. Celoria having been placed in possession of Prof. Schiaparelli's observations made in 1875 and 1886–87, and those of Engelmann made in 1885 and 1886, has re-investigated the orbit, and deduced elements differing widely from these two earlier sets, particularly in the period, which he finds to be a little short of seventeen years (*Astr. Nachr.* No. 2824). If this last orbit be correct, the star has already been watched through nearly a complete revolution. There is, however, a considerable divergency between the recent observations of Schiaparelli and Engelmann, and those of the latter would accord better with a longer period. It is, therefore, much to be desired that astronomers who possess sufficient optical power should give early and careful attention to this star. The following are Sig. Celoria's complete elements:—

$$T = 1868{\cdot}850 \quad \ldots \quad \epsilon = 0{\cdot}09622$$
$$\Omega = 10^\circ{\cdot}938 \quad \ldots \quad a = 0''{\cdot}46000$$
$$\lambda = 220{\cdot}952 \quad \ldots \quad P = 16{\cdot}955 \text{ years}$$
$$\gamma = 61{\cdot}582$$

OLBERS' COMET.—The following ephemeris for Berlin midnight is in continuation of that given in NATURE, vol. xxxvii. p. 234:—

1888.	R.A. h. m. s.	Decl. ° ′	Log r.	Log Δ.	Brightness.
Feb. 11...	17 46 35 ...	6 5·7 S. ...	0·3320 ...	0·3974 ..	0·29
13...	48 57 ...	6 17·2			
15...	51 15 ...	6 28·4	... 0·3394 ...	0·3970 ...	0·28
17...	53 28 ...	6 39·3			
19...	55 36 ...	6 50·0	... 0·3477 ...	0·3962 ...	0·27
21...	57 39 ...	7 0·4			
23...	59 36 ...	7 10·7	... 0·3558 ...	0·3951 ...	0·26
25...	18 1 28 ...	7 20·8			
27...	3 15 ...	7 30·7 S. ...	0·3638 ...	0·3936 ...	0·25

The brightness on 1887 August 27 is taken as unity.

NEW MINOR PLANET.—A new minor planet, No. 272, mag. 13, was discovered by M. Charlois, of the Nice Observatory, on February 4.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *FEBRUARY* 12–18.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 12

Sun rises, 7h. 22m.; souths, 12h. 14m. 28·6s.; sets, 17h. 7m.: right asc. on meridian, 21h. 42·4m.; decl. 13° 46′ S. Sidereal Time at Sunset, 2h. 36m.

Moon (New, February 12, 0h.) rises, 7h. 47m.; souths, 12h. 41m.; sets, 17h. 43m.: right asc. on meridian, 22h. 9·2m.; decl. 13° 19′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	7 54 ...	13 19 ..	18 44 ...	22 47·5 ...	7 39 S.
Venus ...	5 37 ...	9 41 ...	13 45 ...	19 8·2 ...	21 38 S.
Mars ...	22 59* ...	4 20 ...	9 41 ...	13 46·6 ...	8 18 S.
Jupiter ...	2 27 ...	6 42 ...	10 57 ..	16 9·1 ...	20 4 S.
Saturn ...	14 51 ...	22 46 ...	6 41* ...	8 15·4 ...	20 22 N.
Uranus ...	22 4* ...	3 37 ...	9 10 ...	13 3·9 ...	6 4 S.
Neptune..	10 33 ...	18 13 ...	1 53* ...	3 41·6 ...	17 55 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Feb.	h.	
13	9	Mercury in conjunction with and 3° 8′ north of the Moon.
16	12	Mercury at greatest elongation from the Sun 18° east.
17	0	Mercury at least distance from the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Feb. 14,	19 58	*m*
Algol	3 0·9	40 31 N.	,, 12,	22 19	*m*
R Aurigæ	5 8·3	53 28 N.	,, 18,		*M*
R Canis Majoris	7 14·5	16 12 S.	,, 13,	21 35	*m*
			,, 15,	0 51	*m*
S Cancri	8 37·5	19 26 N.	,, 16,	21 43	*m*
S Ursæ Majoris	12 39·1	61 42 N.	,, 15,		*m*
R Boötis	14 32·3	27 13 N.	,, 17,		*m*
δ Libræ	14 55·0	8 4 S.	,, 15,	2 24	*m*
U Coronæ	15 13·6	32 3 N.	,, 15,	0 9	*m*
W Herculis	16 31·3	37 34 N.	,, 18,		*M*
U Ophiuchi	17 10·9	1 20 N.	,, 14,	2 16	*m*
		and at intervals of		20 8	
W Sagittarii	17 57·9	29 35 S.	Feb. 16,	0 0	*m*
U Sagittarii	18 25·3	19 12 S.	,, 16,	4 0	*m*
R Scuti	18 41·5	5 50 S.	,, 18,		*M*
R Lyræ	18 51·9	43 48 N.	,, 16,		*m*
R Aquilæ	19 1·0	8 4 N.	,, 16,		*M*
S Vulpeculæ	19 43·8	27 1 N.	,, 12,		*M*
Y Cygni	20 47·6	34 14 N.	,, 12,	19 56	*m*
			,, 15,	19 50	*m*
δ Cephei	22 25·0	57 51 N.	,, 13,	2 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near 49 Camelopardalis	110°	62° N.	Slow.
From Monoceros	120	5 S.	Slow.
Near ν Herculis	238	46 N.	February 17.
,, σ Ophiuchi	260	3 N.	Swift; streaks.

GEOGRAPHICAL NOTES.

THE French traveller, M. Thouar, who was believed to have perished on his way to the Gran Chaco, has returned to Port Pacheco with his companions. This news was lately sent from Buenos Ayres to Chuquisaca (Sucré).

IN the new number of *Appalachia* Mr. F. H. Chapin describes his ascent of a glacier on Mummy Mountain, Northern Colorado, lying directly north of Long's Peak, and in line with the centre of Estes Park. A single glance at the series of crevasses convinced Mr. Chapin that it was really a glacier, and not a mere accumulation of snow. To the same number Mr. S. H. Scudder contributes a paper on the White Mountains as a home for butterflies.

IN the paper contributed to the Berlin Geographical Society by Dr. H. Meyer on his ascent of Mount Kilimanjaro, he modifies his first statements as to the height which he attained; according to a statement of his companion, Dr. Meyer did not get within 2000 feet of the top.

IN the new Bulletin of the American Geographical Society will be found a useful paper by Mr. A. S. Packard, in which he brings together a *précis* of what was known of Labrador. Accompanying the paper is a good map, in which Mr. Packard has embodied information hitherto unpublished. Dr. Fr. Boas gives the results of his year's sojourn among the Eskimo.

IN the last number of the Proceedings of the Victoria Branch of the Australasian Geographical Society will be found a detailed account of Mr. Cuthbertson's expedition to explore the highlands of British New Guinea. The accompanying map gives a good idea of the nature of the country. Mount Obree was found to be only 8000 feet high, 2000 feet lower than previous estimates.

WE learn from the *Izvestia* of the East Siberian Branch of the Russian Geographical Society (vol. xvii. fasc. 1) that the vertical section of the Angara at its issue from Lake Baikal is 17,920 feet, and that the volume of water discharged from the great Siberian lake reaches 121,353 cubic feet per second. If this outflow were checked, the level of the lake would rise 7 feet in thirteen months.

DR. ROBERT SIEGER contributes to the Geographical Society of Vienna University a paper in which he discusses what information exists as to the changes of level in the African lakes. This shows clearly that for the last ten years at least these have been lowering in level, and, in the case of Tanganyika, to the extent of many feet. The changes which take place are almost entirely dependent on rainfall, and the probability is that there are periods of depression and periods of elevation. It is important that observations should be carried on both in African lakes and African rivers for a period sufficiently long to afford data numerous enough to warrant any conclusion to be drawn.

PROF. EUARD SÜSS, the able author of "Das Antlitz der Erde," recently read a paper to the Vienna Geological Society, on the history of the ocean, which is to some extent supplementary to that work. In this he points out that from the mouths of the Ganges all round the Pacific coasts of Asia and America to Cape Horn, the coasts are outlined by mountain-ranges which close in upon each other in great curves. From Cape Horn, again, all round the Atlantic and the Indian Oceans to the mouths of the Ganges, the coasts are unconnected with mountain-ranges, but are encircled by table-lands or broken mountain patches. We have thus, then, so far as the structure of the ocean basins is concerned, to distinguish between a Pacific and an Atlantic type. As regards the age of the oceans, Prof. Süss concludes from the geological formations that the Pacific is the oldest, next to that the Indian, and last of all the Atlantic. The oceans, he points out, are areas of depression. Each new depression would form a fresh receptacle for water, and so the shore-line of the land would be lowered. Prof. Süss seems to maintain that it is to this, and not to the actual rising of the land, that the elevation of the coast-line in certain regions is due.

MR. J. F. NEEDHAM has been engaged to conduct an expedition from Sadiya to the Hukeng Valley, and thence to Bhamo on the Upper Irrawady. His previous achievements in the Abor Hills, and the country lying between the Brahmaputra and the Zayal Chu, and his success in conciliating the unfriendly tribes on that frontier region, marked him out for selection as the proper officer to conduct the present mission.

THE new part (Nos. 133–34) of the *Zeitschrift* of the Berlin Geographical Society is mainly occupied with Dr. W. Sievers's account of the results of his exploration of the Sierra Nevada of Santa Marta in the north-east of the United States of Columbia, an excellent large-scale map accompanying the number. A considerable section of the paper deals with the geology of the region, after which Dr. Sievers treats of the surface formation, altitudes, climate, vegetation, and agriculture, the land-snails population.

NEWS from Victoria, in the Cameroons, states that the African traveller, Dr. Zintgraff, started for Rio del Rey in the steamer *Nachtigal*, accompanied by thirty porters. He is on his way to the Elephant Lake in order to establish a scientific station. The other half of the Expedition, under the command of Lieut. Zeuner, is to proceed up the Mungo River to Mundame, to reach the Elephant Lake from that part.

OUR ELECTRICAL COLUMN.

IF a platinum plate be immersed in a porcelain or glass vessel containing dilute sulphuric acid, and another similar plate be immersed in another vessel containing caustic potash solution, then if the two vessels be connected by a siphon tube or a cotton wick, a current will be set up, but which rapidly diminishes owing to the polarization of the metal plates by the deposition of oxygen and hydrogen upon them. Becquerel removed the hydrogen by using nitric instead of sulphuric acid, and increased the current considerably. Dr. Alder Wright and Mr. C. Thomson (Royal Society, February 2, 1888) have been examining this form of battery, and have found many other acids which act in the same way, such as potassium permanganate, potassium bichromate, potassium ferricyanide, and bromine dissolved in sulphuric acid, ferric chloride, hydrochloric acid and chlorine. Moreover, they have removed the oxygen by using a concentrated solution of sodium hyposulphite made strongly alkaline with caustic soda, strong caustic soda with pyrogallol, cuprous chloride, ferrous sulphate, and ammonium chloride dissolved in ammonia. They also found the quantity of oxygen

and hydrogen evolved exactly proportional to the current passing. If a silver voltameter were included in the circuit, for every milligramme-equivalent (108 milligrammes) of silver deposited, 1 milligramme-equivalent of hydrogen occupying 11·2 cubic centimetres and 8 milligrammes of oxygen occupying 5·6 cubic centimetres at 0° C. and 760 millimetres, were liberated.

ALTHOUGH Sir William Thomson did not publish any electrical theoretical work in 1887, he perfected during that year his practical electrical measuring instruments. They are in use at the Grosvenor Gallery central station in London. There are no more beautiful or accurate instruments in the world, and they reach over an enormous range both of potential and of current measurement. They were admirably illustrated and described in *Industries* of January 27 by Prof. Fleming.

HERTZ (*Wiedemann Ann.* 1887), has shown that the ultra-violet rays have an influence on the passage of sparks. E. Wiedeman and H. Ebert have been repeating and verifying his experiments. The effect of light falling on the spark region was to lower the potential required to produce it. If a succession of sparks be sent, and a telephone be used, the effect of light falling on the sparks was to change not only the note but the whole character of the sound heard in the telephone. If a Geissler's tube were used, an intermittent and irregular discharge became steady and continuous. The effect was evident only on the negative pole.

IT is known that the magnetic qualities of iron diminish considerably when raised to 525° C. (red heat), but iron remains magnetic up to 650° C. Nickel loses its magnetic properties suddenly at 300° C. Lodeboer recently (January 9) read a paper before the Académie des Sciences, in which he showed that with magnetizing forces of 35, 100, and 200 C.G.S. units the iron retains its magnetic properties up to 680° C.; that beyond this temperature it rapidly loses them; that at 750° C. they scarcely exist, and at 770° C. they entirely disappear, to reappear only on cooling. It is known that the specific heat of iron undergoes a change of condition between 660° and 720° C., and the coincidence of these two changes is very interesting.

THE treatment of sewage by electricity is, it seems, likely to receive a practical test at the Metropolitan Board of Works' outfall at Crossness. Mr. Fewson, of Buckingham, made some experiments in this direction at Wimbledon last summer, and now Mr. W. Webster is about to do the same thing at one of the large tanks on the Thames. The electric current is said to have a wonderful disinfecting and purifying influence. The evolution of gas stirs up the liquid, the nascent oxygen is brought into rapid contact with the impurities and reduces them, precipitation is expedited, and the whole cleansed. It is to be hoped that the cost will not swamp this new and useful field for electricity.

THE extraordinary rise in the price of copper has attracted much attention to the use of iron for lightning conductors. Prof. Silvanus Thompson advocates iron in preference to copper under all circumstances. Iron is much used by the War Department to protect magazines. Dr. L. Weber recommends it even in a solid form rather than as a stranded rope, but the latter form is much more portable and workable; moreover, Prof. Hughes showed it to be less subject to self-induction than a solid rod—an obstruction not to be neglected. Iron conductors are stronger, much cheaper, less easily fused, and less liable to theft than copper. There can be no objection to the use of iron.

THE electro-deposition of aluminium has attracted much attention since the introduction of the Cowles process. Herman Reinbold has proposed the following solution, with which he has obtained good but small results: alum 50 parts, water 300 parts, aluminium chloride 10 parts. This solution is heated to 200° F., and after cooling 39 parts of potassic chloride are added.

THE INSTITUTION OF MECHANICAL ENGINEERS.

THIS Society held its forty-first annual general meeting in the theatre of the Institution of Civil Engineers on Thursday and Friday of last week. After the Annual Report had been presented and accepted, Mr. John Richards' paper "On Irrigating Machinery on the Pacific Coast" was read and discussed. The need of irrigation in this district arises from three causes: the lack of rain, which ceases altogether along the coast in summer-time; the want of surface-water; and the free percolation into the sandy soil beneath. The whole of the land in the country, excepting the low-lying sedimentary plains near the mouths of the rivers, and around the Bay of San Francisco, where water reaches the surface by capillary saturation, requires irrigation. Nearly all the land upon which water can be led, either by training small mountain streams, or by leading long canals from the rivers, has been occupied, so that the only remaining resource for getting water will be by lifting it from the rivers or the gravel strata by machinery. The paper is descriptive of the various pumps and hydraulic rams employed, and was illustrated by means of thirty-five figures.

Mr. William Geipel's paper "On the Position and Prospects of Electricity as applied to Engineering" refers to those branches of electric engineering which involve the employment of considerable power, and are in some way or other connected with the use of dynamos. They comprise electric transmission and distribution of power, and electric lighting, locomotion, and metallurgy.

In the author's opinion the transmission and distribution of power by electricity will occupy in the near future most of the attention of the electric engineer. Owing to its simplicity, the ease with which an electric motor can be applied to any purpose requiring power, and its high efficiency, it is certainly an approach to an ideally perfect system of transmission. In the United States great strides have been made in the applications of electric motors, which already rival those for lighting purposes. One of the great advantages of these applications is due to the low efficiency of belts and shafting where high speed is required and the demand for power is variable. By getting rid of shafting the necessity for additional stability in buildings is obviated, and constant lubrication is done away with. The distribution of power by electricity from a central station to small users can be effected from the same mains and generators as are used for electric light purposes; as to whether gas through the medium of gas engines or electricity by means of electric motors should be used, will become entirely a question of economy and convenience. On the one hand the electric motor can be started and stopped with the greatest ease, it requires little attention, occupies little space, and can be placed anywhere, while against the use of the gas engine, the author brings forward its irregularity of speed owing to the intermittent impulse and the wear and tear in the valves and working parts. Shunt motors, which are now almost exclusively used, possess a practically perfect power of self-control, not only over their rate of speed with varying load, but over the energy absorbed, for they help themselves, as it were, to only such an amount of energy as will enable them to deal with the work imposed upon them. Another advantage in shunt motors, first pointed out by the late Sir William Siemens in 1880, is that they act as generators when themselves driven by any extraneous power, without any complication of the switch gear required with series motors. The author refers to various installations which have already taken place in Europe and America, which are paying their way, whilst at the Falls of Niagara plant is being put down to distribute power obtained from the Falls to neighbouring towns, including Buffalo, which is twenty miles distant; the amount of power is stated at 15,000 h.p., of which 10,000 h.p. is contracted for at £3 per h.p.

Electricity has been applied with efficiency in collieries for underground hauling, pumping, ventilating, and drilling; in ship-yards and similar works it has been proved to be a suitable and economical means of transmitting power for riveting, drilling, &c.

In its application to the transmission of power to great distances, electricity is found to be more economical than either hydraulic, pneumatic, or wire-rope transmission, and comparative tables are given showing the first cost of plant per horse-power transmitted, and also the working cost per horse-power transmitted per hour. For a distance of 22,000 yards the cost of installation for the transmission of 100 h.p. is £87, £310, £192, and £162 per h.p. for electric, hydraulic, pneumatic, and wire-rope transmission respectively; whilst the cost per h.p. transmitted per hour is 4·08, 6·84, 4·50, and 9·73 pence.

Amongst many interesting applications, that made by the Marquis of Salisbury at Hatfield may be specially referred to. The River Lea is utilized to generate electricity by means of turbines, the electricity being transmitted to the house and over the estate for a variety of purposes. The motors at the house

drive pumping and ice-making machinery and an air-propeller fixed in the roof for ventilating; on the farm the motors are used for elevating hay and corn sheaves to the top of the stacks, for thrashing, for cutting rough grass with a chaff-cutting machine for ensilage, in fields extending to a distance of two miles, for grinding corn, &c., to make fodder, and for other purposes. The motors have also been used for pile-driving, for making coffer-dams where necessary in the river, and also for dredging the river and clearing it of weeds, and for pumping the town sewage into a tank at the height of thirty feet for irrigation. The conductors are carried overhead on poles about the farm and underground in wooden troughs to the house. The practical methods employed for electric locomotion—being those of a third insulated rail, an overhead conductor, an underground insulated conductor, and storage batteries—are described, and examples of the application of all are given. To the first belong the Portrush Railway, and Besbrook and Newry Tramway; to the next the electric railway at Moedling, near Vienna, and the Frankfort-Offenbach railway. This plan has been most largely adopted in America, where there are probably not far short of one hundred electric railways at work and projected. Of the underground conductor, the most important example is the electric tramway at Blackpool, while storage batteries are being employed on the North Metropolitan Tramway in London. The ordinary rails have been used as conductors in the short electric railway at Brighton, where the expenses amount to twopence per car-mile.

The plan of transporting material in skips on overhead wire-ropes by means of electricity, introduced under the name of telpherage by Prof. Fleeming Jenkin, has been employed with success for two years past at Glynde, near Lewes, for transporting clay to the railway over a distance of 1600 yards, and is applicable for use in places where material has to be conveyed across hilly districts. In the author's opinion a modification of this plan might be advantageously applied to alleviate the heavy street traffic in our larger cities.

The author considers the question of electric lighting under the three aspects of comfort, convenience, and economy. As regards the first two, electric lighting has the advantage over other systems; whilst as regards cost, although electric lighting, and especially incandescent electric lighting, is still heavy, yet for lighting main streets and railway stations, or other places where concentrated light is required, the arc light is cheaper than gas. As its use extends, the cost of working becomes reduced. Thus in the Waverley Station, Edinburgh, on the North British Railway, thirty-three arc lamps, with 41,884 lamp hours, cost 2·77 pence per lamp hour from July to December 1884; whilst in 1886, thirty-nine arc lamps, having 55,068 lamp hours, cost 1·79 penny per lamp hour.

The cost of incandescent lighting is especially variable, and affected by the local conditions of the installation. The chief of these are the average number of hours of lighting each lamp, and the average distance of the lamps from the generating station. Where conditions are favourable, incandescent lighting can already compete with gas. Messrs. George Jager and Sons' yearly cost of lighting their sugar refinery at Leith is given as an example, it having been £347 with gas and £204 with incandescent lamps. The author draws special attention to the circumstance of the much larger application of electricity to lighting in the United States as compared with this country. In the United States there is hardly a city or town of 20,000 inhabitants which has not a central station for arc or incandescent lamps; and many towns of 3000 to 4000 inhabitants are also supporting them.

The efficiency of dynamo machines being as high as 95 per cent., and there not being much likelihood of material improvement in steam engines, the author draws attention to the importance of improving the lamps by making them with a higher resistance and greater efficiency, the voltage having a great effect on the cost of working distant lamps. Transformers, by means of which high tension currents of electricity, sent from a distant generating station along a small conductor with comparatively small percentage of loss, can then be converted into low tension currents for the supply of ordinary incandescent lamps, are receiving a large amount of attention, the loss by conversion being as low sometimes as 5 per cent. Efforts are also being made to introduce the system of secondary batteries, charged in series by a high tension current, and discharged in parallel circuit, and if it can once be demonstrated to be economical, there would be a large field of application. At Leamington an extensive central station is now at work, the cost of the undertaking being £30,000; while the Bradford Corporation have recently voted a sum of £15,000 for erecting a central station in their town. Both these are instances of direct supply without transformers or secondary batteries. Electric metallurgy is a branch of electric engineering to which attention was first drawn by the late Sir William Siemens, whose death occurred before he had perfected his invention. The electro-chemical separation of ores on a commercial scale by the electric furnace has been recently put to the test, chiefly in obtaining aluminium from conundrum. The furnace designed by Prof. Mabery is built of fire-brick and lined with powdered charcoal; electricity is conducted to the ore by carbon rods, meeting near the centre. The ore mixed with charcoal and granulated copper surrounds and covers the carbons; the furnace is closed with a layer of charcoal and a lid lined with fire-brick. A current of 50 volts electromotive force is supplied and melts the metal around the electrodes, which are moved apart gradually until the whole is melted. The conundrum becomes gradually deoxidized, the aluminium combining with the copper, while the oxygen with the carbon escapes as carbonic oxide, about five hours sufficing to complete the reduction. Aluminium, being only one-third the weight of iron, and possessing great strength, its production at a cheap rate would probably cause a revolution in engineering construction.

The meeting was presided over by Mr. E. H. Carbutt, the President, who was re-elected to the chair, whilst Sir Douglas Galton, K.C.B., was the new member elected on the Council. The meeting was as usual of a very successful character.

THE NATIONAL SMOKE ABATEMENT INSTITUTION.[1]

In presenting the Report to the members for the year 1887, the Council consider it desirable to reprint from the Memorandum of Association the objects for which the Institution was established. These are the following:—

To promote the abatement of coal smoke and other noxious products of combustion in cities and other places, in order to render the atmosphere as pure and as pervious to sunlight as practicable. To check the present serious waste of coal, and the direct and indirect loss and damage accompanying the over-production of smoke and noxious products of combustion. To continue, organize, and extend the public movement inaugurated and hitherto carried on by the Smoke Abatement Committees (otherwise known as the Joint Committees for Abatement of Smoke, appointed by the National Health Society and Kyrle Society of London, and the Smoke Abatement Committee of Manchester), and to take up and proceed with any work undertaken or commenced by such Committees. To advance the aforesaid objects by promoting and encouraging the better and more economical use of coal and coal products, and the selection of suitable fuel, as well as general improvement in the various modes of obtaining, applying, and using heat and light for domestic and industrial purposes. And in connection with such objects to obtain and provide such buildings, appliances, and assistance as may be deemed expedient. And without prejudice to the advancement of the objects aforesaid by other means to advance the same by the following means more particularly:—

(*a*) By calling public attention to the serious pecuniary loss and injury, to the health and comfort, which arise from coal smoke, and from defective heating, ventilating, and lighting arrangements.

(*b*) By stimulating, assisting, and encouraging inventors, manufacturers, traders, and others to bring forward, develop, and perfect new or improved fuels, substances, methods, and appliances for the generation or application of heat or light, and for consuming or lessening the production of smoke and noxious products of combustion.

(*c*) By conducting practical trials of fuels, apparatus, and systems connected with the generation or application of heat or light, and causing reports to be made thereon for the guidance, assistance, or information of inventors, traders, intending users, and the public generally.

(*d*) By granting awards, certificates, medals, or prizes in connection with approved fuels, methods, or apparatus.

(*e*) By establishing, or assisting in establishing, public exhibitions, either periodical or otherwise, of appliances pertaining to heating, ventilating, or lighting.

[1] Report of the Council for the year 1887.

(*f*) By collecting and recording statistics and information, and making, assisting, or encouraging experiments or researches as to the effects upon the atmosphere, and upon life, health, and property of the use of coal and other fuels and means employed or to be employed in connection with heating or lighting; and by printing, publishing, and circulating any such statistics or information, including the intended report of the Committees aforesaid, or any similar composition or literary work.

(*g*) By imparting information, instruction, and assistance to local authorities, manufacturers, workmen, householders, servants, and the public generally whether by means of lectures, demonstrations, pamphlets, written articles, or otherwise in relation to the subject of smoke prevention or abatement.

(*h*) By joining or concurring with any other institution, society, or persons, in doing or causing, or procuring to be done, any of the things aforesaid.

To promote the abatement of noxious vapours arising from manufactures or manufacturing processes, and to resort to and use for that purpose powers and means analogous to those hereinbefore contemplated with reference to Smoke Abatement and any other reasonable means. For all or any of the purposes aforesaid, either alone or in conjunction with others, to promote legislation and parochial and other regulations, and to assist in the enforcement thereof, and of any existing or future legislative, parochial, or other regulations.

In reporting upon the business transacted by the Institution during the past year, it is essential that the members should be reminded of the urgency for further legislation on the subject of smoke prevention.

The Institution has been in communication with the medical officers of health and chief constables throughout the country, and the most valuable information obtained with reference to the working of existing by-laws is given as supplements Nos. 1, 2, 3, 4, 5, and 6, to a paper on Smoke Abatement, read at the Bolton Congress of the Sanitary Institute. These supplements are published in the Transactions of the Sanitary Institute of Great Britain, and by reference to them it will be seen that the municipal authorities of Liverpool are much more alive to the necessity of prosecuting offenders against the Smoke Abatement Acts than the authorities in any of the other places from which reports have been obtained.

By comparison with the Report issued by the Commissioner of Police for the Metropolis of 1886, it will be seen that the number of cases in which fines were imposed in Liverpool was 545, whereas the number of convictions in the metropolis amounted only to 82. It might further be noted, however, in respect to the penalties imposed, that the average of all the fines in Liverpool was 19*s.* 11¼*d.* The average in London was £1 17*s.* 5*d.* The inadequacy of the fines imposed is a serious obstacle in dealing with police prosecutions, and the fines have little effect, if any, in the prevention of smoke, in consequence of the amount of the penalty being so disproportionate to the financial positions of the persons on whom they are imposed.

During the year attention was prominently called by Lord Stratheden and Campbell to the provisions in the Bill introduced by him to the House of Lords "To amend the Acts for abating the nuisance arising from the smoke of furnaces and fire-places within the Metropolis," and resulted in a Select Committee being appointed to consider the terms of the Bill, and to report to the House of Lords. The minutes of evidence were laid before the House of Lords on the 15th of July, 1887, and the published Report contains much valuable information with respect to the working of the Smoke Abatement Acts:—The nuisance created by steamers on the Thames; the necessity for extension of the metropolitan area to be within the Acts; the necessity for controlling the emission of smoke from club-houses, hotels, private residences, and other buildings not within the scope of the existing Acts; the usual course followed by the police in instituting prosecutions; a return showing the number of police employed in carrying out the Smoke Nuisance Abatement Acts; the effect of the increase of smoke on the health of the people, and the advantages from a sanitary point of view to be derived by the prevention of smoke; also particulars regarding the commercial advantages to be derived by the consumption of smoke; particulars of the methods which might be adopted for the complete combustion of fuel in domestic grates; and generally, a great mass of information dealing with the subject laid before the House of Lords by the following gentlemen: Mr. W. R. E. Coles, the engineer appointed by the Home Secretary to examine furnaces in the metropolis; Mr. James Edward Davis, of the Home Office, legal adviser to the Commissioners; Mr. Charles Cutbush, Superintendent of Police; and Mr. Ernest Hart, Chairman of Council of the National Smoke Abatement Institution.

By reference to the Police Orders and Regulations reprinted at the end of this Report, it will be observed in paragraph 36 that hotel-keepers in the metropolis not using steam-engines can only be proceeded against under Section 19, Sub-Section 3, of 29 and 30 Vict., cap. 90, and be guilty of an offence under that Section. In consequence of this Act of Parliament, Section 19, Sub-Section 3, stipulates that any chimney (not being the chimney of a private dwelling-house) sending forth black smoke in such quantity as to be a nuisance is exempt from the working of the Act and it is left to the justices to dismiss the complaint if they are satisfied such fire-place or furnace is constructed in a manner to consume, *as far as practicable*, all smoke arising therefrom, but it does not state any standard smoke shade or any degree to be fixed upon as the limit, and therefore the justices may or may not convict at their option.

The purpose of Lord Stratheden and Campbell's Bill is to prohibit or regulate the emission of smoke from any building, no immunity being granted to hotels, club-houses, or domestic fire-places now exempted from the existing Acts. The effect of the general evidence brought before the Select Committee of the House of Lords was a resolution to await the results of the further operation of the existing Acts, the purpose and intention of which should, it was held, be more fully carried into effect.

The Council invite the careful consideration of members to the necessity for legislation, and on an early date will arrange for a series of meetings to be held, at which it is expected the sanitary inspectors from the leading provincial towns will assemble, in order to compare and suggest revisions for the existing municipal by-laws, as well as for the purpose of drafting propositions to submit to the authorities on the subject of improved legislation in the metropolis.

The Council having considered the desirability of taking the first opportunity for conducting simultaneous tests of the furnaces of a large number of steam-boilers under equal conditions, thought that such an opportunity might be offered at the forthcoming Exhibition to be held in Glasgow. They accordingly directed the Secretary to write to the Lord Provost of Glasgow, laying the outlines of their scheme before him, and suggesting its adoption by the Exhibition authorities. Briefly stated, the proposal was: That the whole range of boilers to be used for working the machinery of the Exhibition should be erected in such a manner that each boiler should have its setting and chimney independent of the other boilers, so that the several systems of stoking and arrangement of furnaces could be fully tested under identical conditions of fuel, atmosphere, and time; while the results as regards smoke would be evident to the public.

It is to be regretted that the authorities in charge of the Glasgow Exhibition have not been able to see their way to co-operating with the Smoke Abatement Institution as proposed. Simultaneous tests on such a large scale have never previously been made. Many tests of great value have been made on furnaces, but these have been at separate times, and under different atmospheric conditions, and the results, however favourable in themselves, have been incapable of classification for comparison. The proposal of the Council, if adopted, would have supplied what is wanting by making these tests of several boilers at the same time, and under the supervision of an impartial body.

With reference to this subject, a correspondence has taken place with Mr. Fletcher, Chief Inspector under the Alkali Acts, as he has in preparation a report upon the injurious effect of the impurities of the air and water on the Clyde. Mr. Fletcher was asked to furnish a copy of the report, but replied that it was in the hands of the Secretary for Scotland. Application has been made to the Secretary for Scotland, and attention drawn to the importance of the series of tests which the Council proposed. The Secretary for Scotland in his reply stated that Mr. Fletcher's report has not yet been brought before Parliament, and with respect to the testing of the boilers, said that he would inform the Committee of the Glasgow Exhibition that he considers the suggestion of the Institution to be deserving of consideration and adoption.

At a meeting of the Institute of Engineers and Shipbuilders in Scotland, on the 8th of December, a very comprehensive paper

was read by Mr. George C. Thomson on "Smoke," and in the discussions which followed, Mr. W. R. W. Smith, Chairman of the Health Committee of the Glasgow Corporation, urged upon the members present the desirability of doing all in their power to secure that at the forthcoming International Exhibition in Glasgow each of the boilers be supplied with a separate chimney, so that a series of exhaustive trials may be made with mechanical stokers, &c., and other means for the purpose of showing what might be done in the way of smoke prevention.

With reference to the subject of testing, the Committee are of opinion that arrangements should be made as soon as possible for obtaining the use of three testing-rooms for testing stoves, grates, and ranges, the rooms being conveniently accessible for such articles, and having gas connections under command. The tests made in these rooms, under the same conditions of chimney and cubic capacity, would then become of greater comparative value than tests made in independent rooms.

Arrangements will be made as soon as practicable for procuring such accommodation for testing, and also for providing the necessary instruments used for testing; and as the system develops, attention will be given to the establishment of a chemical laboratory, the analysis of gases, and testing-rooms for testing-apparatus incidental to the work of the Institution.

SCIENTIFIC SERIALS.

American Journal of Science, December 1887.—On the destruction of the passivity of iron in nitric acid by magnetization, by Edward L. Nichols and W. S. Franklin. From the experiments described in this paper, which was originally read before the Kansas Academy of Science, November 1885, it appears that the action of the magnet tends to lower the temperature of transition to the active state, and that the intensity of the magnetic field necessary to convert passive into active iron at a given temperature increases rapidly with the concentration of the acid. An account is promised of further researches offering a satisfactory explanation of the manner in which the chemical behaviour of iron is modified, and its passivity destroyed in the magnetic field.—On a method of making the wave-length of sodium light the actual and practical standard of length, by Albert A. Michelson and Edward W. Morley. The preliminary experiments recently carried out according to the method here proposed seem to confirm the anticipation that it would furnish results more accurate than any of those hitherto suggested. The apparatus for observing the interference phenomena is the same as that used in the experiments on the relative motion of the earth and the luminiferous ether.—The work of the International Congress of Geologists, by G. K. Gilbert. This is a reprint of an address delivered before the Section of Geology and Geography of the American Association for the Advancement of Science at the New York meeting, August 10, 1887. It deals largely with a revised system of geological terminology, the substance of which has already been published. The question of geological coloured maps is also considered, and practical suggestions made for their greater efficiency and economy.—On the existence of certain elements together with the discovery of platinum in the sun: contributions from the physical laboratory of Harvard University, by C. C. Hutchins and E. L. Holden. These investigations, carried on with Prof. Rowland's magnificent diffraction grating, deal with cadmium, lead, tin, silver, potassium, and several other elements, including platinum, the presence of which in the solar atmosphere is here for the first time determined. Between 4250 and 4950 were found sixty-four lines of platinum, sixteen of which agree with the solar lines.—The flora of the coast islands of California in relation to recent changes of physical geography, by Joseph Le Conte. A careful study of these insular groups, at present from 20 to 30 miles distant from the coast, shows that they at one time formed part of the mainland, from which they were undoubtedly separated during the Quaternary period. That they still formed part of the continent during later Pliocene times is shown by the remains of the mammoth found on Santa Rosa, one of the largest and furthest off of the whole group.—A new instrument for the measurement of radiation, by C. C. Hutchins. The instrument here described and illustrated presents great advantages over the thermopile as an accurate measurer of radiations. It is much more sensitive and requires no longer time to return to zero than for the galvanometer needle to come to rest. A lighted match at 6 feet drives the needle round to its stop.—Mineralogical notes, by George F. Kunz. Descriptions with analyses are given of a rhodochrosite from Colorado, of crystals of hollow quartz from Arizona, of hydrophane from Colorado, and of a remarkable silver nugget weighing 606 ounces from the Greenwood mines of Michoacan, Mexico.

January. — The speed of propagation of the Charleston earthquake, by Prof. Simon Newcomb and Captain C. E. Dutton. A careful comparative study of the reports from all parts of the disturbed area shows a general average speed of $3 \cdot 214 \pm 0 \cdot 072$ miles, or 5171 ± 116 metres per second.—History of the changes in the Mount Loa craters, Hawaii; Part 1, Kilauea, by James D. Dana. The first paper embraces the whole period from 1823 to 1886, during which there appear to have been at least eight discharges from Kilauea. The general dynamical conclusions are that the cycle of movement is simply (1) a rising in level of the liquid lavas, and of the bottom of the crater; (2) a discharge of the accumulated lavas down to some level in the conduit determined by the outbreak; (3) a down-plunge of more or less of the floor of the region undermined by the discharge. It is further shown that Kilauea is a true basalt volcano in its normal state, the rock material being dolerite or basalt, and the heat sufficing for the perfect mobility of the lavas.—The analysis and composition of tourmaline, by R. B. Riggs. The methods of analysis are described, with results for various specimens from different parts of North America and Brazil. The general inference is that there are three types, lithia, iron, and magnesia tourmaline, with an indefinite number of intermediate varieties, iron appearing to be the connecting link between the whole series. The special formulas of the three distinct types are:—

(1) Lithia: $12SiO_2$, $3B_2O_2$, $4H_2O$, $8Al_2O_2$, $2(NaLi)_2O$.
(2) Iron: $12SiO_2$, $3B_2O_3$, $4H_2O$, $7Al_2O_3$, $4FeO$, Na_2O.
(3) Magnesia: $12SiO_2$, $3B_2O_3$, $4H_2O$, $5Al_2O_3$, $\frac{2.8}{3}MgO$, $\frac{2}{3}Na_2O$.

—On the different types of the Devonian system in North America, by Henry S. Williams. It is shown that in North America the Devonian system offers at least four distinct types in four corresponding areas, blending somewhat at their borders, but in their central parts presenting marked peculiarities. The four areas are: (1) Eastern Border, mainly in Northern New England; (2) Eastern Continental, including New York, thence southwards to West Virginia and north-westwards to Canada West and Michigan; (3) Interior Continental, chiefly Iowa and Missouri, extending northwards probably to the Mackenzie basin; (4) Western Continental, in Nevada and conterminous States.—On the law of double refraction in Iceland spar, by Charles S. Hastings. The general inference from these researches is that Huyghens' law of double refraction in uniaxial crystals is probably true to less than 1 part in 500,000, and consequently that there is no known method by which any error in it can be detected by observation alone.—In the Appendix, Mr. O. C. Marsh describes a new genus of Sauropoda and other new Dinosaurs from the Potomac formation; also a new fossil Sirenian from California.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 19.—"Notes on the Spectrum of the Aurora." By J. Norman Lockyer, F.R.S.

I exhibited to the Society on November 17, 1887, a tabular statement showing the bright lines seen in the spectra of various celestial bodies, and I also gave those recorded in the spectrum of the aurora, showing many remarkable coincidences.

I now find that the connection is closest between the auroral spectrum and that of stars III.*a*, and, in anticipation of a subsequent communication of details, I send on the accompanying table, showing the origin of Dunér's bands, so far as I have at present made them out, and their connection with the spectrum in question.

The individual observations which I have used in the table are those collected by Mr. Capron and Mr. Backhouse (NATURE, vol. vii. pp. 182 and 463).

TABLE OF WAVE-LENGTHS OF AURORAL LINES.

Barker...	431	470	482	502	517				562	623
Smyth									558	635
Zöllner										628
A. Clerke			485				532			
Herschel							531			
Backhouse	430			501	516·5		532			606
Lord Crawford			more ref. than F			523				
H. R. Proctor			,,							
Vogel		469								
Ellery										635
O. Struve									554	
Ångström		472				521			556	
Lemström	426	469				525				
German North Polar Expedition									557	
Respighi									557	
Peirce	431	464	486			520	531	545	557	
Probable origin	CH	C (hot)	C (cold)	Mg	C (hot)	Mg	*	Zn† (1)‡	Mn (1)	Fe (1)
Wave-lengths of probable origin	431	474	483	500	516·5	520·1		546	558	615
Dunér's bands			$\underbrace{477\ 485}_{9}$					$\underbrace{550\ 545}_{5}$		$\underbrace{616\ 627}_{2}$
		$\underbrace{460\ 474}_{10}$		$\underbrace{495\ 503}_{8}$	$\underbrace{516\ \ 521}_{7}$				$\underbrace{564\ 559}_{4}$	

* Coronal line. † Another probable origin for this in the aurora is 540 Mn. ‡ This means brightest fluting.

Addendum.—The following table shows the above figures in another form and includes the bright lines recorded in γ Cassiopeiæ:—

Aurora.	Dunér's bands.	Bright lines in γ Cassiopeiæ.	Probable origin.	Wave-length of probable origin.
431	...	...	CH	431
474	460–474 (10)	...	C (hot)	474
...	...	462·3	Sr	460·7
483	477–485 (9)	...	C (cool)	483
500	495–503 (8)	499	Mg	500
516·5	516–521 } (7)	516·7	C (hot)	516·5
520·1	... }	...	Mg	520·1
531	...	531	Coronal line	
...	...	542·2	Mn	540
545	545–550 (5)	...	Zn (1)	546
558	559–564 (4)	555·7	Mn (1)	558
...	585–595 (3)	586	Mn (2)	586
615	616–627 (2)	616	Fe (1)	615
635	...	635·6	*	...

Geological Society, January 25.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read :—On *Ailurus anglicus*, a new Carnivore from the Red Crag, by Prof. W. Boyd Dawkins, F.R.S. The specimen described is a small fragment of the right lower jaw with the last three molar teeth in position, and belongs to the Crag collection of the Yorkshire Philosophical Society. It differs in a marked degree from all fossil European Carnivores, and presents no important points of difference when compared with a series of jaws of recent *Ailurus*. The author gave a description of the fossil, and comparison of it with *Ailurus fulgens*, and also a table giving the comparative measurements of the teeth and jaws of the fossil and of recent *Ailuri*. The species from the Crag was a more powerful animal than any recent *Ailuri* in the British Museum. The paper concluded with a notice of the range of *Ailurus* in space and time. After the reading of this paper the President remarked that seldom had a fact of greater interest in its bearing upon geographical distribution in past times been brought before the Society. Some comments on the paper were also made by Mr. Lydekker, Prof. Seeley, Mr. Newton, and Mr. Blanford.—A contribution to the geology and physical geography of the Cape Colony, by Prof. A. H. Green, F.R.S.—On two new Lepidotoid Ganoids from the early Mesozoic deposits of Orange Free State, South Africa, by Mr. A. Smith Woodward. The results presented in Prof. Green's and Mr. Woodward's papers were discussed by the President, Prof. Rupert Jones, Mr. Blanford, Dr. Geikie, Mr. Clement Reid Prof. Hughes, and Mr. Irving.

* This line is seen as a pretty bright line in the spectrum of the Limerick meteorite, but its origin has not yet been determined, although comparisons have been made with most of the common elements. So far, it has not been observed in any other meteorite.

Royal Microscopical Society, January 11.—Rev. Dr. Dallinger, F.R.S., President, in the chair.—The President referred to the death of Dr. Arthur Farre, a former President of the Society, and one of its earliest Fellows.—Prof. C. Stewart exhibited specimens of *Thecalia concamerata*. In this genus the female shell exhibited a peculiarity which was quite unique. As age advanced the mantle became folded back upon itself in a very curious manner, and simultaneously with this there occurred a similar in-folding of the contiguous portions of the shell by which two depressions were produced, forming a fusiform chamber when the two valves came together. In this cavity the embryonic shells were found.—Edmond's automatic mica stage rotating by clockwork was exhibited and described.—Mr. A. W. Bennett gave a *resumé* of his paper on freshwater Algæ of the English Lake District, with a description of a new genus of *Capsulococcus* and five new species, in continuation of his previous communication on the same subject.—Dr. G. Gulliver read a paper on *Pelamyxa palustris*. The large size of this amœboid organism had enabled it to be cut into sections, and the granulated structure of its exoplasm thus revealed was described. As regarded its classification, it was thought that ultimately it would be found to have a nearer relationship to the true Heliozoa than to the more lowly Amœbæ.—Mr. E. M. Nelson handed round for inspection two photographic positives ; one of *Amphipleura pellucida*, and the other of a fungus growth which attacked calcareous sand, as described by Mr. J. G. Waller in the Journal of the Quekett Microscopical Club, i. p. 345. This object presented some photographic difficulty because of its non-actinic colour. With regard to the other he remarked that in resolving diatoms with oblique light it was essential to decide whether they intended to focus upon the real surface or upon the optical image produced in a higher plane, in consequence of the double nature of the structure of the valve. In the latter case they would obtain a result such as he exhibited, which was a photograph of the optical image and not of the real diatom.—Mr. Nelson also called attention to a curious optical effect for which at present he was unable to account. In a flat box he had placed a glass positive of *A. pellucida* which was viewed as a transparency through a piece of tube fitted at right angles to the surface. If this was looked at when held towards a surface of light such as an optical lamp-shade or a sunlight gas-burner, the black lines appeared to be slightly smaller than the white lines ; but if it was turned towards a small light at a distance, then the black lines appeared very

large and the white ones were reduced to mere threads. The scale of the photograph showed that the effect was not due to the operation of the first diffraction spectrum, and it was still more curious to note that in the case of another positive taken from the same negative and upon the same scale this optical illusion was not observed.

Anthropological Institute, January 24.—Anniversary Meeting.—Prof. Flower, C.B., F.R.S., Vice-President, in the chair.—The following were elected Officers and Council for the ensuing year:—President: Francis Galton, F.R.S. Vice-Presidents: J. G. Garson, Prof. A. H. Keane, F. G. H. Price. Secretary: F. W. Rudler. Treasurer: A. L. Lewis. Council: G. M. Atkinson, E. W. Brabrook, C. H. E. Carmichael, Hyde Clarke, A. W. Franks, F.R.S., Lt.-Col. H. H. Godwin-Austen, F.R.S., T. V. Holmes, H. H. Howorth, M.P., Prof. A. Macalister, F.R.S., R. Biddulph Martin, M.P., Prof. Meldola, F.R.S., Rt. Hon. the Earl of Northesk, C. Peek, Charles H. Read, Lord Arthur Russell, M.P., Prof. A. H. Sayce, H. Seebohm, Oldfield Thomas, M. J. Walhouse, Lieut.-Gen. Sir C. P. Beauchamp Walker, K.C.B.

Paris.

Academy of Sciences, January 30.—M. Janssen in the chair.—Note on the first volume of the *Annales de l'Institut Pasteur*, presented to the Academy, by M. L. Pasteur. This volume contains the first twelve numbers of a monthly serial established and directed by Prof. Duclaux, of the Sorbonne, and entirely devoted to the progress of the new branch of pathological physiology to which M. Pasteur gives the name of "Microby" or "Microbiology." His remarks were mainly confined to the important memoir by MM. Roux and Chamberland, entitled "Immunité contre la septicémie, conférée par des substances solubles." In this memoir is contained the rigorous demonstration of the far-reaching fact that the septic vibrion, a living ferment analogous to the butyric vibrion, develops soluble chemical products, which gradually act as an antiseptic on the organism itself. These products, introduced in sufficient quantities into the body of the guinea-pig, confer absolute immunity from the deadly attacks of the virus, to which that animal is specially susceptible.—Note on the total lunar eclipse of January 28, by M. J. Janssen. The observations taken at the Observatory of Meudon were mainly directed towards determining a point of telluric spectroscopy connected with the absorption bands of oxygen. They were necessarily of a somewhat preliminary character, and will be continued during future total eclipses of the moon.—Researches on ruthenium, by MM. H. Debray and A. Joly. The paper deals more especially with hyperruthenic acid, its purification, physical properties, behaviour in the presence of water, and under varying temperatures.—An apparatus adapted for experiments at high temperatures in the presence of gases under high pressure, by M. L. Cailletet. For this apparatus, which the inventor has had in use for some years, it is claimed that it enables experimenters to raise substances to temperatures near the fusion of platinum while keeping them in a gaseous atmosphere, the nature and pressure of which may be varied at pleasure.—On double dielectric refraction; simultaneity of electric and optical phenomena, by M. R. Blondlot. These experiments have been undertaken in order to determine whether the double dielectric refraction of a condenser is produced and ceases simultaneously with the charge, or whether there exists an appreciable interval of time either between the production of the electric phenomenon and that of the luminous phenomenon, or between periods of cessation of both phenomena. The conclusion seems to be that, if there is any difference in point of time between these several manifestations, it cannot exceed 1/40000 of a second.—On the laws of chemical equilibrium, by M. H. La Chatelier. It is shown that the numerical laws of chemical equilibrium, such as they are deduced from the two principles of thermodynamics, may be expressed in a very simple way by means of M. Massieu's characteristic function H′, which may be regarded as the true measure of chemical force.—On cinchonigine, by MM. E. Jungfleisch and E. Léger. The authors describe the process of preparation, the chemical properties, and the salts of this substance, whose composition is expressed by the formula $C_{28}H_{22}N_2O_2$.—Persistence of the virus of rabies in dead bodies, by M. V. Galtier. These researches show that the virus retains all its virulence in the bodies of dogs that have been dead seventeen and buried fifteen days. Inoculation from the bulb produces rabies in ten and kills in fifteen days after trepanation.—On the antiseptic properties of naphthol-α, by M. J. Maximovitch. The experiments here described show that, owing to its feebler toxic and stronger antiseptic properties, this substance is in every way superior as an antiseptic to M. Bouchard's naphthol-β.—On the presence of primordial fauna (Paradoxidian) in the neighbourhood of Ferrals-les-Montagnes (southern slope of the Montagne Noire), Hérault: (1) stratigraphic study by M. Jules Bergeron; (2) palæontological study, by MM. Munier-Chalmas and J. Bergeron. Considerable interest attaches to the recent discovery of these organisms by M. Bergeron, for the first time in any part of France. They belong to the earliest forms of the Silurian group, forms which were not known to exist when that group was first established by Murchison in 1835. These first French Trilobites of the primordial fauna, as it was named by Barrande, include some exceptionally fine specimens of the genera Conocephalites and Paradoxides, the latter closely allied to the *P. rugulosus* of Bohemia, and the *P. Pradoanus* common in the Cambrian of Spain.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Astronomical Observations and Researches made at Dunsink, sixth part (Hodges, Dublin).—A Student's Manual of Psychology, adapted from Kirchner by E. D. Drought (Sonnenschein).—The Cardinal Numbers: M. Hopkins (Low).—Civilization and Progress; new edition: J. B. Crozier (Longmans).—Lessons on Prescriptions and the Art of Prescribing; new edition: W. H. Griffiths (Macmillan).—Lehrbuch der Entwicklungsgeschichte des Menschen und der Wirbelthiere, Zweite Abthg.: Dr. O. Hertwig (Jena).—Practical Forestry: C. E. Curtis.—South African Butterflies; two vols: R. Trimen, assisted by J. H. Bowker (Trübner).—Journal of the Society of Telegraph-Engineers and Electricians, No. 69, vol. xvi. (Spon).—Journal of the Royal Statistical Society, December (Stanford).—Annalen der Physik und Chemie, 1888, No. 2 (Leipzig).—Beiblätter der Physik und Chemie, 1888, No. 1 (Leipzig).—Brain, parts 39 and 40 (Macmillan).

CONTENTS.

THURSDAY, FEBRUARY 16, 1888.

KINEMATICS AND DYNAMICS.

An Elementary Treatise on Kinematics and Dynamics. By James Gordon MacGregor, M.A., D.Sc., &c., Munro Professor of Physics, Dalhousie College, Halifax, N.S. (London: Macmillan and Co., 1887.)

THE logical order of arrangement has been carefully attended to in this book: Part I., on "Kinematics," building up a new subject on the foundation of Euclid's axioms in conjunction with the idea of the variables, such as velocity and acceleration, due to the flow of time; while Part II., on "Dynamics," requires three new axioms —Newton's Laws of Motion—to make a fresh start and connect mechanical effects with their causes.

But it is doubtful if the strictly logical order is the best order for the student to make his first acquaintance with a new mathematical subject: the ideas must grow in his brain by accretion round simple fundamental problems. A student would master the present treatise more easily by reading Part II. first, and referring back to Part I. as occasion required, for the explanation of the details of the mathematical calculations. There is nothing to prevent this order of study here, although the author has, from logical considerations, placed the kinematical part first.

One defect of the logical system is that it places some of the most difficult parts of the subject in the way of beginners: for instance, the theory of the change of units, a theory of which the importance can only be appreciated by those who have made considerable progress in the subject.

In Part I., "Kinematics," the treatment is simple and concise, but we should like to see more examples of phenomena on a large scale, such as those of physical astronomy, or even of railway-train problems.

In questions involving the size of the earth (pp. 74 and 80) it is the circumference and not the diameter which should be given in metres, the circumference being 40,000,000 metres, a kilometre being a centesimal minute of latitude. Or, if the size of the earth is given in miles, it is the nautical mile which should be used, the circumference of the earth being $360 \times 60 = 21,600$ nautical miles, a nautical mile being a sexagesimal minute of latitude.

The expression "knots an hour" (p. 60) is irritating to a sailor, as emanating from the engine-room; the proper nautical expression is "knot" simply, a speed of 10 knots being 10 nautical miles an hour.

The formula $\frac{1}{2}v^2 = \frac{1}{2}v_0^2 + as$ is to be preferred to that on p. 34, $v^2 = v_0^2 + 2as$; in all cases the factor $\frac{1}{2}$ should go with the v^2 in the equation of energy, so that the objectionable expression "*vis viva*" may finally be stamped out from all dynamical treatises.

In dealing with rotation, in Chapter V., the author would do well to study Maxwell's geometrical representation of the direction by means of the screw, right-handed or left-handed; and to discard all attempts by comparison with a clock-wise or counter-clock-wise rotation, requiring as these do a specification of the aspect of the plane of motion.

Pure homogeneous strain is analyzed in Chapter VII. as far as is possible by simple geometrical methods; such a strain may be produced by the superposition of three linear strains in directions at right angles to one another. In a linear strain the increment of distance of two points in the line of the strain is properly their *elongation;* while the ratio of the elongation to the original distance is called the *extension*, not the *elongation*, as on p. 167.

In Part II., "Dynamics," we find in Chapter I. the discussion on the units of measurement of weight, mass, and force customary in mathematical treatises, and of the usual unsatisfactory nature. The author, disregarding the vernacular use of the word "weight," defines the weight of a body as the force with which it is attracted by the earth, but is at variance with his own definition in the statement of the majority of the subsequent examples, relapsing into the language of ordinary life. A collection of 500 different ways of spelling the name of the town of Birmingham has been made, and a similar collection could be made from the present treatise of different ways of expressing the simple ideas of the pound *weight* and the pound *force*, to use the ordinary language of practical men. The attraction of the earth on a pound is, in the vernacular, "the force of a pound," not the "weight of a pound," the latter implying what the mathematician likes to distinguish as the "mass of a pound." Thus a mathematical precisionist, to express the simple idea of a force of 10 pounds, to be consistent should call it "a force equal to the weight of the mass of 10 pound weights," the absurdity of which is evident.

Again, in straining after the equation $F = ma$, when using the gravitation unit of force, the mathematician in the F.P.S. (foot-pound-second) system of units is obliged to use the variable unit of mass of g pounds to measure the invariable quantity, the mass of the body; while what he calls the weight of the body, and denotes by w, measuring it in pounds, is, although variable with g, always measured by the same number.

Next we have the equation $w = mg$, the source of all the confusion in dynamical teaching, and only to avoid writing the dynamical equation with gravitation units in the form

$$F = w\frac{a}{g}.$$

This terminology culminates in the solecisms that on p. 477 we must suppose pressure to be measured in poundals on the square foot in hydrostatical problems; and that if the equation $w = mg$ is supposed to be used with absolute units, that the weight of a body is measured in poundals; as if a mathematician asked in a shop for "half a poundal of tea, or tobacco." Ordinary people measure weight in pounds, so that if mass is also measured in pounds, then $w = m$.

It is time now, as Prof. Minchin has pointed out, that "the astronomical unit of mass," defined in § 315, should disappear, and that in all problems of physical astronomy the gravitation constant k should be retained, while m, the mass, is measured in terms of the ordinary units.

Although the author does not allow himself the use of the methods and notation of the Calculus, still he has managed to discuss a number of interesting problems in the dynamics of a rigid body, usually proved by the methods of Analytical Mechanics.

Working under these restrictions, he has given elegant elementary proofs of the chief properties of the common catenary; but here, again, it is time that the equation

should be presented in the form $y/a = \cosh x/a$, using the notation of the hyperbolic functions; which might also be employed with advantage in the statement of the results of the examples on p. 302. Chains of 5000 feet span, and 400 feet versed sine, are in existence, providing striking numerical examples in this part of the subject.

Most of the examples are carefully chosen, but the author by diligent search could easily add more interest to the collection, particularly to the examples on parabolic trajectories, and problems concerning the motion of railway-trains. Ex. 85, p. 499, certainly requires careful revision. The diagrams of the simple machines are of the usual academic nature; the author should consult Prof. Kennedy's "Mechanics of Machinery" for better illustrations, especially of the differential pulley, and of pulley tackle in general. If the differential screw is given (p. 435), why not also the integral screw, which is to be met with more commonly in real life—for instance, in railway couplings, and in the rigging of ships.

Except for the parts criticized above, on the units of weight, mass, and force, the present treatise shows that the author has read with profit and discrimination the most recent treatises on dynamics; he has produced a very useful work, suitable for instruction in technical colleges, and likely also to prove a necessary corrective to the very abstract treatment of the subject of mechanics too common in the character of University instruction.

A. G. GREENHILL.

ATLAS OF THE DISTRIBUTION OF PLANTS.

Atlas der Pflanzenverbreitung. (Berghaus's "Physikalischer Atlas," Abtheilung V.) Bearbeitet von Dr. Oscar Drude. (Gotha: Justus Perthes, 1887.)

THE history of the science of the distribution of plants begins with Linnæus, who was the first to cite systematically the countries and situations in which the plants he described grew. This we find carefully done in the first edition of the "Species Plantarum," published in 1753. No perceptible advance beyond this was made before the appearance of Humboldt and Bonpland's "Essai sur la Géographie des Plantes" in 1805, which work may be designated the real foundation of the science. It was followed in 1823-24 by the Dane, Schouw's "Grundtræk" and "Plantegeographisk Atlas," the latter containing twenty-two maps illustrating the vegetation of the world, and especially the distribution of plants cultivated for food. There is also a German edition of both the "Outlines" and the "Atlas." From this date onward many of the most eminent botanists investigated distribution in connection with classification of plants, notably R. Brown, A. P. De Candolle, H. C. Watson, C. Darwin, A. De Candolle, J. D. Hooker, Edward Forbes, Von Martius, and Grisebach, to say nothing of the younger botanists. But the results of their labours ars still scattered, or at least only partially elaborated; for Grisebach, in his "Vegetation der Erde," deals with the facts from a peculiarly narrow stand-point.

It is true that both Drude and Engler ("Versuch einer Entwicklungsgeschichte der Florengebiete") have attempted something beyond this, but neither, we suspect, regards his work as more than a preliminary effort. The primary geographical divisions of these two writers are essentially the same, though their nomenclature differs; but, considering the complexity of the subject, probably no two persons would agree exactly on these points; yet it is highly desirable that there should be something approaching uniformity in the names of the divisions. Grisebach designates his primary divisions "Gebiete," and Drude his "Reiche"; whilst Engler's four primary divisions are designated "Reiche," and his secondary ones "Gebiete." Let us now briefly examine the main features of Drude's Atlas. Following the most authoritative English writers on zoological and botanical geography, we will call the primary divisions regions, and the secondary divisions sub-regions.

Drude divides the world into fourteen floral regions, and each of these into a number of sub-regions, indicating by lines and dots the overlapping of the elements of contiguous sub-regions. The regions are: (1) Northern, (2) Central Asia, (3) Mediterranean, (4) East Asia, (5) Middle North America, (6) Tropical Africa, (7) East African Islands, (8) Indian, (9) Tropical America, (10) Cape, (11) Australia, (12) New Zealand, (13) Andes, (14) Antarctic.

While agreeing in the main with the foregoing divisions, we cannot but regard some of them as including too, much or too little, according to the number of primary divisions adopted. We recognize the difficulties of the task, and admit that it is practically impossible to divide the vegetation of the world into regions of equal value and importance, even leaving out of consideration the mountain flora within the tropics. Instead, however, of giving Madagascar and the neighbouring islands the rank of an independent region, we should treat it as a sub-region of the tropical African flora. On the other hand, the Indian region seems too comprehensive, as it includes the whole of tropical India, Malaya, Cochin-China, the Malayan Archipelago, New Guinea, North Australia, and Polynesia, even to the Sandwich Islands. The very extensive recent collections of Madagascar plants, made by various English and French travellers, prove that the flora is really a sub-region of the tropical African flora. With regard to the flora of Polynesia, it is true that the littoral element consists almost exclusively of species common to the Malayan Archipelago and North Australia, many having an even wider range; but the Australian and American affinities of the endemic element are certainly too pronounced, in our opinion, to treat this flora as a sub-region of the Indian; and the Sandwich Island flora is as highly specialized, to say the least, as that of New Zealand. Perhaps it would be more convenient to make it an independent region. Again, the purely Australian types surely predominate largely over the Asiatic in North Australia, especially if we eliminate the widely-dispersed coast plants. Dr. Drude's New Zealand region includes the surrounding islands, except the more southern Macquarie; yet, of the eighteen vascular plants recorded from this island, sixteen are common to the New Zealand group. The Auckland and Campbell groups should be reckoned in the Antarctic region rather than New Zealand; and St. Paul and Amsterdam Islands, as well as the Tristan d'Acunha group, do not belong to the same category. Further, the higher mountain flora of Central America and South Mexico has certainly a greater claim to be included in the Andine region than has that of the Galapagos, though Dr. Drude separates them.

We have called attention to these defects or incon-

sistencies in the limitation of the regions, because we believe that the latest and fullest data relating to the regions in question clearly indicate that it is not a matter of opinion.

The Atlas as a whole is a most laborious and careful compilation, and we do not doubt that it will meet with the favour it deserves. The second sheet of maps illustrates the areas of certain important natural orders and genera; the third, the horizontal zones of vegetation of the world; the fourth, the flora of Europe; the fifth, the floras of Europe and Asia; the sixth, the floras of Africa and Australia; the seventh, the floras of America; and the last represents the areas of plants cultivated for their economic products.

It would be easy enough to find fault with some of the details of the limitation of Dr. Drude's sub-regions, those of tropical Africa and Eastern Asia for example, though it would not always be so easy to suggest more satisfactory ones; but we prefer judging the work by its merits rather than by its real or assumed defects.

This Atlas, it should be added, is a cartographical development of Dr. Drude's "Florenreiche der Erde," which appeared in 1884, and formed the Ergänzungsheft 74 to *Petermann's Geographische Mittheilungen*; that is to say, it is a development so far as the maps are concerned, but the explanatory letterpress has been reduced to four pages folio. The maps, sixteen in number, are admirably executed, and exceedingly elaborate; indeed, the only fault we find in them is an excess of detail, with perhaps too little explanatory text for beginners.

As the author very truly observes, the material available for such a work is now almost inexhaustible, and the task of selecting from it for the purposes in view was no easy one. He brings into contrast the position of botanical geography in 1848, when the first edition of Berghaus's "Physical Atlas" was published, and there was nothing approaching a complete flora of any of the larger areas outside of Europe in existence. Even in 1855, when De Candolle gave to the world his now classical work, "Géographie Botanique Raisonnée," he could only deal with fragments of floras. Now, though it may safely be asserted that future discoveries can in no way affect the main theories of distribution based upon what is already known, very much remains to be done in fossil botany before we shall be able to trace in detail the early migrations of plants. Therefore the only thing that can be successfully accomplished yet is to work out more completely the present distribution of plants, which is practically all that Wallace has done for animals. But he deals specially with the quality and probable origin of the zoology of his regions; and it is just this aspect of botanical geography that awaits further development.

OUR BOOK SHELF.

Tenants of an Old Farm. By Henry C. McCook, D.D. (London; Hodder and Stoughton, 1888.)

THE object of this book is to present "a series of exact truths from natural history in a popular form." The author's original intention was to write a number of essays upon insect life, and particularly upon the life of ants and spiders, which he has especially studied. Friends, however, persuaded him to give the essays a colloquial form, so that they might appeal to as wide a circle of readers as possible. We are not sure that the change was in all respects an improvement, for, as Dr. McCook says, "the truths of Nature are attractive enough in themselves, and need not the seasoning of fiction." The book is very popular in the United States, and there can be little doubt that it will also be appreciated on this side of the Atlantic. The author is a keen and accurate observer of Nature, and his enthusiasm for his subject is so steadily maintained that it cannot but exert some influence on the minds of young students. For the present edition a brief introduction has been written by Sir John Lubbock, who bears cordial testimony to the fidelity and skill with which Dr. McCook has carried on his researches. The work is remarkably well illustrated.

Digging, Squatting, and Pioneering Life in the Northern Territory of South Australia. By Mrs. Dominic D. Daly. (London: Sampson Low, 1887).

THIS is an interesting account of a part of South Australia which is sure to become more and more important. The writer spent three years—from 1870 to 1873—in the Northern Territory, and by far the best chapters are those in which she records her own experiences. The history of the district during the last fourteen years has, however, been carefully compiled from the most trustworthy sources. She has, of course, a good deal to say about the natives, her accounts of whom are freshly and brightly written. Mrs. Daly is of opinion that, so far as the treatment of the aborigines is concerned, only one rule holds good—"firmness accompanied by kindness, fair play, and an honest payment for work done." If they make themselves disagreeable, they must be kept "in their proper place," "for," she says, "when a native shows signs of sulkiness and defiance, it is perfectly certain some mischief is brewing."

Photography Simplified. (London: Mawson and Swan, 1887.)

THIS is a third edition, considerably revised and enlarged, of an elementary and practical treatise, intended chiefly for amateurs and those about to become acquainted with the subject. The earlier chapters deal with the purchasing of apparatus, followed by the various processes of taking the negative, developing, printing, &c., and are written in a very plain and intelligent way. The book concludes with an appendix containing additional useful formulæ, together with a set of labels for the photographic laboratory.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

An Explanation explained.

I AM glad to find that Dr. Guppy has at last enabled us to get to the bottom—I cannot say to the foundation—of the story which was related by the Duke of Argyll on November 17 in last year, to the discredit of Prof. Bonney and the authorities of the Geological Society. It is now admitted that the paper, said to have been "offered to," and "refused by," the Society, never came before the President and Council in any form whatever; and that in fact the paper was not only never presented, but was never even written!

Dr. Guppy's references to myself are capable of the simplest explanation. During the whole time that he was absent in the Solomon Islands, I was in the habit of receiving specimens

and letters from him; and, as he has acknowledged in his book, I carefully studied these specimens and gave him all the advice and assistance in my power in carrying on his geological investigations. Upon his return we had several conversations, always of the most friendly character, concerning the best mode of embodying his observations for communication to different Societies; and until the present time I had not the smallest idea that he was in any way dissatisfied with anything I had ever said or done in connection with the subject.

With respect to a particular conversation referred to by Dr. Guppy as having occurred in the spring of 1885, I have no recollection whatever; but I unreservedly accept his statement as to the *facts* of the case, and only demur to his *interpretation* of them. If Dr. Guppy or anyone else asked my opinion as to the fitness for the Geological Society of "a paper in which Mr. Darwin's theory of coral reefs would be brought under consideration," I should undoubtedly point out to him that the Geological Society had always been averse to publishing papers dealing with such broadly theoretical questions as the origin of coral reefs, and I should advise some other means of publication as more appropriate.

That the Geological Society is not partial in its reluctance to publish papers of a theoretical character will be seen from the fact that although Mr. Darwin in 1837 read a paper to the Society, embodying the chief points of his theory of coral reefs, yet the Society never published the paper in their Transactions. At the time that this occurred Darwin was a member of the Council, and a few months later he became Secretary of the Society.

It is surely unnecessary for me to remark that in any advice which I gave to Dr. Guppy, I was acting simply on my own judgment and individual responsibility. Dr. Guppy was a Fellow of the Society at the time, and had precisely the same right to present papers to it which I had myself. Dr. Guppy chose to ask my advice; I gave it to him as to a friend, and he was perfectly free to act upon it or to reject it as he thought fit. I may add that the Secretary of the Geological Society has nothing to do with the acceptance or rejection of papers, except as a member of the Council, and then only when the question has been brought up by the President for the approval or condemnation of his own action.

How my unofficial act of friendly advice concerning the destination of an unwritten paper came to be represented as the refusal of a paper offered to the Society I am at a loss to conceive! Why the Duke of Argyll, having received the statement which is now before us, should proceed to formulate the very grave accusation against Prof. Bonney personally, and the authorities of the Geological Society, it is for his Grace to explain.

With respect to Dr. Guppy's complaint that his memoirs have been "studiously ignored" during the recent controversy, I cannot help thinking that he has been unduly sensitive. Writing to Prof. Huxley in October last year, and pointing out that the Duke of Argyll was mistaken in supposing there had been no discussion on Mr. Murray's theory, I said "it would be an endless task to attempt to give references to the various scientific journals which have discussed the subject," but in penning these words I had not the smallest idea of speaking slightingly of any of the memoirs which want of space prevented me from citing, and least of all concerning those which contain the facts and observations of Dr. Guppy, of the value and importance of which I had such good opportunities of judging.

JOHN W. JUDD.

Reason and Language.

PROF. MAX MÜLLER has been so kind as to favour the readers of NATURE with his views on language and reason, concisely expressed in a letter to an American friend. As one grateful reader, I much desire both to express my thanks, and also to beg for yet a little further information with respect to matters of such extreme interest.

The Professor says: "Because we reason—that is, because we reckon, because we add and subtract—therefore we say that we have reason." Now, in the first place, I should be glad to be told why "reason" is to be regarded as identical with such "reckoning"? I have been taught to distinguish two forms of intellectual activity: (1) acts of intuition, by which we directly apprehend certain truths, such as *e g.* our own activity, or that A is A; and (2) Acts of inference, by which we indirectly apprehend others, with the aid of the idea "therefore"—evolving into explicit recognition a truth previously implicit and latent in premisses. The processes of addition and subtraction alone, seem to me to constitute a very incomplete representation of our mental processes.

The Professor also identifies language and reason, denying to either a separate existence. As to "reason" he says: "We have only to look into the workshop of language in order to see that there is nothing substantial corresponding to this substantive, and that neither the heart nor the brain, neither the breath nor the spirit, of man discloses its original whereabouts." The expression "whereabouts" would seem to attribute to those who assert the existence of "reason," the idea that it possesses the attribute of extension! In order to understand clearly the passage quoted, we should learn what Prof. Max Müller really means by the term "spirit," which here figures as one species of a genus also comprising the breath, the brain, and the heart. Reason, however, is not represented as being simply language "as we now hear it and use it," but "as it has been slowly elaborated by man through all the ages of his existence upon earth." Thus understood, the Professor "cannot doubt" "the identity of reason and language." Nevertheless, he immediately proceeds to point out a striking want of identity between them. He says, quite truly, "We have two words, and therefore it requires with us a strong effort to perceive that behind these two words there is but one essence,"—namely, that denoted by the Greek word *logos*—"the undivided essence of language and thought." Now, the intimate connection of language (whether of speech or gesture) with thought is unquestionable; but intimate connection is not "identity." If thought and language are "*identical*," how came two words not to have two meanings, or two thoughts to be expressed by one word? The plain fact that we have different words with one meaning, and different meanings with one word, seems to demonstrate that thought and language cannot be "identical."

"No reason without language—no language without reason" is a statement true in a certain sense, but a statement which cannot be affirmed absolutely. Language (meaning by that term only *intellectual* expression by voice or gesture) cannot manifestly exist without reason; but no person who thinks it even *possible* that an intelligence may exist of which ours is but a feeble copy, can venture dogmatically to affirm that there *is* no reason without language, unless he means by reason, mere "reasoning," which is evidently the makeshift of an inferior order of intellect unable to attain certain truths save by the roundabout process of inference.

But I demur to the assertion that truly intellectual processes cannot take place in us apart from language. In such matters our ultimate appeal must be to our own reflective consciousness. Mine plainly tells me that I have every now and then apprehensions which flash into my mind far too rapidly to clothe themselves even in mental words, which latter require to be sought in order to express such apprehensions. I also find myself sometimes expressing a voluminous perception by a sudden gesture far too rapid even for thought-words, and I believe that other persons do the same. A slight movement of a finger, or the incipient closure of an eyelid, may give expression to a meaning which could only be thought in words by a much slower process.

It is the more remarkable that Prof. Max Müller should deny the existence of reason, since he unequivocally affirms, in rather lofty language, the existence of truth. Yet surely the existence of truth, in and by itself, is inconceivable. What can truth be, save a conformity of thoughts and things? I affirm, indeed, the certain existence of truth, but I also affirm that of reason, as existing anteriorly to language—whether of voice or gesture. What is the teaching of experience? Do men invent new concepts to suit previously coined words, or new words to give expression to freshly thought-out concepts? The often-referred-to jabber of Hottentots is not in point. No sounds or gestures which do not express concepts would be admitted by either Prof. Max Müller or myself to be "language."

The Professor speaks of the "alarmingly small" number of primitive concepts; but who is to be thereby alarmed? Not men who occupy a similar stand-point to mine. I fully agree with Prof. Max Müller in saying, "After the genesis of the first concept, everything else becomes intelligible."

We come now to the supreme question of the origin of languages. As to this, the Professor observes: "No one who has not himself grappled with that problem can appreciate the complete change

that has come over it by the recognition of the fact that roots are the phonetic expressions of the consciousness of our own acts. Nothing but this, our consciousness of our own repeated acts, could possibly have given us our first concepts. Nothing else answers the necessary requirements of a concept, that it should be the consciousness of something manifold, yet necessarily realized as one. . . The results of our acts become the first objects of our conceptual thought." The truth of these statements I venture to question. After noting the dogmatic nature of the assertion "Nothing but this *could*, &c.," I must object to the statement of fact as regards human beings now. I do not believe that the infant's first object of thought is "the results of its own acts." In the first place, no object of our early thoughts is merely the "results of our own acts," but a combined result of our own activity and of the action on us of our environment. Secondly, my observations lead me to believe that the infant's first thoughts relate to things external, and certainly not to the results of its own activity as such, which is a highly complex and developed thought. It may be that the Professor, when he says "The results of our acts *become* the first object of our conceptual thoughts," means that such acts in remote antiquity *became* the object of man's first thought. This is probably the case, since, with respect to the origin of thought and language, Prof. Max Müller has adopted Noiré's crude notion that they sprang from sounds emitted by men at work, conscious of what they were doing, in the presence of others who beheld their actions and heard the sounds; the result being the formation of a conceptual word, to attain which five stages had to be gone through, as follows :—

"(1) Consciousness of our own repeated acts.

"(2) *Clamor concomitans* of these acts.

"(3) Consciousness of our *clamor* as concomitant to the act.

"(4) Repetition of that *clamor* to recall the act.

"(5) *Clamor* (root) defined by prefixes, suffixes, &c., to recall the act as localized in its results, its instruments, its agents, &c."

But, if language and reason are identical, reason could not exist before a single conceptual word existed. Nevertheless, to attain to this first single word, we see, from the above quotation, that man must have had the notion of his own acts as such; the notion of their repetition; the notions of clamor, action, and the simultaneity of clamor and action; the will to recall the act (yet *nihil volitum quia præcognitum*); and finally the notions of consequence, instrumentality, agency, or whatever further notions the Professor may intend by his "&c."

Thus he who first developed language must be admitted to have already had a mind well stored with intellectual notions! But can it for one instant be seriously maintained, close as is the connection of language with reason, that their genesis (miracle apart, of which there is no question) was *absolutely* simultaneous? He must be a bold, not to say a rash, man who would dogmatically affirm this. But if they were not *absolutely* simultaneous, one must have existed, for however brief a space, before the other. That intellectual language could have existed without reason is absurd. Reason, then, must, for however short a period, have preceded language.

In conclusion, I desire to point out a certain misrepresentation with respect to natural selection. The Professor says: "In the evolution of the mind, as well as in that of Nature, natural selection is rational selection; or, in reality, the triumph of reason, the triumph of what is reasonable and right; or, as people now say, of what is fittest." But, we may ask in passing, if reason has no existence, how can it "triumph"? The misrepresentation of natural selection, however, lies in his use of the word "fittest." When biologists say that the "fittest" survives, they do not mean to say that that survives which is the most "reasonable and right," but that that survives which *is able to survive*. What there is less "reasonable and right" in a Rhytina than in a Dugong, or in a Dinornis than an Apteryx, would, I think, puzzle most of our zoologists to determine; nor is it easy to see a triumph of reason, in the extermination of the unique flora of St. Helena by the introduction of goats and rabbits.

ST. GEORGE MIVART.

Mechanical Equivalent of Heat.

I FIND that the mode of regarding J advocated in my letter in last week's NATURE (p. 320) is not quite new, for my brother, Dr. Oliver Lodge, writes to tell me that Clerk-Maxwell, on p. 298 of his "Theory of Heat," has called J the specific heat of water. However, he has not done so throughout the book, and I do not think it is the meaning generally attached to the symbol, though it seems to me that it should be so; that is to say, J should always be considered as denoting the specific heat of water at the temperature 0° C.

ALFRED LODGE.

Coopers Hill, Staines, February 6.

"Is Hail so formed?"

I CANNOT accept Dr. Rae's explanation as a "simpler solution" of the phenomenon described by me in NATURE of January 26 (p. 295), because it is based upon meteorological conditions that were at the time non-existent.

My own observation of the pine-tree convinced me that at or near the summit there was no adherent ice or rime; and had there been beads of ice upon the leaves I should still have failed to see what should have caused them while frozen to become detached and change from beads to pellets.

There was a fine mist during the whole of the day, and I observed the phenomenon at 3.30 p.m.

A letter appeared in NATURE upon the same day as mine, drawing attention to the unusual atmospheric conditions observed about that time, and containing facts which manifestly support my theory.

CECIL CARUS-WILSON.

Bournemouth, February 11.

The New Army Regulations.

THE new regulations for the Woolwich entrance examination have been very unfavourably received by men of science. This hostile criticism is in some respects the consequence of the absence of clear discrimination between them and those already in force for the Sandhurst examination.

It must be remembered that candidates for Woolwich cadetships must be between the ages of 16 and 18; that 6000 marks are awarded for mathematics, with 1500 more for drawing and English composition; and that in both the last June and December competitions less than 4000 marks sufficed to place a student among the successful competitors. Since candidates can pass in these subjects alone, it appears unreasonable to complain that youths of scientific power are excluded from the Royal Military Academy. Classics are sufficiently discouraged by the fact that they have no mark value after the cadet has entered the Academy. The 5000 marks offered in the entrance examination for Latin and Greek merely serve to encourage candidates who have been educated on the classical sides, which are almost always the stronger at our public schools. They really tend to widen rather than to narrow the sources from which candidates are drawn.

After a quarter of a century of continuous experience as a student and teacher of elementary science, I find myself reluctantly forced to the conclusion that chemistry, physics, and geology are not good educational subjects for lads under 16 years of age. I believe that it is in most cases desirable that youths intended for a scientific career should not specialize too early. A sound foundation of mathematics and modern languages is almost necessary to enable them to attack their scientific subjects efficiently. With minds trained to the use of the exact and powerful processes of mathematical reasoning, and able to readily appreciate and avail themselves of the wealth of scientific literature in France and Germany, they will probably become more useful officers than if they had acquired a smattering of science.

On the other hand, your wise censure of the discouragment of science in the Sandhurst regulations must commend itself to all thoughtful men. The case is even stronger than at first sight appears in the studious moderation of your judicious article. The limits of age are higher for Sandhurst, being 20, or in some cases 24. The training of the Line cadets is less complete. As they only spend one year at Sandhurst, they are obliged to confine their attention more strictly to professional subjects. Officers of the Line have often more leisure than those in the scientific corps, and there are many reasons why even a slight acquaintance with science would be helpful to them. It also seems hard that a candidate should be handicapped by not taking up Latin. Sometimes it has been discontinued for a considerable period, and a candidate can ill afford to take up "a 2000 subject," considering the severity of the competition.

I would wish respectfully to suggest that a memorial should be presented to the War Office by all interested in the teaching of science, praying that, if a candidate for an army examination

wishes to substitute for Latin one of the sciences enumerated in Group II., it should be allowed a maximum of 3000 marks.

2 Powis Square, W. HENRY PALIN GURNEY.

"British and Irish Salmonidæ."

As your reviewer allows that he "intentionally omitted" five words from a sentence of mine which he quoted in order to criticise, I may well leave comments on such a proceeding to your readers. I willingly acquit him of having purposely made me to suggest utter nonsense, as I cannot help thinking that his knowledge of fish-culture was such that he was unaware he was doing so.

As to the second point he says, "I doubted and still doubt if there is any method practised in which layers of moss are used and are separated from the eggs by muslin and similar material." As he rejects the Howietoun account which I gave, I now submit extracts from two standard works, one American, the other English, which will, I believe, be conclusive to those who are ignorant of fish-culture, for every fish-culturist is aware that this plan is commonly adopted. Livingstone-Stone ("Domesticated Trout," ed. 3, 1877) remarked :—"Theodore Lyman recommends placing each layer of eggs in a fold of mosquito netting to keep them from mixing with the moss and so facilitate the unpacking of them. *This is a great improvement. By all means use mosquito netting*" (p. 149). Mr. Andrews, of Guildford, wrote thus in the Badminton Series ("Salmon and Trout," 1885) :—"The plan of packing does not vary much with trout breeders. The eggs are placed in alternate layers between moss, and protected by a covering of mosquito netting, muslin, swans' down, calico, or butter cloth, so arranged that the eggs shall not be crushed or escape" (p. 447).

As regards the third point, your reviewer now appears to be convinced that *Salmo namaycush* is a char, as I stated it to be. It must be a matter of regret that he omitted to investigate the foregoing questions prior to authoritatively writing upon them in such a well-known publication as NATURE.

Cheltenham, February 4. FRANCIS DAY.

IN his last letter Mr. Day has certainly proved the correctness of the statement in his book that salmonoid eggs are packed with layers of moss from which they are separated by muslin or other textile fabric. If I had known as much about salmon-culture as he, I certainly should not have questioned the statement; it is to be noted that I only questioned and did not deny. If I had been as completely versed in the knowledge of Salmonidæ as Mr. Day, I should have written a book on the subject instead of reviewing his. But the essential point, which Mr. Day seems incapable of appreciating, is this: that there was nothing in the notes on the subject of packing in his book which confirmed the statement in the text; and although my doubts as to the correctness of that statement are removed by his letter, they were perfectly justifiable in a reader of his book. Mr. Day does not apparently suspect that people interested in the subject, including the reviewer, read his book for the sake of gaining information, and not because they already know as much about the subject as himself. All I had to do was to give my impressions of the book as I found it: the fitness of my criticisms is only the more established by the lengthening appendix to his book which Mr. Day is now publishing in your correspondence columns. YOUR REVIEWER.

MODERN VIEWS OF ELECTRICITY.[1]

PART III. MAGNETISM—(*continued.*)

VIII.

IT will now be perceived that a fly-wheel in rotation is the mechanical analogue of magnetism, or more definitely of a section of a line (or tube) of magnetic force; and that a brake applied to such a fly-wheel, with consequent slip, dissipation of energy, and production of heat, is in some sort a mechanical analogue of an electric current.

The field is regarded as full of geared elastic vortices or whirls, some of which are cogged together, so to speak, while others are merely pressed together by smooth rims. It is among these latter that slip is possible, and in the regions occupied by them that currents exist; the energy dissipated here being transmitted through the non-slippery or dielectric regions from the source of power, just as energy is transmitted from a steam-engine through mill-work or shafting to the various places where it is dissipated by friction.

[1] Continued from p. 348.

Mechanical Force acting on a Conductor conveying a Current.

In Fig. 41 the conducting portion is shown with opposite rotations on either side of it. Now superpose a uniform rotation all in one direction upon this, so as to increase the spin on one side and diminish it on the other. Immediately the extra centrifugal force on one side will urge any movable part of the conductor from the stronger to the weaker portion of the field.

The field for a direct and return circuit may be similarly drawn by superposition of their separate whirls (see Fig. 40); and so it becomes evident why a circuit tends to expand so as to inclose the largest possible area, even if no other magnetic field than its own be acting on it.

Also if two circuits are arranged near each other in a plane, with their currents in opposite directions, they will more or less neutralize each other's effect on the space between them, causing (if equal) a region of no spin there. Their neighbouring portions will thus get urged together by the unbalanced pressure on the other side: or, currents in the same direction attract.

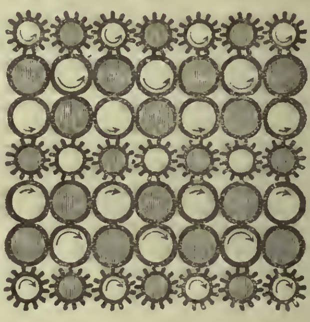


FIG. 44.—Two parallel conductors conveying equal currents in one direction and getting pushed together by the centrifugal force of the outside whirls, no whirl existing between them. The length of the arrows again suggests the distribution of magnetism in the conductors. Fig. 40 showed the correlative repulsion of opposite currents.

As for the effect of iron introduced into a circuit, it brings into the region of space it occupies some two or three hundred times as many lines of whirl as were there before, and these naturally contribute mightily to the effects, both those exhibiting mechanical force and those exhibiting inertia.

When one says, as roughly one may do, that iron brings 300 fresh lines into the field, one means that for every whirl otherwise excited, 300 more are faced round in the iron. And this process goes on while the field is increasing in strength until the total number of whirls in the iron begins to be called upon; when this point is reached the rate of addition is not maintained, and the iron is said to show signs of saturation. Ultimately, if ever all its whirls were faced round, the iron would be quite saturated; but long before this point is reached another cause is likely to make itself felt, viz. the falling off in the strength of the whirls already faced round, by the action of the strong magnetic induction, which is all the time acting so as to weaken the iron currents so far as it is able. And thus at a certain point hitherto unreached by experiment the iron may not only fail to increase the strength of the field any more, but may actually begin to diminish it.

The easiest way to picture the effect of iron is to think of its wheels as some two or three hundred times as massive as those of air, so that their energy and momentum are very great.

That which is commonly called magnetic permeability may in fact be thought of as a kind of inertia, an inertia per unit volume; though how it comes to pass that the ether inside iron is endowed with so great inertia one cannot say. Perhaps it is that the iron atoms themselves revolve with the electricity, perhaps it is something quite different. Whatever the peculiar behaviour of iron, nickel, &c., be due to, it must be something profoundly interesting and important as soon as our knowledge of their molecular structure enables us to perceive its nature.

Induction in Conductors not originally carrying Currents but moving in a Magnetic Field.

To explain the currents induced in a conductor moving through a uniform magnetic field is not quite easy, because none of the diagrams lend themselves naturally and simply to the idea of circuits changing in form or size.

If we take a rigid circuit in a magnetic field, like Fig. 45, and revolve it out of its plane 180°, it is obvious that a current will be excited in it, for the process is essentially the same as if the conductor were kept still and the field reversed.

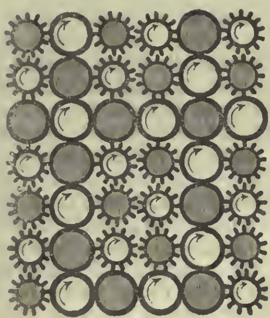


FIG. 45.—Section of a uniform magnetic field with two rails and a slider in it. If the slider be moved to or fro, the wheels inside get initially, compressed or extended, and thereby gain or lose energy respectively thus exciting the state of slip known as induced current.

But to understand the current excited in a closed circuit when a portion of it moves across the lines so as to embrace a greater number of them, one has to take into account the fact that the inside whirls are expanding and doing work in forcing the conductor away, while the outer whirls are resisting the motion, and being thereby compressed and rendered more energetic. Thus the wheels inside revolve slightly slower as the circuit expands, and those outside revolve slightly quicker. Both these processes cause a slipping of the gearing, first all round the inside and then all through the substance of the wire, whereby positive electricity moves forward in one direction round the circuit, the negative moving oppositely; and so a current is accounted for. It is not to be supposed, however, that any finite expansion of the wheels really occurs: the motion is rapidly equalized by diffusion through the wire, and fresh wheels come in round it from outside; hence directly after the conductor has stopped moving the field is again steady, but with many more wheels inside the contour than it possessed at first.

Representation of an Electrostatic Field again, and superposition of it on a perpendicular Magnetic Field.

An electrostatic strain is, we know, caused by a displacement of positive electricity one way along the lines of force, and by an equal displacement of negative the other way. Half the process was indicated crudely in Fig. 6; we may now represent it rather more fully with the help of our elastic cells by Fig. 46.

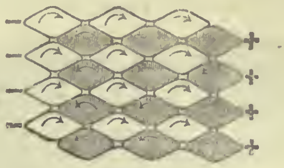


FIG. 46.—A portion of an electrostatic field between two oppositely charged bodies, with its lines of force going from right to left, and showing a tension along and a pressure at right angles to them, due to the elasticity of the cells (which elasticity may be due to their containing fluid in a state of whirl). Magnetic lines of force perpendicular to the paper are also shown in section. While this magnetic field was being excited and propagated from below upwards, a slight strain would be produced in the elastic cells, like but immensely less than that shown; as contrasted with its normal condition (Fig. 37). Conversely, while this electrostatic strain was being produced, the positive whirls would be infinitesimally quickened and the negative ones retarded during the displacement, thus producing a minute magnetic effect. If the medium is not magnetized, the whirls are not necessarily absent, only faced all ways.

Here the positive cells have been pulled one way, the negative the other way; and when the distorting force is removed, the medium tends to spring back to its normal condition, exerting an obvious tension on bodies attached to it in the direction of its lines of force, its elongated direction, and an obvious pressure in all perpendicular directions, its compressed directions.

Now, if all the cells are full of parallel whirls, as in the preceding magnetic diagrams, it is not improbable that this electrostatic distortion or "shear" of the medium may affect its magnetic properties slightly, and that, if the direction of electrostatic strain were rapidly reversed, a small magnetic oscillation would also ensue; but the exact details of these mutual actions are difficult to specify at present.

Disruptive Discharge.

Disruptive discharge may be thought of as a pulling of the shaded cells violently along past the others; the process being accompanied by a true disruption—a sort of electrolysis—of the medium, and a passage of the two electricities in opposite directions along the line of discharge.

Consider the locomotion of any one horizontal row of shaded cells in Fig. 46 during the occurrence of such a disruption of the medium. The cells slide on towards the right, and, as they slide, the spin of the negative cells above them is retarded while that of those below them is accelerated; consequently a true magnetic effect is produced, just like that accompanying a current, and a disruptive discharge has therefore all the magnetic properties of a current.

Effects of a Moving Charge.

This locomotion of a set of positive cells, or of negative cells the other way, as just considered, is very near akin to the motion of a charge through a dielectric medium.

A charge can only exist at the boundary between a dielectric and a conductor, or at least between one dielectric and another of greater density. So, when a charged body moves along with extreme rapidity, it can be thought of as exciting a rotation in the cells most closely in contact with it greater than that which it excites in the opposite kind of cells, and thus produces the whirl proper to a magnetic field. Thus does a moving charge behave just like a current of a certain strength.

It may be, indeed, that this is the customary way of exciting a voltaic current; for the chemical forces in a cell cause a locomotion of charged atoms, and thus set

up a field, which, spreading out in the way Prof. Poynting has sketched, reaches every part of the metallic circuit and excites the current there.

Electrostatic Effects of a Moving or Varying Magnetic Field.

Just as we have seen that a moving or varying electrostatic field may produce slight magnetic effects, so one can perceive that a moving or varying magnetic field brings about something of the nature of an electrostatic strain.

For a spreading out field is continually propagating the rotation on from one layer of wheels to the next. If there is any slip, we thus get induced currents, and the rate of propagation is comparatively slow, being a kind of diffusion; but even if there is not any slip, yet, unless the wheel-work is absolutely rigid, the rate of propagation will not be infinite. The actual rate of propagation is very great, which shows that the rigidity of the wheels is very high in proportion to their inertia, but it is not infinite; and accordingly the propagation of rotation is accompanied by a temporary strain. One part of the field is in full spin, another more distant part is as yet unreached by the spin; between the two we have the region of strain, the wheel-work being distorted a little while taking up the motion. Thus does a spreading out magnetic field cause a slight and temporary electrostatic strain, at right angles both to the direction of the lines of force and to the direction of their advance.

Generation of a Magnetic Field. Induction in Closed Circuits.

Picture to oneself an unmagnetized piece of iron: its whirls are all existent, but they are shut up into little closed circuits, and so produce no external effect. Magnetize it slightly, and some of the closed circuits open out and expand, with one portion of them in the air. Magnetize it strongly, and we have a whole set of them opened out into vortex cores, still with the whirl round them, and constituting the common magnetic lines of force. There is no need to think of iron and steel in this connection. In air or any substance the whirls are still present, though much fewer or feebler, and their axes ordinarily form little closed circuits—it may be inside the atoms themselves. But wrap a current-conveying wire round them, and at once they open out into the lines of force proper to a circular current.

Again, think of an iron ring, or a hank of wire as bought at an ironmonger's: wrap a copper wire several times

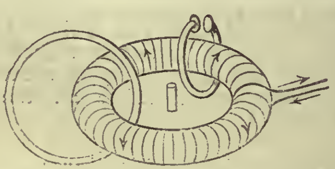


FIG. 47.—Closed magnetic circuit like Fig. 42, with a single-ring secondary circuit, and another open secondary loop; also with a short conducting-rod standing up in it.

round it, as a segment of a Gramme ring is wound (Fig. 47), and pass a current. The closed vortices in the iron at once expand: a portion of each flashes out and across the air-space inclosed by the ring (not by any means confining itself to a plane, of course), and enters the ring on the opposite side; so that directly the current is steady the lines all lie inside the iron again, but now inclosing an area—the area of the ring—instead of being shut up into infinitesimal links. In a sense the iron is still unmagnetized, for its lines of force still form closed contours within it, and none protrude any part of themselves into the air, except for irregularities. But in another sense it is highly and permanently magnetized round and round in itself, the magnetism being not easy to get out of it again, except by judiciously arranged reverse currents.

It is now like one great electric vortex ring instead of like a confused jumble of microscopic ones. Its section was shown in Fig. 42.

During the variable period, while the current is increasing in strength, or while it is being reversed, the region inclosed by the ring and all around it is full of myriads of expanding lines of force flashing across, broadside on, from one side of the iron to the other, and there stopping. It is the presence of these moving lines, changing rapidly from a "simply-connected" into a "multiply-connected" state, or *vice versâ*, which causes the powerful induced currents of "secondary generators."

In every case of varying magnetic field, in fact, we have lines moving broadside on, propagating their whirl, and more or less disturbing the medium through which they move.

Next consider a moving or spinning magnet. Its lines travel with it, and, being closed curves, they also must move broadside through the field, so that in this case we may expect just the same effect as can be obtained from a varying magnetic field.

If a broadside-moving line of force cut across a conductor, its motion is delayed, for its wheels slip and only gradually get up a whirl inside the ill-geared substance; thus, as we know, causing an induced current (see Fig. 43).

If a conducting ring is looped with the iron ring previously mentioned, as a snap-hook is looped with an eye, then every expanding vortex, while the ring is being magnetized, has necessarily to cut through the conducting ring once and no more, no matter what its shape or size. The electromotive force of induction is in this case therefore perfectly definite, and simply proportional to the number of turns made by the secondary round the core of the ring (Fig. 47).

Instead of supposing a closed conducting secondary circuit, imagine an open one: there is still an E.M.F. in it, though rather less than before because a few of the expanding lines flash through the gap and produce no effect, so the electricity must surge to and fro in the conductor as water surges up and down in a tilted trough, and a small condenser attached to the free ends will be alternately charged and discharged. The gap might become so large that nothing is left but a short rod (Fig. 47): in this also similar oscillations would occur.

But now suppose no secondary conductor at all; nothing but dielectric inclosed by the ring. In it there must be an electric displacement excited every time the magnetism of the ring is reversed. It may be an oscillatory displacement, but still on the whole in one direction during rise of magnetism, and in an opposite direction during reversal of magnetism. A charged body delicately suspended within the ring may feel the effect of the minute electrostatic strain so magnetically produced.

To see the mode in which an electrostatic displacement arises in the space embraced by the ring we have only to turn to Fig. 42, and look at the set of wheels along the line A B separating one half the section from the other. They cannot steadily rotate either way, for they are urged in opposite directions by the two halves; in other words, there is no magnetic field near such a ring, as is well known; but, nevertheless, during a change of magnetism, while the whirls inside are changing in speed, the rub on the dielectric necessary for checking the outer wheels of the conductor is either increased or diminished; and if the wheels have any elastic "give" in them, as we know they have, the electrostatic strain in the field is thereby altered during the varying stage of the magnetism.

OLIVER J. LODGE.

END OF PART III.

(*To be continued.*)

THE MECHANISM OF THE FLIGHT OF BIRDS.

THE following is a translation of an article in *La* 5 *Nature* (December ~~3~~, 1887), on the mechanism of the flight of birds, by Prof. E. H. J. Marey. Through the courtesy of the editor of our French contemporary we are able to reproduce the figures illustrating M. Marey's interesting paper.

In a preceding article [see NATURE, vol. xxvi. p. 84], I showed that photography could represent the successive positions of a bird's wing, at different moments in its

FIG. 1.—Sea-gull. Transverse flight. Ten images per second.

flight; that there might be obtained at the same time the positions of the bird in space at equal and known intervals of time; and I expressed the hope of solving by this method the obscure problem of the mechanism of flight.

Since that time, the photographic method has been perfected, and the number of species of birds to which my researches have extended has been multiplied.

From the comparison of the several species which I have had at my disposal, the results show that, except in certain differences in details, they all execute movements

FIG. 2.—Small heron. Transverse flight. Ten images per second.

of the same nature; in all, the wings bend up at the moment of ascension, spread out quickly when at the wished for height, are then lowered, carried in front, and approached to the body; at the close of the descent, the ioints anew bend up, and the ascent recommences.

The illustrations 1, 2, 3, 4, and 5 represent the flight of the sea-gull, the heron, the pigeon, and the pelican.

These illustrations reveal curious attitudes which the eye has not time to seize, and with which we are not familiarized in the artistic interpretations of birds. According

FIG. 3.—Pigeon. Transverse flight. Ten images per second. (Fac-simile of instantaneous photographs taken by the author.)

to a just remark of Mr. Muybridge, the European painters almost always represent birds flying with their wings elevated; the Chinese and Japanese, on the contrary, represent them indifferently with wings both raised and lowered. That does not, however, mean that the artists of the extreme East have faithfully reproduced the different attitudes of birds: the comparison of their representations with those of instantaneous photography shows clearly that no more in China than here does the eye perceive actions which last only for a very brief moment.

Seen only under one aspect, representations of a bird

on the wing do not give us correct ideas or the movements of the wings; we must photograph the bird under several aspects in order thoroughly to comprehend this mechanism. We have made several arrangements in order to procure this effect. One of these, placed at a height of 12 metres (nearly $13\frac{1}{2}$ yards), gave representa-

Fig. 4.—Crested heron. Transverse flight. Ten images per second.

Fig. 5.—Pelican. Transverse descending flight. Ten images per second.

Fig. 6.—Sea-gull seen from above. Ten images per second. (Fac-simile of instantaneous photographs taken by the author.)

tions of the bird as seen from above (Fig. 6); others, variously placed, showed it from the side, or flying in the direction of the photographic apparatus (Fig. 7). These representations, taken under different conditions, complement each other. Thus, the birds seen from above show a singular curvature in the flat surface of the

wing, the existence of which one would not suspect from the profile representations. This curvature appears at the end of the depression of the wing, at the moment in which the joints begin to bend upwards in order to prepare for an ascent. Hence results a spiral aspect of the wing, recalling the form which Mr. Pettigrew considers the essential element in a bird's propulsion. But we must observe that this form is only produced at the very close of the act of descent, at the "*point mort*" of the wing's action, as we say in mechanics, and at a moment in which it, having become passive, is about to remount by the resistance of the air. These figures also show a fact wholly unforeseen—namely, that the movements in flying are not symmetrical. It had been previously supposed that the bird, when desirous of turning laterally the direction of its flight, executes movements more extended from the side which is to progress most rapidly; that is to say, that it gives more amplitude to the movements of the right wing if it wishes to turn to the left, and reciprocally. It is scarcely needful to say that photochronography condemns entirely the hypothesis in which it was supposed that one of the wings of the bird could bend more frequently than the other; the movements of the two wings are perfectly synchronous, if not equal, in extent. It is seen, in short, from these representations, that the body of the bird inclines and moves in different ways, so as to carry its centre of gravity to one side or the other, according to the necessities of the equilibrium. The bird whose attitudes are portrayed in Fig. 6 seemed careful to bear the weight of its body to the left on account of the smaller surface of its right wing, from which some feathers were missing.

The representations taken in front and a little obliquely, as in Fig. 7, give also useful information. They show that the extremity of the wing—a part of the organism in full activity, since it strikes the air with greater speed—presents, at the time of lowering, changes of surface which the secondary *rémiges* extending from the carpus to the shoulder do not offer. There exists in the wing feathers of the different orders a species of separation, showing that the carpal articulations are the seat of a light twisting movement favourable to the bending of the surface of the carpal *rémiges*. In these representations may also be readily seen the bending and convergence of the wings at the close of their lowering, the depression which the anterior side of the wing presents at this moment from the effect of a flexion beginning at the elbow. In order to follow in all their details the changes of movement in the

Fig. 7.—Sea-gull flying obliquely in the direction of the photochronographic apparatus. (Fac-simile of instantaneous photograph taken by the author.)

wings, it has been necessary to make many experiments, so as to obtain, during a single stroke of the wing, ten or twelve successive views of the bird seen under each of these different aspects.

These representations having once been obtained, I was in possession of all the elements necessary to understand completely the motions of the wings according to the three dimensions of space. But in order to represent them, figures in relief were necessary; and circumstances were favourable to this. At Naples, where I then was, the almost lost industry of casting bronze in wax has been preserved from the most remote antiquity. I modelled in wax a series of figures representing the successive attitudes in a single revolution of the wing, ten for the sea-gull, eleven for the pigeon: these models, when given to a skilful moulder, were reproduced in bronze with perfect fidelity.

Fig. 8 represents, disposed in a series, and following each other in their order of succession, at intervals of 1/88 of a second, the phases of one stroke of a pigeon's wing.

These bronze figures were made white, in order to render more apparent the effects of light and shade. Thanks to the multiplicity of the attitudes represented in this series, all the phases of the motion of the wings are easily followed: it is seen how they fold, rise, expand, and sink.

In order the better to understand how the movements of the bird's wing follow each other, of which photochronography gives an analysis, I have had recourse to the use of the zootrope, which recomposes them, and gives to the sight the impression of a bird flying.

The zootrope, represented in Fig. 9, offers this speciality, that it is formed by figures in relief. This is a great advantage from the point of view of the impression which it gives; in fact, these small figures of birds, arranged in a circle in the apparatus, present themselves to the observer under various aspects.

At the beginning of the movement the bird's backs are seen; then, in their circular course, they present their sides, pass across in full view, and at last return to the observer. Besides, the movements of the wings, which in nature are extremely rapid, and consequently imperfectly seen, are here much slower, so that the phases may be easily followed, and in an instant, more may be perceived than the most attentive observer of the flight of birds could discover by the most careful observation...

Fig. 9 shows the arrangement of the zootrope; it

cannot unfortunately give an idea of the effect produced by the apparatus in motion.

But it may be said that this rotatory method interprets the movements of the bird without indicating the forces which produces them. While it would be well to know that force, it is better still to measure the mechanical labour expended in order to sustain and transport itself in the air.

Let us see whether our photographic images reveal to us anything in regard to this.

When one knows the mass of a body, and the speed with which it moves, one can calculate the force

FIG. 8.—Bronze figures representing eleven successive positions at successive moments in the stroke of a pigeon's wing.

which has set this body in motion, and the labour expended by this force. If we take a projectile of a certain weight, and throw it before the photochronographic apparatus, and take a series of images of this projectile at intervals of 1/100 of a second, Fig. 10 shows the trajectory curve followed, and the space which separates the images from each other shows the space traversed by the projectile in each of the hundredth parts of a second during which its movement has lasted. From ten to ten a more brilliant image has been produced by an aperture in the diaphragm larger than the others: these marks are useful in order to facilitate the numbering of the images,

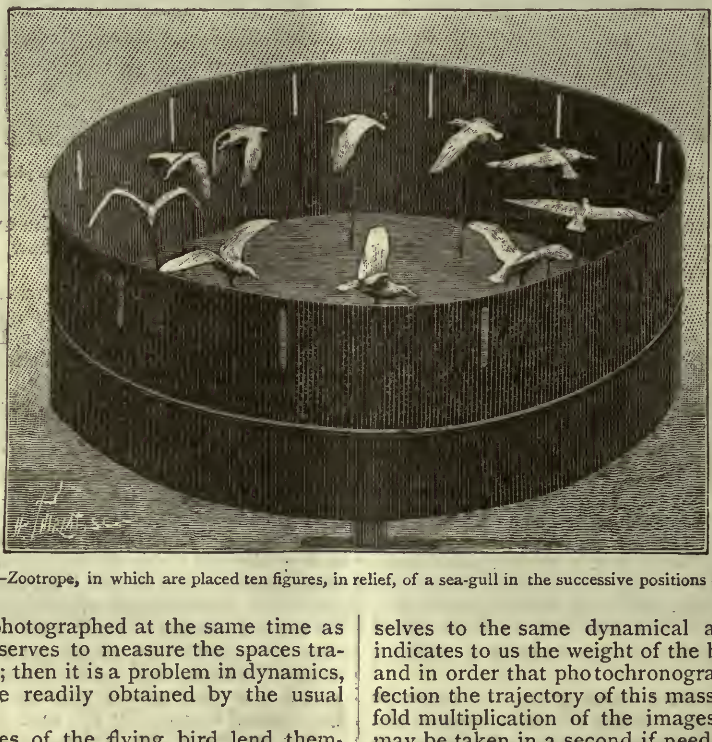


FIG. 9.—Zootrope, in which are placed ten figures, in relief, of a sea-gull in the successive positions of flight.

a fixed metrical scale, photographed at the same time as the object in motion, serves to measure the spaces traversed at each moment; then it is a problem in dynamics, whose solution may be readily obtained by the usual methods of calculation.

The successive images of the flying bird lend themselves to the same dynamical analysis. The balance indicates to us the weight of the bird; we know its size; and in order that photochronography may give us to perfection the trajectory of this mass, it only requires manifold multiplication of the images obtained (a hundred may be taken in a second if need be). But those images

will be partially confused, because the bird, in the hundredth part of a second, only traverses a space equal to the length of its body: the image of the second will therefore partly cover that of the first, the third that of the second, and so on. In this confusion one can scarcely distinguish the moment in which the wing lowers itself, or that in which it is raised. But this is of no importance: we fix on the head of the bird a small but very brilliant metallic point, and the image of this point, clearly seen in the series of figures, reveals the trajectory of the bird, together with its speed, and the accelerations and slackening of speed produced by the movements of the wings. One may then face the dynamic problem of flight. It is granted first that the bird does not oscillate sensibly in the vertical sense, whence one must conclude that the resistance of the air under its wings is precisely equal to its weight. On the other hand, it is to be observed that the motion of the animal presents alternations of speed and slowness, showing that the propelling force and the resistance of the air predominate

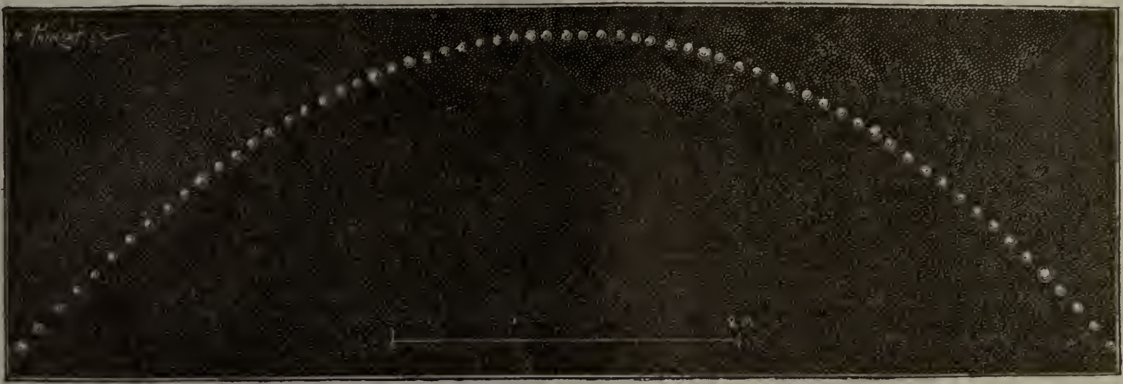


FIG. 10.—Trajectory of a white ball thrown in front of a black screen. The interval between two successive images is measured on the metrical scale. The time taken to travel over this interval is 1/100 of a second.

by turns. From the value of these accelerations there must be deducted the value of the horizontal component of the bird's motion, and that of the resistance of the air.

The calculations based on these experiments have given the following results for the forces which act during the flight of the sea-gull:—

Vertical component	0·623	kilogramme
Horizontal component	0·898	,,
Total ...	1·521	,,

These forces develop themselves during the act of lowering the wings; the ascent is passive, and is due to the pressure of the air upon the lower surface of the wings, which act then for the support of the bird, as in a paper kite.

As the resistance of the air under the wings acts at a point a considerable distance from the articulation of the shoulder, and as the pectoral muscles, by which the wings are lowered, act very near the articulation—that is to say, on the arm of a very unfavourable lever—it results

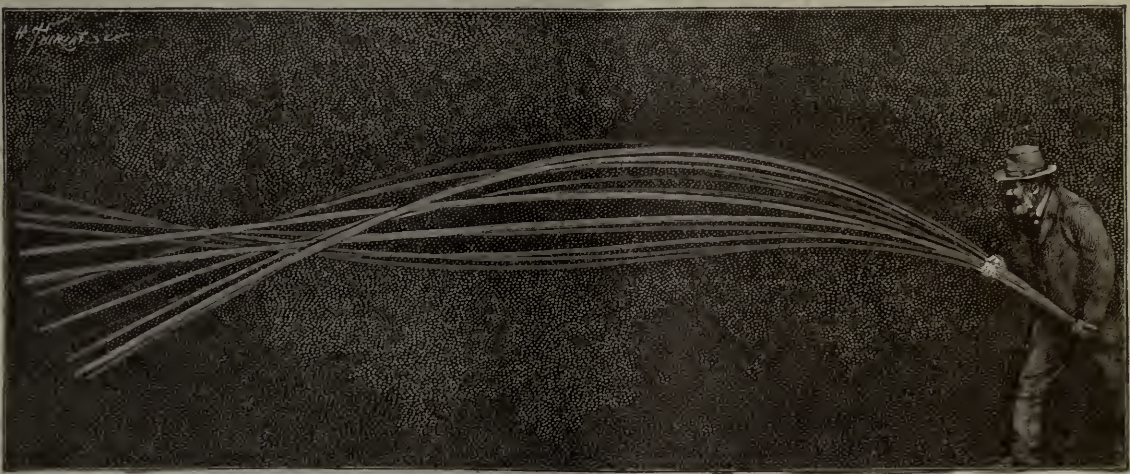


FIG. 11.—Curves and nodes produced by a vibrating stalk, one end of which is fixed. (Fac-simile of instantaneous photographs taken by the author.)

that the effort of the muscles is much greater than the resistance of the air which they surmount. For the pectorals of the sea-gull, the effort developed would be 19 kilogrammes.

It is frequently asked whether the muscles of birds have not a specific strength greater than those of other animals—that is to say, whether two bundles of the same thickness of muscles belonging, one to a bird, the other to a mammal, would have different powers. In the sea-gull which served for my experiments, one transverse section of the pectoral muscles arranged perpendicularly to the direction of their fibres had about 11 centimetres square of surface, or about 1·600 kilogramme per square centimetre. Other birds had formerly given me nearly similar returns for their specific strength; thus, the buzzard developed 1200 grammes per square centimetre, the pigeon 1400 grammes.

Aëronauts hope that they will one day invent a machine

capable of transporting man through the air, but many of them are troubled by a doubt; for they ask themselves whether the force of the bird does not exceed that of the known motors. The experiments on that subject may reassure them, for, if we compare the muscular force of the bird with that of steam, we see that one muscle would be comparable to an engine at very low pressure. In fact, the steam which would develop 1·600 kilogramme per square centimetre would scarcely have more than an atmosphere and a half of pressure. But the true comparison to establish between the animated motors and the engines consists in measuring the work which each of these motors can furnish, with equal weight, in the unity of time.

The measure of the work of a motor is obtained by multiplying the effort put forth, by the path which the point of application of that effort traverses. Photochronography expresses at each moment the spaces traversed by the mass of the bird and the displacement of the centre of pressure of its wings, giving thus the factor path in the measure of the work. In this way it is found that for the five strokes of its wing which the sea-gull gives every second, at the moment when it flies away, the labour done would be 3·668 kilogrammes. This calculation is very high; it corresponds to that which an engine would make in raising its own weight to a height of more than 5 metres in a second.

But that is only a maximum which the bird does not attain to except at the moment of flight, when it has not attained much speed. In fact, according as the passage of the bird is accelerated, the air under its wings presents a more resisting fulcrum. I have previously experimentally demonstrated this fact, announced by the brothers Planavergue, of Marseilles, and of which the following is the theory.

When the bird is not yet in motion, the air which is struck by its wings presents, in the first instance, a resistance due to its inertia, then enters into motion, and flies below the wing without furnishing to it any support. When the bird is at full speed, on the contrary, its wing is supported each moment upon new columns of air, each one of which offers to it the initial resistance due to its inertia. The sum of these resistances presents to the wing a much firmer basis. One might compare a flying bird to a pedestrian who makes great efforts to walk on shifting sand, and who, in proportion as he advances, finds a soil by degrees firmer, so that he progresses more swiftly and with less fatigue. The increase of the resistance of the air diminishes the expenditure of labour; the strokes of the bird's wing become, in fact, less frequent and less extended. In calm air, a sea-gull which has reached its swiftest, expends scarcely the fifth of the labour which it had to put forth at the beginning of its flight. The bird which flies against the wind finds itself in still more favourable conditions, since the masses of air, continually renewing themselves, bring under his wings their resistance of inertia. It is, then, the start which forms the most laborious phase of the flight. It has long been observed that birds employ all kinds of artifices in order to acquire speed prior to flapping their wings: some run on the ground before darting into the air, or dart rapidly in the direction they wish to take in flying; others let themselves fall from a height with extended wings, and glide in the air with accelerated speed before flapping their wings; all turn their bill to the wind at the moment of starting.

My experiments have, up to the present, only been able to apply to the flight of departure. In order to study the full flight there are conditions difficult to realize. With a courtesy for which I thank him, M. Eiffel has offered to me on the gigantic tower which he is erecting (at Paris) a post of observation which will leave nothing to be desired. From that enormous height, birds photographed during a long flight will give photochronographic images much more instructive than those which I have hitherto been able to obtain.

Without entering into the dry details of experiments and calculations made,[1] I have aimed at showing that the movements of birds, if they escape the sight, may be faithfully recorded by a new method which is applicable to the most varied problems of rotation and of mechanics.

Photochronography, in fact, gives experimentally the solution of problems often very difficult to solve by calculation.

Imagine a certain number of forces acting in different ways upon a known mass; the complicated way in which those forces are arranged sometimes renders long calculations needful in order to determine the positions which the moving object will occupy at successive moments; whilst if the body itself, submitted to those different forces, can be placed before the photochronographic apparatus, the path which it will follow expresses itself upon the sensitive plate.

Distinguished physicists disputed lately as to the form the free extremity of a vibrating stalk ought to present in which are produced curves and nodes: the greater number of them supposed that between the last node and its free extremity the stalk would present a bent form. Experiment has shown that it is not so, and that the last elements of the vibrating stalk are perfectly rectilinear (Fig. 11).

How many problems whose solution has formerly cost efforts of genius might be solved by a very simple experiment! Galileo in our day would not have needed to lessen the speed of the falling body in order to observe its motion. He would let fall a brilliant ball before a dark field, and would receive from it photographically successive images. Upon the sensitive plate he would have read, in the simplest way possible, the laws of space, of the speed and the accelerations which he has had the glory to discover.

To return to our subject, the laws of the resistance of the air to the living creatures of different forms which move in it ought to be searched into by photochronography. Already interesting results have been acquired: we have been able to determine the path of motion and the speed of small polished bodies (*petits appareils planeurs*) which move freely in the air, and which the eye has not time to follow in their rapid motions. Studies like these, undertaken and methodically carried out, will certainly lead to a comprehension of the still obscure mechanism of the hovering of birds.

[1] See the *Comptes rendus* of the Académie des Sciences 1886-87.

TECHNICAL EDUCATION.

WHEN the time comes for the discussion of the new Technical Instruction Bill, attention will no doubt be given to an important series of resolutions (printed on the next page) which have just been passed by the Executive Committee of the North of England Branch of the National Association for the Promotion of Technical Education. The first six of these resolutions were unanimously adopted by the Committee, and the seventh was, on the motion of Mr. T. Burt, M.P., seconded by Mr. J. H. Girling (President of the Trades Council), adopted with one dissentient. The following are the advantages which the Committee desire to secure:—(1) For primary and secondary education a greater freedom of instruction under the existing code preparatory to technical education in the higher schools. (2) A direct or indirect pecuniary aid for superior education in science and art schools and in Colleges which afford technical education. (3) For all apprenticeship schools or trade classes a supervision by members of the trade, but no Government grant, thus to avoid any objections which might be raised by Trades Unions, or any jealousy arising from an apparent protection of one

or more particular trades. (4) For University Colleges a grant similar to that made to training Colleges for education afforded to persons intending to become teachers.

The resolutions are as follow:—

1. That public funds (rates and taxes) should not be employed to meet the current expenses of teaching specific trades.

2. That it is undesirable that instruction in the use of tools should be introduced into primary schools as a grant-earning subject.

3. That with a view to preparing pupils for technical education later on—

(*a*) The grant to day-schools should depend, to a much less extent than at present, on the results of the examination of individual pupils in reading, writing, and arithmetic, and should be largely dependent on the inspector's report of the general character of the teaching and the equipment of the school.

(*b*) There should be greater liberty in the choice of subjects in primary schools, and the same class subject should not necessarily be taken throughout the school.

(*c*) The grant to evening continuation schools should be regulated by the report of the inspector on the character of the teaching, and on the attendance list, and not upon the result of the examination of individual pupils.

4. That when a technical school is combined with a science and art school, the contribution to the building fund, through the Science and Art Department, should exceed £1000, if, in the opinion of the Department, the requirements of the locality demand it.

5. That it is desirable that, when specific trades are taught in technical schools, the practical teaching of each trade should be under the general direction of a committee, consisting mainly of members of that trade; that the teaching should be given in the evening, and be restricted to pupils actually engaged in the respective trades, and that, when specific trades are taught, any deficiency in current expenses should be guaranteed by the trade of the district.

6. That a certain percentage of persons preparing for appointments as teachers in elementary schools should be allowed to attend lectures and laboratory work at Universities and University Colleges, where a curriculum satisfactory to the Education Department is provided, and that the same grant should be made on account of such students as in the case of ordinary training Colleges.

7. That it is desirable that University Colleges in which higher scientific and technological training are combined should be assisted by a Government grant, provided that evening instruction is given in all the subjects taught, at fees which shall bring the advantages of the College within the reach of all classes. The due administration of the grant should be secured by the appointment of certain nominees of the Government on the Executive Council of the College.

THREATENED SCARCITY OF WATER.

THE appendices to the Weekly Weather Reports for the year 1887, recently published by the Meteorological Office, contain some interesting details relative to the rainfall. It is shown that the mean rainfall for the whole of the British Islands during 1887 was only 25·8 inches, whereas the mean for the twenty-two years 1866 to 1887 was 35·3 inches, so that there is a deficiency of nearly 10 inches over the whole area of the British Islands, or 27 per cent. less than usual. In the wheat-producing districts, which comprise the east of England and Scotland, the south of England, and the Midland Counties, the fall during 1887 was 21 inches, and the average value for twenty-two years is 28·5 inches, showing a deficiency in these parts of the Kingdom of 7·5 inches, or 26 per cent. less than usual. In the principal grazing districts, which comprise the west of England and Scotland, as well as Ireland, the fall in 1887 was 30·5 inches, and the value for the twenty-two years is 42·0 inches, showing a deficiency of 11·5 inches, or 27 per cent. less than the average. In the north-west of England the rainfall for 1887 was only 24·9 inches, which is 15·7 inches or 39 per cent. less than the average, and in the south-west of England the fall was 28·3 inches, which is 16·6 inches or 37 per cent. less than usual. Last year was the driest of any year since 1866, and this feature was common to all parts of the United Kingdom; the amount of rain measured was only about one-half of that recorded in 1872, which was the wettest year of the period. If the comparison is confined to the last ten years, the deficiency is nearly as marked, and 1887 is still found to be about 25 per cent. below the average, but the greatest deficiency in this case occurs in the Midland Counties, where it amounts to 36 per cent. of the average. The reports issued by the Meteorological Office for the first five or six weeks of the present year show the deficiency of rainfall still to be augmenting, and even more quickly than in any period last year. In the Midland Counties the rainfall to February 6 was only 0·6 inch instead of 2·9 inches, so that the deficiency from January 3 is as much as 79 per cent. of the average fall; and at Hereford, where the total fall is only 0·29 inch, the deficiency is 90 per cent. of the average. In the east of England the deficiency is 64 per cent., in the south-west of England 61 per cent., and in the north-west of England 58 per cent. There has been a deficiency of rain in all districts of England each week for seven consecutive weeks since December 19, with the exception of a single district (England N.E.) in one week, and since the beginning of October there have been but four weeks in which the excess of rain was at all general. Out of fifty-seven weeks since the commencement of 1887 there have been but ten in the south-west and east of England with an excess of rainfall, and only eleven in the north-west of England. With these facts to hand, there seems reasonable ground for alarm being felt in some localities at the threatened scarcity of water.

CHARLES HARDING.

PROFESSOR ASA GRAY.

WHEN the history of the progress of botany during the nineteenth century shall be written, two names will hold high positions: those of Prof. Augustin Pyrame De Candolle and of Prof. Asa Gray. In many respects the careers of these men were very similar, though they were neither fellow-countrymen nor were they contemporaries, for the one sank to his rest in the Old World as the other rose to eminence in the New. They were great teachers in great schools, prolific writers, and authors of the best elementary works on botany of their day. Each devoted half a century of unremitting labour to the investigation and description of the plants of continental areas, and they founded herbaria and libraries, each in his own country, which have become permanent and quasi-national institutions. Nor were they unlike in personal qualities, for they were social and genial men, as active in aiding others as they were indefatigable in their own researches; and both were admirable correspondents. Lastly, there is much in their lives and works that recalls the career of Linnæus, of whom they were worthy disciples, in the comprehensiveness of their labour, the excellence of their methods, their judicious conception of the limits of genera and species, the terseness and accuracy of their descriptions, and the clearness of their scientific language.

Asa Gray was born in Paris, Massachusetts, on November 18, 1810, and took his M.D. degree when twenty, at the Medical College of Fairfield, Oneida County. His proclivities were all scientific from a very early age, and he is said to have, whilst still a student, delivered lectures on chemistry, geology, and botany, in private establishments of that county. The two former subjects were at first his favourites—indeed, his earliest

contribution to science is a paper, by G. B. Crawe and himself, on the mineralogy of Jefferson and St. Lawrence Counties (N.Y.), in *Silliman's Journal* (1834, 346)—but they soon gave place to botany, owing to his having attracted the attention of Dr. John Torrey, State Botanist for New York, and Professor of Chemistry and Botany, but practically of botany only, in the New York Medical College. In 1833, Dr. Torrey made Gray his laboratory assistant, a post he held for some months, during which he presented what was his first botanical paper to the Annals of the New York Lyceum. This, which was on a very intricate and much misunderstood group of American sedges (*Rhynchospora*) showed Gray's acuteness as an observer, and skill in systematizing, as clearly as anything he has since written. In the following year he was appointed Curator of the New York Lyceum, where he extended his studies to the North American grasses and Cyperaceæ, and prepared his first botanical text-book, which was published in 1836, under the title of "Elements of Botany." The "Elements" is a noteworthy book; it was at once accepted as the best that had appeared in the States, and as second to none in the English language; its only rival was Lindley's "Introduction to the Natural System of Botany," the first edition of which had (in 1831) been reprinted under Dr. Torrey's supervision for the use of the American schools.

Whilst still attached to the New York Lyceum, Gray accepted the appointment of naturalist to Capt. Wilke's South Pacific Exploring Expedition, which was then being fitted out; but after two years' delay, and the curtailment of the opportunities for research that were to have been afforded him on the voyage, he threw up the appointment. This result is much to be deplored, for no young naturalist was ever better fitted by education, and by training as an observer and collector, to have taken advantage of the splendid opportunities which that expedition afforded.

Having relinquished the prospect of Pacific exploration, Dr. Gray was invited by his friend Dr. Torrey to co-operate with him in the preparation of a flora of the North American Continent; and into this work, which became the leading object of his scientific life, he eagerly entered. At the same time he accepted the Chair of Botany in the University of Michigan, subject to the condition of being allowed a year's leave to be passed in Europe for the purpose of verifying the nomenclature of the American flora by a study of the species of which the types existed only in European herbaria. This was in 1838, and his first visit was to Glasgow, where the then Professor of Botany (Sir W. Hooker) was engaged on a flora of British North America, under the auspices of the Secretary of State for the Colonies. After a long sojourn in Glasgow, Dr. Gray visited the principal herbaria in London, France, Switzerland, Italy, Austria, and Prussia, making life-long friendships with scientific men of all pursuits wherever he went; his letters of introduction, coupled with his bright intelligence, genial disposition, and charming personality, giving him the *entrée* to *salons* as well as to the museums of every capital. This was the first of seven visits that Dr. Gray paid to Europe, and of which the last was concluded a very few weeks before his fatal illness.

On his return to America in 1839, Dr. Gray resided at New York, when the first volume of the flora of North America was completed, in conjunction with Dr. Torrey, and the second, elaborated wholly by himself, was begun, but not completed till 1843. In the meantime (in 1842) he had been appointed by the Fellows of Harvard College, Cambridge, Fisher Professor of Natural History, the duties of which Chair were restricted to an annual course of lectures on botany, and the charge of the College Botanical Gardens, to which an official residence is attached. This was his home for the rest of his life, and here, with funds partly derived from the College, and partly from private sources, largely supplemented by interchanges of specimens and books, he founded the Harvard Herbarium and Library.

The great desideratum for the conduct of Dr. Gray's new duties was a much fuller class-book of botany than the "Elements" of 1836, and in the same year he completed the first edition of his "Botanical Text-book." In this the subject-matter is divided into two parts, one devoted to structural and physiological botany, the other to the principles of systematic botany, including chapters upon plants useful to man. This was the first of a series of editions of a work that has been for nearly half a century the text-book of schools and colleges throughout the United States, and the latter issues of which have been generally recommended by the botanical professors of the United Kingdom as the best of its class. In 1880 the first volume of the sixth edition appeared, but the advances in botanical science made since the fifth was published, quite a quarter of a century before, had been so many and great that the amount of matter which this sixth will contain is quadruple that of the fifth. It will be when complete a co-operative work in four volumes. The first is by Gray himself, and is devoted to morphology, taxonomy, and phytography; the second, by Prof. Goodale, Gray's able successor in the Fisher Professorship, includes vegetable histology and physiology; the third, by Prof. Farlow, will treat on Cryptogams; and the fourth, which Dr. Gray reserved for himself, was intended to be occupied by the classification of Phænogams, their special morphology, distribution, and products. Gray's other educational works are: "Lessons in Botany and Vegetable Physiology," somewhat on the plan of Lindley's "School Botany," but much fuller; also two smaller works, that for charm of matter and style have no equal in botanical literature—"How Plants Grow," and "How Plants Behave"—they rival chapters in Kirby and Spence's introduction to entomology in instruction and interest; and lastly, "Field, Forest, and Garden Botany."

The great outcomes of Gray's labours in systematic botany are his works on the flora of North America, from the Arctic islands to Mexico, and from the Atlantic to the Pacific Ocean. In one form or another these embrace a great proportion of the 10,000 or 12,000 species which that continent is supposed to contain. More than half are included in the two volumes published in conjunction with Torrey, and in his "Synoptical Flora," of which two parts are published, and in his "Manual of the Botany of the Northern States." The remainder are described or mentioned in more or less detail in multitudes of detached papers, and especially in memoirs upon collections made by naturalists attached to the many Expeditions organized by the Government for the exploration of railway routes across the continent, and by collectors and private individuals in previously unexplored regions. It was the hope of their author that the publication of these collections would have accelerated the completion of the general flora, and such would have been the case had their author lived; but as it is, the stars have in great measure obscured the planet, for one of the greatest obstacles to the study of North American plants is the multitude of these detached memoirs, with complicated titles, in which so many genera and species are to this day buried. If the great work cannot be continued, as it is to be hoped it may be, by Dr. Gray's most competent herbarium keeper, Sereno Watson, it is most desirable that the contents of these memoirs should be reduced to a systematic form with the least possible delay.

Next to the synoptical flora, Dr. Gray's most original work is his "Genera Floræ Boreali-Americana Orientalis," which was intended to contain descriptions, with figures, of every genus of the Eastern States, with discussions upon their affinities, morphology, and distribution. Only two volumes had appeared when want of funds decreed its end. As a fragment it is unique, and had it but been

completed it would have been of great morphological value. To have done this would, however, have required more than the ten volumes that were regarded when the work was commenced as sufficient to complete it, and this independently of the Cryptogams.

Nor was Dr. Gray's all closet work: he diligently collected and observed over a considerable area of his native continent; along the Altantic coast from Canada to Florida; in the prairie and Rocky Mountain regions from Wyoming to the borders of New Mexico; in the great basin of Utah and Nevada; and along the Pacific coast from Oregon to St. Barbara.

With two notable exceptions, Dr. Gray confined his descriptive work to North American botany. These exceptions were: one, the fragmentary botany of Wilkes's South Pacific Exploring Expedition, with the execution of which he was intrusted, but which came to an end before it was half finished, for want of funds it is believed, after the publication of one quarto volume with a superb atlas of plates; the other is a memoir on the flora of Japan, founded chiefly on the collections made in that country by the United States North Pacific Exploring Expedition, which in point of originality and far-reaching results was its author's *opus magnum*. By a comparison of the floras of Japan with those of Eastern and Western America, and of these with one another, and all with the Tertiary floras of the Northern States, he drew in outline the history of the vegetation of the north temperate hemisphere in relation to its geography, from the Cretaceous period to the present time. It is a brilliant generalization, bearing the unmistakable stamp of genius.

It remains to allude to Gray's admirable defence of the doctrine of "the origin of species by natural selection," of which he, as one of a favoured few, had been fully informed by Darwin himself in 1857 ("Life and Letters," ii. 120), before it appeared in the Linnean Journal. His opinion, which was, from the first, cautiously favourable, but with reserve, soon ripened into a conviction of the truth of the principles involved. He alludes to it first in the concluding remarks to his essay on the flora of Japan, cited above, published in 1859, wherein he says that he is "disposed to admit that closely related species may, in many cases, be lineal descendants from a pristine stock." Again, in a letter to Mr. Darwin, dated early in July 1860, speaking in terms of highest praise of the "Origin," the following passages occur:—"The moment I understood your premisses, I felt sure that you had a real foundation to hold on. Well, if one admits your premisses, I do not see how he is to stop short of your conclusions, as a probable hypothesis at least." And, referring to his own review of it in *Silliman's Journal* (March 1860), he says:—"It naturally happens that my review of your book does not exhibit anything like the full force of the impression the book has made upon me. Under the circumstances, I suppose I do your theory more good here by bespeaking for it a fair and favourable consideration, and by standing non-committed as to its full conclusions, than I should if I announced myself a convert; nor could I say the latter with truth." It may be remarked here that just at this time a battle over species was raging in America, of which but faint echoes reached our shores. This was over the question of the single or multiple origin of species by creation. Gray was the champion of single creations, and, believing himself strongly supported by theological considerations, may well have felt that the further leap to evolution was one into the dark. Be this as it may, for the five years following the publication of the "Origin," Gray devoted himself to impressing upon the American public his opinion of its extraordinary merits by reviews in weekly and monthly periodicals, by lectures, and by discourses at scientific Academies. Latterly (in 1876) he collected many of these into a single volume which he entitled "Darwiniana." In it he defines his own position "as one who is scientifically, and in his own fashion, a Darwinian, philosophically a convinced theist, and religiously an acceptor of the 'creed commonly called the Nicene,' as the exponent of the Christian faith." From this position he never moved, and he subsequently delivered two lectures in further exposition of these views, at the Divinity School of Yale College. These were published in 1880, under the title of "Science and Religion." Finally, Mr. Darwin, whilst fully recognizing the different stand-points from which he and Gray took their departures, and their divergence of opinion in some important matters, regarded him as the naturalist who has most thoroughly gauged his work, and as a tower of strength to himself and his cause.

As a reviewer and bibliographer, Gray's labours must have been Herculean, and they were uninterrupted for nearly half a century. Even when on his travels in Europe, he was in the habit of contributing scientific notices to periodicals in the States. In 1836 he commenced writing reviews of botanical works, and notices of botanists, travellers, and collectors for *Silliman's Journal of Science and Arts*; and this function he continued to perform without intermission (latterly as a co-editor of that important periodical) till within a few days of his last illness. The number of these articles is truly astonishing, as is the knowledge they display of all branches of botany, Phænogamic and Cryptogamic. They are without exception just, sober, and discriminating, critical rather than laudatory, and eminently considerate in tone where censure is necessary. A selection from these, many being discussions full of original matter and suggestive observations, would be an instructive and acceptable contribution to the botanical literature of the century, and a meet tribute to their author's merits and memory.

Dr. Gray's figure and features were familiar in the scientific circles of this country; but for the information of others it may be stated that he was of spare, wiry figure, rather below the average height, his expression was keen and vivacious, and his movements, like his intellect, alert. He was a Foreign Fellow of the Royal and Linnean Societies, a correspondent of the Institute of France, and of the other Continental Academies, a Doctor of Laws of Oxford, Cambridge, and Edinburgh, and had served as President of the American Academy of Arts and Science, of the American Association for the Advancement of Science, and as a Regent of the Smithsonian Institution. Accompanied by Mrs. Gray he spent the summer of 1887 in Europe, chiefly in England, returning to Cambridge in September. In October he went to Washington on the affairs of the Smithsonian Institute. Soon after his return, on the 28th of November, he was struck with paralysis, from which he never rallied, and he died at the end of the following January. It is characteristic of him that his last letter, written in pencil immediately before his seizure to the contributor of these lines, was on the subject of a review for *Silliman's Journal* of Planchon's "Review of the Vines." Dr. Gray married in 1848, Jane, daughter of Judge Charles G. Loring, of Boston, who survives him. He left no family. An excellent medallion likeness of him in bronze was, on his seventy-fifth birthday, presented by his friends to Harvard College, Cambridge, U.S.

J. D. H.

NOTES.

On Tuesday evening a question was asked in the House of Commons, by Mr. Howorth, about the new regulations for the entrance examination at Woolwich. Mr. Howorth inquired whether these regulations were final and permanent, or only temporary. Mr. Stanhope, we regret to say, replied that the regulations are intended to be of permanent application. If that be so, it is the more necessary that a vigorous protest against the scheme should be made by those who have any

true appreciation of the place which properly belongs to the study of science in education.

On Tuesday the Committee of the Athenæum Club elected three new members in accordance with the rule which empowers the election of nine persons annually "of distinguished eminence in science, literature, or the arts, or for public services." The names of two of the new members are familiar to students of science—Major-General Donnelly, R.E., C.B., Secretary to the Department of Science and Art; and Prof. G. Carey Foster, F.R.S.

One of the leading native residents of Bombay, Sir Dinshaw Petit, has just given the Bombay Government a property valued at 300,000 rupees for the establishment of the proposed Technical Institute of Bombay.

We regret to announce the death of Dr. Maximilian Schmidt, an eminent geologist and Director of the Zoological Gardens at Berlin. He was born at Frankfurt in 1834, and died at Berlin.

Emile Rousseau, the well-known French chemist, died on the 4th inst., at the age of seventy-three.

The annual general meeting of the Fellows of the Royal Horticultural Society was held on Tuesday, Sir Trevor Lawrence, President of the Society, in the chair. The special Committee appointed to inquire into the working of the Society recommended that premises in No. 111 Victoria Street should be secured, and that for shows and meetings the Society should hire the drill-hall of the London Scottish Rifle Volunteers. The Committee also reported that they had under consideration "the construction of new by-laws intended to facilitate the carrying out of as complete a horticultural policy as possible—one in which all aspects and departments should be considered to the undue preponderance of none; but to the general advantage of all." After some discussion the Committee's recommendations were adopted. Several members of the Council having placed their resignation in the hands of the Fellows, it was resolved on the motion of Mr. Wilks, seconded by Mr. Veitch, to decline to accept their resignation, and they were then all formally re-elected, including Sir Trevor Lawrence, who was re-appointed President.

The Calendar and General Directory of the Science and Art Department for 1888 has been issued.

The curious fact was some time ago brought to light by Nahrwold that solid particles are ejected from a platinum wire glowing under the influence of an electric current, and form a metallic incrustation upon the walls of a glass tube by which the wire is surrounded. The cause of the emission of these solid particles of platinum has, however, until recently, remained a complete mystery. In the number of the *Annalen der Physik und Chemie* just received will be found an interesting paper by Dr. Alfred Berliner, who, in the course of a series of experiments upon the occlusion of gases by platinum and palladium, has discovered the source of this singular phenomenon. Thin strips of platinum, before being charged with the gas under experiment, were inclosed in a narrow glass tube, and freed from all occluded gas by being heated to redness, *in vacuo*, by the passage of a constant electric current for several hours. At the expiration of this time the metallic incrustation was invariably found when occluded gas had been evolved. On charging the strips with various quantities of any particular gas, the amount of incrustation formed after the complete expulsion of the gas in each experiment was found to vary in the same proportion. Hence it appears pretty clear that the evolution of gas is necessary for the emission of solid particles. This result is strongly confirmed by the fact that palladium, which has such a remarkable power of occluding gases, produces a similar incrustation much more readily and at a lower temperature. It appears probable that the action is merely mechanical; that we have, in fact, an immense number of microscopic volcanoes or solfataras, evolving the occluded gases with such energy that portions of the crater walls are detached and carried away by main force, like their brethren on the large scale, the scoriæ and lapilli, to distances very considerable in comparison with the size of the vents.

The next meeting of the French Association for the Advancement of Science will be held in Oran from March 29 to April 3. Interesting excursions will be made to Biskra and other places.

At the meeting of the Meteorological Society of France, on December 20, M. E. Lemoine was elected President for the ensuing year. M. Renou made a communication on the relations which exist between the annual number of solar spots and thunder-storms in various places, and concluded that the works hitherto published were far from sufficing to show any direct dependence between the two phenomena. At the meeting of January 11, M. Janssen, the retiring President, expressed his opinion of the necessity of organizing meteorological stations on a uniform plan at a certain number of stations, under the superintendence of professional and paid observers on whom a definite programme could be imposed, instead of having volunteer observers; this view was also supported by M. Renou. The latter gentleman also spoke of the importance of adopting a uniform cloud nomenclature, and announced that he would shortly present to the Society a work upon this subject. The general secretary presented a note from M. Garrigou-Lagrange on a new electrical anemometer giving the direction of the wind, and the horizontal and vertical components of its force (see Nature, November 3, 1887, p. 18), and recommended its adoption at some coast stations.

The Council of the Royal Meteorological Society, 30 Great George Street, Westminster, S.W., have appointed a Committee to collect observations on British hail- and thunder-storms from volunteer observers. The objects which they hope to attain thereby are:—(1) A knowledge of the nature and causes of the different kinds of thunder-storms, their attention having been specially called to the subject by the great loss of life and property during the past summer. (2) A discovery of the localities where hail and thunder are most frequent and destructive. (3) If possible, to obtain an increased power of forecasting hail and thunder, whereby they hope that eventually damage to persons, stock, and property might be lessened. Forms and instructions will be sent to intending observers.

We have received a "Brief Sketch of the Meteorology of the Bombay Presidency in 1886-87." It is by Mr. F. Chambers, Meteorological Reporter for Western India. Mr. Chambers points out that the meteorology of the year 1886-87 in the Bombay Presidency presents several features of special interest. There was a decided reappearance, for some time, of almost exactly the same unfavourable meteorological conditions which characterized the year 1877, when the rainfall in many parts of the Presidency was disastrously deficient. Fortunately, these unfavourable conditions did not last long enough to produce distress, for although a prolonged break in the rains caused considerable anxiety for a time, an excessive fall of rain late in the season brought relief, and on the whole the year was a favourable one.

A severe earthquake occurred in Grenada on January 10. A rumbling noise was immediately followed by a slight shock and gentle lateral oscillations. Then came a very violent shock and vertical undulating oscillations. These were succeeded by gentle oscillations. The shock is supposed to have lasted from twenty to thirty seconds. Several houses in the town of

St. George's were so much damaged that they cannot be safely occupied. The walls of the St. David's Roman Catholic Church are so seriously injured that they will have to be taken down in some places and rebuilt. The sacristy was all but demolished; the basement wall of the presbytery was thrown down, and the building itself had to be propped up with posts. The Court House in the same parish sustained considerable injury. The tower of the Anglican Church in Grenville was cracked. During the succeeding week several mild shocks of earthquake were experienced in the island, the strongest of which occurred between 7 and 8 o'clock on the night of Sunday, January 15.

DR. FORBES WATSON'S collection of commercial products, which was lately offered to the University of Aberdeen at the comparatively small price of £250, has been bought by Dr. Carnelley and his father, and presented to University College, Dundee. Prof. D'Arcy W. Thompson, at whose suggestion the purchase was made, writes to the *Dundee Advertiser*: "This excellent gift puts us at once in possession of a museum which is first-class of its kind, and of which town and College should be proud for ever. Dr. Forbes Watson is well known to many of the older men in Dundee for his knowledge of jute and all other commercial fibres. His works are standard on the subject. His collection was amassed with unrivalled opportunities and the highest technical skill. Great part of it was brought together as an official duty for the India Museum, and was presented by the Department to Dr. Forbes Watson when that Museum was broken up. It contains nearly 7500 samples. Between 700 and 800 of these are fibres, including textiles and paper-making materials. There are over 500 dyes and dye-stuffs, 500 oils and oil-seeds, 600 or 700 gums, resins, and guttas, nearly 2000 medicinal substances (may they be useful to us in the future), and more than as many samples of food-stuffs. The bulk of the collection is stored in bottles, filling fourteen cabinets, and there are also stands and cases for the display of specimens. Altogether the cases and bottles in which this great collection is stored represent a cost greater than the price which Dr. Forbes Watson now asks and receives."

THE Batavia *Niewsblad* announces that the Government has decided upon establishing a bacteriological laboratory in that town. An institution for the pursuit of that special branch of study will be built immediately the funds for the purpose become available. The existing laboratory arrangements will be improved and extended, so as to admit of pathological and bacteriological investigations.

WE are glad to learn from the *American Naturalist* that the project of a Marine Biological Laboratory on the New England coast is not languishing. Several thousand dollars have already been subscribed towards the erection of the necessary building and its equipment and maintenance. The Committee on the Laboratory have arranged a course of eight lectures, the proceeds of which are to be added to the fund.

LIEUT. NIBLACK, U.S.N., has returned to Washington from a three-years' voyage to Southern Alaska, where he has been engaged on coast-survey duty. He has brought with him many photographs and objects which will be of interest to students of ethnology and anthropology. He devoted special attention to the totem posts of Southern Alaska. He says that in that country winter is the best season for ethnological studies. The natives are then at home, whereas in the summer they are often far inland.

ASSISTANT CHARLES A. SCHOTT, assistant in charge of the Computing Division of the United States Coast and Geodetic Survey, has addressed a letter to the Superintendent of his Bureau about the recent discovery, by Assistant G. Davidson, of records of the magnetic declination, A.D. 1714. He says that these records greatly increase our knowledge of the secular variation of the declination. *Science* gives the following account of the substance of M. Schott's letter:—"By means of these observations we are able to improve materially the expressions for San Blas and Magdalena Bay, to add the new station Cape San Lucas, and to make their influence felt as far north as San Diego and Santa Barbara. It is the range which is greatly improved; besides, the epoch of maximum declination is shifted in the right direction. Apart from the fact that a region of west declination is here for the first time observationally indicated on the Pacific coast, the power of the newly recovered declinations is due to the circumstance, that, as far as known, they cover a time when the needle was in or near a phase the opposite of the present one. For want of early observations, those previously collected for San Diego and Santa Barbara, Cal., were extremely difficult to handle; and, while it was not an easy matter to establish new expressions for these stations, their correctness, or rather applicability over the whole period of time the observations cover, is quite reassuring. He points out the desirability of new observations (either using funds yet available before July next, or providing funds to be used after that date) at San Diego, Santa Barbara, and Monterey, and states that these stations have received no attention for seven years. These observations are demanded to give greater precision to the computed variations on our charts."

A NUMBER of American geographers, all belonging to Washington, have incorporated the American National Geographical Society for a term of 100 years. The principal objects of the Society are to increase and diffuse geographical knowledge, to publish the Transactions of the Society, to publish a periodical magazine and other works relating to the science of geography, to dispose of such publications by sale or otherwise, and to acquire a library under the restrictions and regulations to be established by its by-laws.

THE Commissioners of Public Schools of Baltimore, Md., deserve much credit for the efforts they are making to secure for the schools under their care important reforms which have always been advocated by students of sanitary laws. They lately resolved that the Mayor and City Council of Baltimore should be requested to authorize them to appoint an officer, to be known as the Sanitary Superintendent of Public Schools, whose duty would be: (1) to carefully examine all plans submitted for the construction of new school-houses, and suggest such modifications as may be necessary from a sanitary point of view; (2) to advise with the Commissioners with reference to necessary alterations in school-buildings to improve their hygienic condition; (3) to examine all text-books before adoption, in order that type, printing, or paper injurious to the eyesight of pupils may be avoided in selecting such books; (4) to satisfy himself, by personal examination if necessary, that all pupils admitted to the schools have been properly vaccinated, or are otherwise protected against small-pox; (5) to take such other measures, in conjunction with the Health Commissioner of the city, as may be necessary to prevent the spread of contagious diseases in, or through the medium of, the public schools; (6) to examine annually the eyesight of all children attending the public schools, and keep an accurate record of such examinations; (7) to report annually, or as often as may be required by the Commissioners, upon the sanitary condition of the schools, and of the pupils attending them, and to advise the Commissioners upon sanitary questions connected with schools whenever required; (8) to give instruction, by lectures or otherwise, to the teachers in the schools upon the elementary principles of school hygiene.

WE have received the second volume of the Transactions of the Meriden (Conn., U.S.A.) Scientific Association. It contains, among other things, a valuable list, drawn up by Mr. Franklin Platt, of the birds of Meriden.

A THIRD edition of Mr. John Venn's "Logic of Chance" (Macmillan) has just been issued. The work has been re-written, but the author explains that the alterations he has made do not imply any appreciable change of view on his part as to the foundations and province of probability. In the preface Mr. Venn mentions that he is engaged in preparing a work on inductive logic.

THE scientific writings of Jean Méry (1645–1722) have been brought together in a work edited by M. L. H. Petit, Assistant Librarian to the Medical Faculty of Paris. The work contains many contributions to biology which have not hitherto been generally known.

TOME III., Cahier I, of the "Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux," is devoted to a full bibliography of the Γ function.

WE have received the *Annuaire de l'Observatoire Royal de Bruxelles* for 1888, this being the fifty-fifth year of its publication. It contains the usual astronomical tables and data for the current year, and a mass of meteorological, geographical, financial, and other statistics. There is a complete list of the asteroids and comets discovered in the past year, with the elements of their orbits, and there is also an account of the state of solar activity in 1886. The tables of units and physical constants have been considerably extended, and a detailed account of electrical and magnetic measurements has been added. In addition there are, as usual, several scientific papers by the officials of the Observatory. M. Folie gives an account of his further investigations into the movements of the earth's axis, and in a subsequent article M. Niesten applies the correction for diurnal nutation to the various and widely differing determinations of the annual parallax of γ Draconis; the value obtained is $+0''\cdot086$. The important series of barometric observations, extending over a period of fifty years, are discussed at great length, and illustrated by diagrams.

MR. JOHN HEYWOOD, of Manchester and London, has issued a little book called "Flower-Land," by the Rev. R. Fisher. It is written in a simple style, and will no doubt be useful as an introduction to botany for children.

THE Perthshire Society of Natural Science has begun to issue Transactions and Proceedings. We have received the first part of the first volume. It contains notes on some rare birds lately placed in the Society's Museum, by Colonel H. M. Drummond Hay; mnium riparium, by R. H. Meldrum; some localities for Perthshire plants, by R. H. Meldrum; origin of the interbedded and intrusive volcanic rocks of Kinnoull Hill, by H. Coates; and the flora of the Woody Island, by W. Barclay.

MESSRS. KEGAN PAUL, TRENCH, AND CO. will publish immediately, in the "International Scientific Series," Sir J. William Dawson's new book entitled "The Geological History of Plants."

"A HISTORY OF PHOTOGRAPHY," by Mr. W. J. Harrison, will shortly be published. It is intended to serve as a practical guide to photography, and as an introduction to its latest developments.

A WORK containing a full account of the volcanic eruptions for the last sixty-four years on the island of Hawaii has been printed and will shortly be published in the city of Honolulu. The author is Mr. William Lowthian Green, at present Prime Minister of the kingdom of Hawaii, whose work on "The Vestiges of the Molten Globe" appeared in England some years ago, and has since attracted the attention of M. Lapparent and other Continental geologists. Mr. Green's new work will contain a complete tabular statement of the eruptions, and a map of the island of Hawaii.

MR. EDGAR THURSTON, Superintendent of the Government Central Museum, Madras, has printed a list of fishes obtained during a residence of three weeks at Râméswaram Island, which is separated on the one hand from the Indian continent, and on the other from the Island of Manaar, by an interrupted ridge of rocks known as Adam's Bridge. The fish-fauna of the coral reefs of this island stands out in striking contrast with that of other places on the Madras coast. "Coral Fishes," *i.e.* brightly coloured fishes—*Chætodon*, *Platyglossus*, *Heniochus*, *Pseudoscarus*, &c.—abound round the reefs, and feed either on the small delicate marine Invertebrata which swarm on the living corals, or, if their teeth are adapted for the purpose, on the hard calcareous substance of the corals. The bright colouring of the fishes is explained by Mr. Thurston on the well-known principle that "the less the predominant colouring of any creature varies from that of its surroundings, the less will it be seen by its foes, the more easily can it steal upon its prey, and the more it is fitted for the struggle for existence." Conspicuous by their abundance are several species belonging to the family Sclerodermi, including *Balistes* (file-fish), whose jaws are armed with teeth well suited for breaking off pieces of hard coral, or boring holes into the shells of the Mollusca, on the soft parts of which they feed. The file-fishes are said to destroy an immense number of mollusks, thus becoming most injurious to the pearl-fisheries. Present, too, in great numbers are several species of the family Gymnodontes: *Tetrodons* (globe fishes), including the beautifully marked little *Tetrodon margaritiferus*, and *Diodons*, which have a very bad reputation among the natives as being very poisonous.

IT is generally supposed that the Ainos of Yezo are amongst the disappearing races of the earth, and that they are "fast dying out," as the phrase usually runs. This appears to be an error, for according to a communication on the subject in the *Japan Weekly Mail*, from Mr. Bachelor, during the past fifteen years there has been little, if any, diminution in their number, which he puts down, so far as the Island of Yezo is concerned, at from 1300 to 1600 souls. Actual detailed statistics respecting the numbers of the Ainos do not appear to be given in the Japanese censuses, but official statistics do exist for certain Aino settlements since 1871, which may be taken as an index. These show an increase of 129 persons in sixteen years, although, by a careful examination of the data, it appears that one village not included in the earlier was given in the later years. In 1871, there were 665 males, 639 females, and 260 huts; in 1886, the numbers were 691, 742, and 318 respectively. These figures, making every reasonable allowance, show at least that there is good ground for doubting whether the Ainos are dying out, in Yezo at least, as rapidly as it is the fashion to assume that they are.

THE seventh annual meeting of the members of the Sanitary Assurance Association was held at their offices, 5 Argyll Place, Regent Street, on Monday. Prof. Roger Smith presided, and expressed his satisfaction at the continued prosperity of the Association.

IN the footnote in NATURE, December 15, 1887, p. 152, second column, line 5 from foot, *for* Careton *read* Cureton.

THE additions to the Zoological Society's Gardens during the past week include a Common Boa (*Boa constrictor*) from South America, a Royal Python (*Python regius*), two West African Pythons (*Python sebæ*) from West Africa, an Indian Python (*Python molurus*) from India, presented by Mr. Leopold Field; a Griffith's Fox (*Canis griffithi*) from Persia, two Cockateels (*Calopsitta novæ-hollandiæ*) from Australia, deposited; a Pluto Monkey (*Cercopithecus pluto*) from West Africa, an Antarctic Skua (*Stercorarius antarcticus*) from the Antarctic Seas, received in exchange.

OUR ASTRONOMICAL COLUMN.

MELBOURNE OBSERVATORY.—The Annual Report of this Observatory, dated August 14, 1887, states that the buildings and equipment of the Observatory were in good condition with the exception of the mirrors of the great Cassegrain reflector, which had become so dull as materially to interfere with the observation of the fainter nebulæ. It was proposed to substitute mirror A, the less tarnished of the two, for mirror B, now in the telescope, and either to have B repolished on the spot or to send it to Dublin to be re-polished under the care of Sir H. Grubb. The new transit circle was in excellent order, and 2487 right ascensions and 1301 polar distances had been observed during the year. Eighty-seven southern nebulæ had been examined with the great reflector, and four searched for, but not found. The use of the photo-heliograph, which had been altered in July 1886, so as to take pictures on a scale of 8 inches to the solar diameter, had been much interfered with by bad weather, and only 121 photographs had been secured. The principal fresh work proposed for the Observatory was the co-operation in the photographic survey of the heavens; the Victorian Government having consented to the Observatory joining in that undertaking, and having placed £1000 on the estimates of the current year towards the necessary expenditure.

THE AMERICAN NAUTICAL ALMANAC OFFICE.—The Report of Prof. Newcomb, Superintendent of the Office, for the year ending 1887 June 30 has recently appeared. From this we learn that the printing of the several Nautical Almanacs published by the Office fell a little into arrear in 1887, the printing of the American Ephemeris for 1890, which should, according to custom, have appeared in June, not being quite ready in October. The computations for the following years were in their usual state of forwardness. The principal part of the Report deals with the new tables of the planets on which Prof. Newcomb and his assistants are engaged. The work is divided into four sections—viz.: (I.) The computation of the general perturbations of the planets, the work now in hand relating to those of the four inner planets; on twelve of the fourteen pairs of planets which come into play in this part of the undertaking, the work has already been completed. The incomplete perturbations are those of Venus and Mars by Jupiter. (II.) The re-reduction of the older observations, and discussion of the later ones, with a view of reducing them all to a uniform system. In this section Maskelyne's Greenwich observations from 1765 to 1811, and Bradley's, 1750 to 1762, have been already reduced, the latter by Dr. Auwers. Airy's Greenwich observations, the Paris observations from 1800 as reduced by Leverrier, and Bessel's Königsberg observations, will need no discussion except that necessary to reduce them to the adopted standard system. The re-reduction of Piazzi's Palermo observations, 1791–1813, is in hand, but it is not yet decided as to whether Taylor's Madras observations should be included. (III.) The computation of tabular places of the planets from Leverrier's tables up to the year 1864—the most laborious and difficult part of the work—is in a fairly advanced state. (IV.) The final discussion of the results. Prof. Newcomb estimates that the equations of condition for correcting the elements of the four inner planets will be ready for solution towards the end of 1889, but they will involve extended discussion and comparison in order to get the results in the final form for publication. Of the work on the four outer planets, Uranus and Neptune are yet untouched; but Mr. Hill's new theory of Jupiter and Saturn is in the hands of the printer, and Mr. Hill is now engaged in the construction of the tables and ephemerides for finally correcting their elements. In connection with the lunar theory, the principal work on hand is the comparison of Hansen's tables with observed occultations since 1750. Another branch of the planetary work is the determination of the mass of Jupiter from the motions of Polyhymnia: the perturbations of the planet have been computed from its discovery in 1850 to October 1888, and the work awaits the observations during the approaching opposition to be brought to a final discussion.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 FEBRUARY 19–25.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 19

Sun rises, 7h. 9m.; souths, 12h. 14m. 6·2s.; sets, 17h. 19m.: right asc. on meridian, 22h. 9·7m.; decl. 11° 22′ S. Sidereal Time at Sunset, 3h. 15m.

Moon (at First Quarter February 20, 2h.) rises, 10h. 25m.; souths, 17h. 46m.; sets, 1h. 18m.*: right asc. on meridian, 3h. 42·8m.; decl. 14° 34′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	7 29	13 17	19 5	23 13·3	3 7 S.
Venus.....	5 39	9 49	13 59	19 44·5	20 46 S.
Mars......	22 38*	3 57	9 16	13 51·7	8 43 S.
Jupiter....	2 3	6 17	10 31	16 12·1	20 12 S.
Saturn....	14 20	22 16	6 12*	8 13·4	20 29 N.
Uranus...	21 36*	3 9	8 42	13 3·3	6 0 S.
Neptune..	10 5	17 45	1 25*	3 41·7	17 56 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Feb.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
20	Aldebaran	1	15 56	near approach	346 —
21	119 Tauri...	5½	19 10	near approach	7 —
24	*d′* Cancri...	6	21 30	22 28	112 226

Feb.	h.	
23	1	Mercury stationary.
24	20	Saturn in conjunction with and 1° 22′ north of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Feb. 19,	19 38	*m*
			,, 24,	19 17	*m*
R Arietis	2 9·8	24 32 N.	,, 22,		*M*
R Tauri	4 22·2	9 55 N.	,, 21,		*M*
R Leporis	4 54·5	14 59 S.	,, 23,		*M*
R Canis Majoris...	7 14·5	16 12 S.	,, 21,	20 26	*m*
			,, 22,	23 42	*m*
δ Libræ	14 55·0	8 4 S.	,, 22,	1 58	*m*
U Coronæ	15 13·6	32 3 N.	,, 21,	21 51	*m*
U Ophiuchi	17 10·9	1 20 N.	,, 19,	3 2	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·5	27 47 S.	Feb. 19,	2 0	*M*
W Sagittarii	17 57·9	29 35 S.	,, 19,	4 0	*M*
Z Sagittarii	18 14·8	18 55 S.	,, 25,	1 0	*M*
β Lyræ	18 46·0	33 14 N.	,, 24,	0 0	*M*
S Sagittarii	19 12·9	19 14 S.	,, 24,		*M*
δ Cephei	22 25·0	57 51 N.	,, 20,	2 0	*M*
			,, 23,	20 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near β Trianguli...	30°	35° N.	February 24.
From Canes Venatici...	181	34 N.	February 20.
Near δ Serpentis...	234	11 N.	Swift; streaks.
,, π Herculis...	262	36 N.	Feb. 20. Swift.

GEOGRAPHICAL NOTES.

AT Monday's meeting of the Royal Geographical Society, the paper read was by Mr. Randle F. Holme, on Labrador, which he visited in July–October of last year. Mr. Holme succeeded in penetrating into the heart of Southern Labrador, as far as Lake Waminikapou, and not far from the Grand Falls, which Mr. Holme believes will turn out to be the greatest falls in the world; but, as General Strachey pointed out in the discussion, Mr. Holme's conception of the height is probably exaggerated. Mr. Holme went from Newfoundland to Bonne Espérance on the south-east coast of Labrador, and sailing northwards touched at several points, proceeding up Hamilton Inlet and the Grand River, to the point mentioned above. Mr. Holme found many difficulties in the way, and much of the country he visited was virtually unexplored. With regard to the height of the Grand Falls, Mr. Holme states that the centre of Labrador, as is generally known, is a vast tableland, the limits of which are clearly defined, though

of course the country intervening between this limit and the coast always consists, more or less, of a slope. Roughly speaking, it may be said that in the south and north there is a more or less gradual slope from the height of land to the coast, while in the south-east the descent is sudden, and almost immediately after leaving the tableland there is reached a level which is but little above that of the sea. In the north-east portion the edge of the tableland approaches nearest to the coast, while it trends considerably to the west in the rear of Hamilton Inlet. The most fertile part of the country is that which lies between the tableland and the sterile belt on the coast, though the height of land itself is by no means a desert. On the height of land there is found a succession of great lakes joined together by broad placid streams. When the streams of water reach the edge of the tableland, they of course commence a wild career down towards the sea. In the case of the Grand River this rapid descent commences with the Grand Falls, and almost the whole of the great drop to the sea-level is effected in the one waterfall. The elevation of the Labrador tableland is given by Prof. Hind as 2240 feet. From this height the Moisie and Cold Water Rivers descend to the sea by means of a considerable number of falls. But in the Grand River below Lake Waminikapou there is only one fall, viz. that which occurs 25 miles from the river-mouth. This fall is 70 feet. It is true that the whole of the river from Lake Waminikapou to the First Falls is rapid, but there is no place where there is any considerable drop, and indeed no place where it is necessary to take the boat out of the water. Now the lake first above the Grand Falls is on the height of land. In the channels joining the various lakes above the falls there are no rapids and there is scarcely any stream. It therefore follows, assuming the elevation of the tableland on the east to be approximate to that on the south, that in the 30 miles beginning with the Grand Falls and ending with Lake Waminikapou, there is a drop of about 2000 feet. Some of this drop is probably effected by the rapids immediately below the falls, but the greater part is no doubt made by the fall itself. The river is said by Maclean to be 500 yards broad above the falls, contracting to 50 yards at the falls themselves. The interior of the country Mr. Holme found was richly wooded, and the climate mild, though the plague of flies and mosquitoes was almost intolerable. The few Indians who inhabit Labrador belong mostly to the Cree nation, and according to Mr. Holme are probably perfectly unmixed with either whites or Eskimo. As an agricultural or pastoral country Mr. Holme thinks Labrador has no future, though something may be made of its iron, of the existence of which strong indications exist. Mr. Holme's observations have enabled us greatly to improve our maps of Labrador, and the photographs he brought home give an excellent idea of the general character of the country.

OUR ELECTRICAL COLUMN.

SOME very interesting and remarkable trials of the transmission of energy were recently made between Kriegstetten and Solothurn in Switzerland, by Prof. H. F. Weber and others, when it was found that 30 horse-power put in at the first place delivered 23 horse-power at the other, 8 kilometres away—showing an efficiency of 75 per cent. The current, 11 amperes, driven under an E.M.F. of 2000 volts, showed absolutely no loss whatever, owing to the use of Johnson and Phillips' "oil" insulators. This mode of insulation proved absolutely perfect.

THE distribution of electricity for lighting purposes by means of secondary generators, is now being discussed at the Society of Electrical Engineers. This mode of working seems to have solved the question of the economical erection of conductors. Alternate currents of high tension in the main conductors allow wires of small diameter to be used, and a special form of induction coil transforms these currents of high tension, 2000 volts, to currents of low tension, 100 volts and under, for use in private houses. The system, due to Messrs. Gaulard and Gibbs, is in use at the Grosvenor Gallery installation, as well as at Eastbourne and Brighton, and is probably going to be largely used. Mr. Kapp's paper "On Alternate Current Transformers, with Special Reference to the Best Proportion between Iron and Copper," will lead to an interesting discussion. All induction coils, when used as transformers, are simply a magnetic circuit or closed iron core interlaced with an electric circuit or a closed copper core, and constructed so that the electric circuit shall embrace as many as possible of the lines of force of the magnetic circuit. Mr. Kapp divides transformers into two classes—one in which the copper coils are spread over the surface of the iron core as in a Gramme armature, and the other in which the iron core is spread over the surface of the copper coil. The former he calls "core transformers," and the latter "shell transformers." He advocates working transformers at low inductions—that is, far below the point of saturation of iron—because it increases the plant-efficiency, reduces the heat or energy lost in the iron core through hysteresis, and prevents the production of sound. The plant-efficiency of transformers sometimes reaches as high as 99 per cent., and they are perfectly self-regulating. There is very little choice between core- and shell-transformers, but the former have the advantage. Economy in construction and facility in manufacture and repair seem to be principal points of advantage to reach. It is amusing to find how, now that the system has proved to be practical, every man is devising his own transformer, and labouring to show that Gaulard and Gibbs were not the inventors of the system, and that their transformers are not the best.

PROF. EWING'S discovery of hysteresis in iron has been shown, both by Kapp and Ferrari, to play a very significant figure in the efficiency of transformers.

GUGLIELMO, of Turin, has shown that no loss of electricity takes place through moist air surrounding an aërial wire unless the E.M.F. exceeds 600 volts, after which the leakage increases with the E.M.F. and the saturation of the air.

IN Boston an electric lamp has recently been used to search for a body drowned in the harbour. The U.S. steamship *Albatross* is furnished with a full complement of lamps for fishing. The glow-lamp is encased in a wire netting, which acts as a trap. The fish, being attracted by the light, swarm into the net, which is then closed and pulled in.

THE new number just issued (No. 201) of the Proceedings of the Royal Society contains the following electrical papers: "On the Photometry of the Glow-lamp," by Captain Abney and General Festing; "On the Development of Feeble Currents," by Dr. Alder Wright and Mr. C. Thompson; and "On the Heating-Effects of Electric Currents," by Mr. Preece.

MAKING GLASS SPECULA BY HAND.[1]

THE author of this paper gives a very interesting account of the construction of glass specula, discusses the actual difference in form between a spherical and a parabolic mirror, and gives an account of some experiments to determine the thickness of the silver film. In making the specula Mr. Madsen used glass for the grinding tool in place of metal, as he considered that the coefficient of expansion of iron and glass being different, greater truth would be obtained by the use of the same material for the tool, thus following the practice of Foucault and of the French opticians of the present day. When a true spherical surface was thus obtained the polish was given by rouge on pitch with a tool the same size as the mirror, and the correction of the spherical curve was obtained by a very ingenious plan of graduating the polisher in such a way that the greatest action would be on the required part of the mirror, the arrangement of the squares of pitch being such as to prevent the occurrence of rings of unequal polish. In this Mr. Madsen seems to have been most successful.

In working, the mirror was uppermost, and this is a very important point in many respects. There is no doubt that in working this way the mirror is in the condition of least strain, and if it were possible this plan should always be followed, but it is absolutely impossible to do this with a mirror much larger than the size he worked, which might almost be taken as the limiting size of mirror possible with this method of working. Short, Mudge, Herschel, and all the early workers used this plan in making their comparatively small mirrors; but since, with larger sizes, the mirrors have been worked face upwards as the only possible way, and it is to be regretted that this plan was not followed.

In discussing the actual amount of the glass to be abraded to obtain the correction, the author finds that for telescopes where the focal length exceeds twentytimes their diameter this amount is

[1] "Notes on the Process of polishing and figuring 18-inch Glass Specula by Hand, and Experiments with Flat Surfaces," by H. F. Madsen. (Journal and Proceedings of the Royal Society of New South Wales, vol. xx., 1886, pp. 79-91).

so small that it can be neglected, and that a spherical form is as good if not better than any other; and there is no doubt, for telescopes of about this ratio, Sir John Herschel is quite right when he makes the statement in "The Telescope," p. 81, "that is a good form that gives a good image; and that the geometrical distinctions between the parabola, sphere, and hyperbola, become mere theoretical abstractions in the figuring and polishing of specula." But in the case where the aperture of the mirror is about one-sixth of the focal length the distinction between the sphere and the parabola does exist and becomes a large quantity, which only the Foucault method of working allows to be dealt with properly. In enumerating the different plans used by opticians in getting the parabolic curve, the author is in error in stating that Lassell adopted the method of local polishing, as he always used a large tool, and got the figure by alterations of the stroke. Foucault was the inventor of the system of local polishing, and this was afterwards used by Draper, who finally rested on that as the best method of working.

The author considers that when the focal length exceeds 40 feet even with a theoretically perfect mirror the slightest touch or variation in temperature will be sufficient to destroy good definition with high powers, irrespective of the disturbing effects of the atmosphere, and he comes to the remarkable conclusion that "by decreasing the focal length the rays cross at a less acute angle, and small variations in the reflecting surface have not so detrimental an effect"—a statement that is entirely unsupported.

No actual tests of the work that the 18-inch mirrors will do are given. The experiments on the thickness of silver-on-glass films are interesting, as are also those on the effect of pressure or heat in altering the colours or colour-bands seen between two plane surfaces almost in contact. Dr. Draper, by actually weighing the amount of silver deposited on a large surface, came to the conclusion that it was about 1/200,000 of an inch thick; and the author, by comparing its thickness with the length of a wave of light, comes to about the same conclusion, and considers that by ordinary care in polishing no optical change will be produced in the reflecting surface by the film of silver deposited upon it.

The roads to success in making the mirrors of a reflecting telescope are many and various. Almost every maker in this fascinating pursuit had his own that gave to him best results. This was more particularly the case before Foucault published his most admirable memoir on the construction of silver-on-glass telescopes. In this memoir Foucault describes his method of local polishing, and the tests that can be applied to the concave surface, and a method of obtaining the true parabolic surface with absolute certainty, bringing the art of specula-making at once to a system of working by measurements in place of the old empirical process that had up to that time been in use; and everyone now uses Foucault's method of *testing* concave surfaces, and nearly everyone his plan of figuring by local polishing. Mr. Madsen gives a very interesting account of the road he took, an account that would have been much more valuable if the details of the processes used in making both the concave and the flat mirrors had been fully given, as it is now more in the improvements in these details that gain is to be looked for than in any of the main lines already known.

A. AINSLIE COMMON.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 2.—"On the Spectrum of the Oxyhydrogen Flame." By Profs. G. D. Liveing and J. Dewar.

In a former communication the authors described simultaneously with Dr. Huggins the strongest portion of the spectrum of water; subsequently they described a second less strong but more refrangible section of the same spectrum. M. Deslandres has noticed a third still more refrangible section. The authors now find that the spectrum extends, with diminishing intensity, into the visible region on the one hand, and far into the ultra-violet on the other. These faint parts of the spectrum they have photographed, using the dispersion of a single calcite prism and a lengthened exposure; and in the present communication they give a map of the whole extent observed, and a list of wave-lengths of upwards of 780 lines.

The spectrum exhibits the appearance of a series of rhythmical groups more or less overlapping one another, and the arrangement of the lines in these groups is shown to follow, in many cases, the law that the distances between the lines, as measured in wave-lengths, are in an arithmetic progression. M. Deslandres had previously announced that the succession of lines in several spectra, as well as in the telluric groups A, B and α of the solar spectrum, follow this law when their distances are measured in reciprocals of wave-lengths, and he has stated that the groups A, B and α have counterparts in the spectrum of water. The authors find a striking resemblance between those groups and certain parts of the water spectrum, but no exact correspondence.

Dr. Grünwald, of Prague, predicted on theoretical grounds that certain lines would appear in the spectrum of water, and the authors have found a considerable number of lines which tally closely with Dr. Grünwald's predictions, some of them, in the extremities of the spectrum, being the strongest lines observed in those regions.

February 9.—"True Teeth in the Young *Ornithorhynchus paradoxus*." By Edward B. Poulton, M.A., F.L.S., of Keble and Jesus Colleges, Oxford. Communicated by W. K. Parker, F.R.S.

This paper was a preliminary account of typical mammalian teeth developing beneath the site of the horny plates, which subserve mastication in the adult animal. In the upper jaw there are three teeth on each side: in the lower jaw two teeth, corresponding to the two posterior teeth of the upper jaw, were proved to exist, but the anterior one may be also present, for the jaws examined were not complete. The animal in which the teeth were found was about 8·3 decimetres long in the curled up attitude in which it had been received, and the larger hairs had alone appeared above the skin.

The anterior tooth of the upper jaw was long, narrow, and simple, as compared with the others; it was very fully developed, containing completely formed dentine and enamel, and its apex was nearly in contact with the lower surface of the oral epithelium. All the other teeth were broad and large, those of the upper jaw possessing two chief cusps on the inner side of the crown, and three or four small cusps on the outer side, while this arrangement was reversed in the lower jaw. Dentine was only formed upon the large cusps, and was not present upon all of these. The histological details and the manner of development appear to be precisely as in the higher Mammalia, a fact which strongly supports the identification of teeth with the placoid scales of Elasmobranchs. If teeth are so extremely ancient, then we should expect them to be unmodified in the ancestral Mammalia, although the other more recently specialized characters in the higher mammals are found in a more primitive condition in the former.

The teeth were found in some sections of the skull prepared for Dr. Parker by his son, Prof. W. Newton Parker. These sections, which had not been examined by Dr. Parker, were lent to the author, and Dr. Parker most generously encouraged the publication of the discovery, and assisted the investigation with other material.

Mathematical Society, February 9.—Sir J. Cockle, F.R.S., President, in the chair.—Messrs. A. E. H. Love and G. G. Morrice were admitted into the Society.—The following communications were made:—Further remarks on the theory of distributions, by Capt. Macmahon, R.A.—The free and forced vibrations of an elastic spherical shell containing a given mass of liquid, by A. E. H. Love.—On the volume generated by a congruency of lines, by R. A. Roberts.—Isoscelians, by R. Tucker.

EDINBURGH.

Royal Society, January 16.—Prof. Chrystal, Vice-President, in the chair.—Obituary notices of some former Vice-Presidents of the Society were read.—Prof. Tait communicated a paper by Prof. A. Macfarlane, on a problem in relationship.—Mr. W. Peddie read a paper on transition-resistance and polarization at platinum surfaces. He showed that transition-resistance increases greatly while polarization is proceeding. The ratio of the final to the initial resistance is in some cases as 2 to 1, when the electromotive force of polarization is equal to that of a Daniell cell. From his results regarding the time-rate of increase of polarization he deduced (10^{-9}) cm. as the value of the distance between the platinum and the layer of gas

condensed upon it.—Mr. Peddie also read a note showing that the phenomenon of "electric-absorption" must be exhibited if a dielectric has a film of gas condensed on its surface.—Prof. Tait communicated a paper by Mr. Albert Campbell on the change in the thermo-electric properties of tin at its melting-point. While the tin is solid its line on the thermo-electric diagram is inclined upwards. Liquefaction occurs before the line reaches that of iron. At this point the direction of the line changes and becomes nearly identical with that of iron. Thus the "specific heat of electricity" in tin changes sign at the melting-point. This shows that the loosening of molecular attraction, which occurs at the melting-point, produces the same effect in tin as is produced in iron, while still solid, at the higher of the two temperatures at which its magnetic and other properties suddenly alter.—Prof. Tait read a paper on the thermo-electric properties of Signor Battelli's iron; and showed from Mr. Omond's Ben Nevis observations that ice-crystals may, in the greater number of cases, have at least a share in the production of the observed phenomena.

PARIS.

Academy of Sciences, February 6.—M. Janssen in the chair.—Second note on the law of probabilities as applied to target-firing, by M. J. Bertrand. The paper deals specially with the objections urged by General Putz in the *Revue d'Artillerie* against the principle admitted by Poisson, and against the law of probability now generally adopted in schools of gunnery. Reference was also made by General Menabrea to the important researches of M. Siacci in this field of inquiry.—Remarks in reply to an objection raised by M. Khandrikoff to the theory of solar spots and protuberances, by M. H. Faye. During his observation of the recent lunar eclipse Prof. Khandrikoff noted some protuberances, the presence of which in the absence of spots for some days before the eclipse seemed to militate against M. Faye's well-known theory. To this objection M. Faye replied at some length, pointing out that it is partly based on a misunderstanding of the true character and bearing of his views.—On perfect numbers, by Prof. Sylvester. Recently M. Servais stated that a perfect number (if such exist) containing only three distinct prime factors is necessarily divisible by 3 and 5. It is here shown that no such number exists, the line of argument employed at the same time demonstrating the theorem that there exists no perfect number containing less than six distinct prime factors.—Observations made at the Observatory of Algiers during the total lunar eclipse of January 28, by M. Ch. Trépied. These observations comprise, among other matters, a study of the colours assumed by the lunar disk; a spectroscopic examination of the eclipsed portion of the disk; and the occultations of the stars contained in the list prepared by the Observatory of Pulkowa for the purpose of obtaining an exact determination of the apparent diameter of the moon. Communications were also received from the Observatories of Bordeaux and Nice on various phases of the same occurrence.—Ephemeris of the planet 252 for the opposition of the year 1888, by M. Charlois. The true positions, right ascension and declination, are given for the period from March 5 to March 19. At opposition (March 12) the magnitude will be 13·4.—Note on permanent deformations and thermodynamics, by M. Marcel Brillouin. Two propositions are established: (1) that for most elastics solids there exists no finite relation between the temperature t, the mechanic variable X, and the geometric variable x; (2) that for most solid bodies there exists a linear equation with total differentials between t, X, and x; or, more correctly, there exist as many equations of this class as there are independent geometric variables. In a future communication the theoretic results of this study will be announced.—Influence of diet in determining the fixation and elimination of carbon in man, by MM. Hanriot and Ch. Richet. The results are tabulated of mixed nitrogenous, fat, and feculent diets, including beef, bread, potatoes, butter, cheese, sugar, wine, and coffee, continued for a period of fifteen days.—On the presence of striated muscles in mollusks, by M. Raphael Blanchard. M. Hermann Fol's recent statement that true transversal striation of the muscular fibre is found in no mollusk, is shown to be erroneous and based on defective observation of these organisms, in some of which true transversal striation certainly occurs.—On the endomorphic modifications of the granulitic systems in Morbihan, Brittany, by M. Charles Barrois. This paper is devoted to a careful study of the remarkable endomorphic modifications and mechanical transformations of the Guéméné, Saint-Jean Brevelay, and Grandchamp granulitic formations, which traverse the Department of Morbihan in its entire length, and the typical constituents of which are: (1) zircon, apatite, black mica, oligoclase, orthose, and quartz; (2) orthose, microcline, quartz, tourmaline, and white mica.—On the Senonian and Danian systems of South-East Spain, by M. René Nicklès. Without attempting accurately to determine the respective limits of these formations, the author indicates the presence of extensive marine deposits in the Devonian containing fossiliferous limestones with several species of Hemipneustes associated with large banks of Hippurites and Pironea.—General Menabrea presented to the Academy the prospectus of a new edition of the works of Galileo, in about twenty-five volumes, which is about to be issued at the expense of the Italian Government, and copies presented to all the more important public libraries.—The Administrative Commission of the Academy announces that it has decided to supply Corresponding Members with the *Comptes rendus* free of charge from January 1, 1888. Correspondents are requested to acknowledge receipt of the first number, and notify their change of address to Messrs. Gauthier-Villars et Fils, publishers, Paris.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Treatise on Photography, 5th edition: Capt. Abney (Longmans).—The Story of Creation: E. Clodd (Longmans).—British Dogs, parts 15 and 16: H. Dalziel (U. Gill).—Beobachtungen der Russischen Polarstation an der Lenamündung, ii. Thiel, Meteorologische Beobachtungen: A. Eigner; II. Liefg. Beobachtungen vom Jahre 1883-84: R. Lenz.—Meteorological Observations at Stations of the Second Order for the Year 1883 (Eyre and Spottiswoode).—The Geographical Distribution of the Family Charadriidæ, H. Seebohm (Sotheran).—Annuario publicado pelo Imperial Observatorio do Rio de Janeiro, 1885-86-87 (Rio de Janeiro).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, 4th Series, vol. i. No. 2.—(Manchester).—Proceedings of the Manchester Literary and Philosophical Society, vol. xxvi. (Manchester).—Zeitschrift der Gesellschaft für Erdkunde zu Berlin, Nos. 133 und 134 (Reimer, Berlin).

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THURSDAY, FEBRUARY 23, 1888.

PHYSICAL SCIENCE AND THE WOOLWICH EXAMINATIONS.

SINCE the appearance of our article of January 26 considerable interest has been manifested in this question, and during the past week important communications on the subject have come from the Secretary of State for War, and from the head master of Clifton College. We learn from Mr. Wilson's letter to the *Times* that the new regulations are not only calculated to do harm by the discouragement of science, but that they are also retrograde in another very important particular. By increasing the range of the obligatory examination in mathematics, though they will not very greatly affect the selection of candidates, yet, in the case of very many of them, by compelling wider and less thorough study, they will damage the training in that subject.

In answer to a question put by Mr. Howorth, in the House of Commons on February 15, the Secretary of State for War is reported to have said that the new regulations are intended to encourage those subjects which it is believed can be least easily crammed; to give a preponderance to those subjects which are to the majority of officers of greatest practical importance; and that the new regulations are to be of permanent application.

Are the regulations calculated to achieve these purposes? We think it can be shown very clearly that they are not. It is therefore with renewed hope that discussion will lead to their amendment that we enter upon the following examination of them.

(1) The new regulations are intended to encourage subjects which it is believed can be least easily crammed. Mr. Wilson, as we have already pointed out, has indicated that in the case of mathematics they will distinctly tend to encourage quantity at the expense of quality. With regard to science subjects, the examination statistics which we published in 1884 clearly proved that experimental science was not then chosen by candidates on account of susceptibility to cram, for it was at that time less frequently selected than any other subject by successful candidates. During the three or four years that preceded 1884, a branch of experimental science was offered by only 22 per cent. of the successful candidates; since that date the numbers have risen, notwithstanding the increased thoroughness of some parts of the examination; and in 1887 about 38 per cent. of the successful candidates offered a branch of experimental science. This development is noteworthy, and may be considered to indicate an increased appreciation of the value of such work by teachers and students, since it has taken place in spite of the subject being rather a bad than a good one from the mark-winning point of view, and also during a period notable for improvement in some parts of the examination. Geography and geology, which may be, as some hold, more susceptible of cramming than chemistry and physics, show no corresponding tendency. In 1887 this subject was taken up by a distinctly smaller proportion of successful candidates than in the years that preceded 1884.

There does not appear, then, to be any justification for treating science as a subject more easy to cram than others that are more favourably treated. Had it been true that it is so, it would have been selected by a greater proportion of candidates formerly, and it would certainly have been discouraged by the nature of the examination during the last few years.

(2) Are the subjects selected those most calculated to be of practical importance to a majority of officers in the Engineers and Artillery? A flood of light is thrown upon this point by the course of instruction given to the cadets in the Royal Military Academy.

During the first year of training at Woolwich, cadets will study in the compulsory courses the following subjects:—

Mathematics,	for which	3000	marks are given.
Field Fortification,	,,	2000	,, ,,
Military Topography,	,,	2000	,, ,,
French or German,	,,	1000	,, ,,
Chemistry and Physics,	,,	1000	,, ,,
Model Drawing,	,,	300	,, ,,

During the second year of training, the cadets are divided into two classes. Those who are selected for the Engineers will then study, in addition to purely professional subjects—

Mathematics,	for which	2000	marks are given.
Chemistry and Physics,	,,	1000	,, ,,
Freehand Drawing,	,,	1000	,, ,,

In the case of the Artillery cadets during their second year, chemistry and physics alone of the ten or eleven subjects examined in the entrance competition are considered to be of sufficient practical importance to be retained.

Thus it stands admitted by the military authorities, according to their own regulations for the education of cadets, that, of the subjects examined in the competition for Woolwich, experimental science stands next to mathematics as a subject of practical importance in the training of officers for the scientific branches of the army.

That very great weight should be given to mathematics and modern languages in the examinations for Woolwich cadetships is obviously proper; but since it is admitted, by the courses of instruction in the Royal Military Academy, that capacity for and extensive training in experimental science form part of the necessary equipment of an officer of Engineers or Artillery, a system of selection which includes no means whatever of securing youths capable of such studies in the preliminary examination, and which places youths of scientific power at such considerable disadvantages in the competitive part of the examination, plainly needs to be amended, in the interest both of the service and of the candidates. We do not doubt that those who get into the Academy will be excellently taught there, but under these regulations many will be rejected who are eminently fitted to do well, in favour of others who are less gifted with the qualities that are admittedly most valuable.

The Committee who framed these regulations has, we fear, forgotten that the Professors at Woolwich will not create a capacity for science work by the mere teaching of science to the successful cadets, and that the utmost they can do in the case of those whose talents are linguistic rather than scientific,[1] will be to compel them to acquire by hard, uncongenial labour the necessary minimum of knowledge that is required in the subsequent examinations. It is abundantly

[1] The enormous value to be given to modern languages is likely to result in many such winning admission to Woolwich in future.

evident that the War Office Committee has made a serious mistake. The new regulations, so far as experimental science is concerned, are needless as precautions against cramming; they will not give due weight to the subjects which are, by their own showing, of most practical importance to officers; and they will influence most unfairly the selection of candidates by giving no chance for scientific power to tell in the results of the examinations.

There is another side of this question which is of very great public importance, viz. the influence of these and other examinations on school work in general. Regulations such as those now in force at Sandhurst, and those about to come into operation at Woolwich, make it increasingly difficult for science subjects to maintain their proper place in schools where they are already adopted, and hinder their adoption elsewhere. Many of the ablest youths in our public schools enter as competitors in these and other examinations, and as they must offer the subjects that pay best, such regulations as those under discussion lower the general standard of school work by constantly withdrawing from the science classes a large proportion of the best students. At present good work in science pays less well very often than mediocrity in other subjects. This, as was pointed out by Sir Lyon Playfair in his Presidential Address to the British Association in 1885, helps to arrest progress in science teaching. We do not, of course, claim that the interests of science in schools should be allowed to outweigh the manifest needs of the public services. But the fact that public examinations exercise a potent influence, not only on the education of the candidates, but also on the general tendency of school work, throws great responsibility on those who control them, and makes it our duty to urge that this influence shall not be lost sight of, especially when, as in the case of Woolwich cadets, an aptitude for experimental science is admittedly a quality that will be of great practical value in the professional work of the successful competitors.

THE MOTHS OF INDIA.

A Catalogue of the Moths of India. Compiled by E. C. Cotes, First Assistant to the Superintendent, Indian Museum, and Colonel C. Swinhoe, F.L.S., F.Z.S., &c. Part I. Sphinges. Part II. Bombyces. (Calcutta: Printed by the Superintendent of Government Printing, 1887.)

IT is not too much to say that the task of writing a catalogue of the moths of India is one which might appal an entomologist of far longer experience than either of the authors of this work. For when we consider that no general catalogue or revision of the Heterocera exists more recent than that of Guénée, that almost the whole of the types of the described species are in England, whilst both the authors of this book are in India, and that the number of Indian moths is so great that in the two first families alone upwards of 1600 species or supposed species are catalogued, it is evident that the difficulty of such a work is enormous; and as the authors are not known as lepidopterists of long standing, and are resident on opposite sides of India, no one would expect too much from the first attempt at what has long been very much wanted—namely, some work which would enable the rapidly-growing circle of working naturalists in India to know what has already been described and where the descriptions have appeared.

I think, therefore, that the cordial thanks of all will be given to Colonel Swinhoe and Mr. Cotes for their bold attempt to fill this blank, and that no one will be too critical as to how their task has been done when the great difficulties under which they labour are remembered. There is not a word of introduction to say to what extent either of the authors is responsible for the work, but I believe that Mr. Cotes is really the compiler, and that Colonel Swinhoe, whose collection is much richer than that of the Indian Museum in the species which occur in Western India, has added such additional species and notes as he possesses.

The plan of the work is nothing more than a bare catalogue of names and references, with localities so far as known to the compilers or to the authors of these names; and, as we see that in some genera almost all the species are unknown to either of the authors except from the descriptions or plates, it is evident that a large proportion of the names are names and nothing more.

In the genus Syntomis, for instance, we find forty-two supposed species catalogued, of which fifteen are described by Moore, nine by Walker, and ten by Butler; of all these only eleven are in Colonel Swinhoe's collection, and thirteen in that of the Indian Museum, and we do not find that a single attempt has been made to discover how many of these forty-two names represent distinct species.

As long as authors continue to do as Messrs. Moore, Butler, and the late Mr. Walker have so freely done—namely, to describe anything they do not personally know, with little regard to what has already been described—it is evident that, when their views as to variation are also extremely narrow, a great many synonyms must result, and we think a little genuine work would tend to show that of the forty-two supposed species of Syntomis not more than perhaps twenty really exist in nature. It is, however, quite as probable that while not more than twenty distinct species are described from India, at least twenty more remain undiscovered, for it is hardly possible for anyone who does not know India personally to understand how infinitesimal our knowledge of the moths is, except in some half-dozen localities like Bombay, Calcutta, and Sikkim; and even in such places as these what we know is but little compared to what we do not know. Surely here is a field for study and amusement which must attract many who will, sooner or later, provide the materials and collect the knowledge necessary for a "Catalogue *raisonné*," but the sooner a good example is set, by the careful and scientific description of the genera and species which are known, with due regard to distribution and variation, the more and better will be the work done.

A book is projected by Mr. F. Moore, whose knowledge of Indian moths is certainly greater than that of all other entomologists combined; but it is sincerely to be hoped that he will not adopt such a plan or style of work as his recently published "Lepidoptera of Ceylon." The bulk and cost of such a work on the Lepidoptera of India would quite prevent its use by those most likely to use it to advantage, and even if it was completed in

his lifetime the earlier parts would be out of date before the last were published.

What is wanted is something like Stainton's "Manual of British Lepidoptera," together with a serial publication which would give such a medium to entomologists for publishing their discoveries as *Stray Feathers* gave to Indian ornithologists. When such a journal has been going on for twenty years or so, it will be time to think of a Catalogue of the moths of India really worthy of the name. At present such an ambitious scheme as that proposed by Mr. Moore seems to me only likely to stand in the way of something better hereafter.

It is a great pity that no references are given in this Catalogue to the descriptions of the very numerous genera, so many of which are the creation of Mr. Moore. How many of them will eventually stand, time alone can show, but certainly many of them will merge in genera known in other parts of the world besides India.

I think also that if the authorities for the localities given for the various species were stated, as has been done in the case of specimens in the Calcutta Museum, this would be a great addition to the Catalogue. It is quite as important to know who collected a particular species as to know in whose collection it exists; and many localities are given without any good authority.

Another improvement in the form of the work would be an abbreviation of the references, in the same way as is done in Standinger's "Catalogue of European Lepidoptera"; a short bibliography of works cited, and their abbreviated citations, will take away any possibility of doubt, and save innumerable repetitions of such references as

"Walker, Cat. Lep. Het. B. M.,"
"Moore, Proc. Zool. Soc. Lond.,"
"Felder, Reise Novara Lep.,"
"Butler, Ill. Typ. Lep. Het. B. M.,"

which might be reduced with advantage to

"Walk., B.M.,"
"Moore, P.Z.S.,"
"Feld., Nov.,"
"Butl., B.M."

But notwithstanding the imperfections of this Catalogue, its publication will be a real blessing to naturalists, if only by saving them an immense deal of the most tedious, troublesome, and unsatisfactory work—the hunting up of descriptions and references. How far these are correct, I have not as yet been able to prove; but the few omissions which I have discovered may easily be forgiven.

H. J. ELWES.

PROLEGOMENA TO THE STATISTICS OF THOUGHT.

Die Welt in ihren Spiegelungen unter dem Wandel des Völkergedankens. Prolegomena zu einer Gedankenstatistik. ("The Universe as reflected in the Movement of Thought among the Races of Mankind. Prolegomena to the Statistics of Thought.") By A. Bastian. One-vol. in 8vo, with an Atlas or Ethnographical Picture-book in oblong folio. (Berlin: E.S.Mittler, 1887.)

DR. BASTIAN'S idea is that the new science of ethnology supplies materials from which it is possible to construct a system of psychology on the inductive methods of natural science. The inductive study of the material universe has given us our modern science, and with it modern materialism. But materialism, says Dr. Bastian, is but a one-sided expression of the legitimate tendency of the age towards induction and natural science. It overlooks the fact that the world of ideas offers as legitimate a field for the application of scientific method as the world of material phenomena. Ethnology, which considers man not as an individual, but in his social aspects, teaches us that the universe of thought also obeys laws, and can be studied by the genetic method. And therefore Dr. Bastian desires to see the statistics of thought put together in a way that will exhibit the whole range of ideas about the universe and its contents which have been prevalent among the various races of men. These statistics will form the basis for a psychology constructed on inductive principles.

The description of a science which has still to be created must necessarily be vague and hazy, and in the present case the vagueness is increased by the fact that Dr. Bastian writes in a very involved and enigmatic style, so that his meaning cannot be read, but must be divined. But so far as we have been able to follow him we gather that in his "Prolegomena to the Statistics of Thought" he designs something different from what is given in ordinary Prolegomena, and that the volume should rather be called a provisional collection of materials for the comparative study of the ideas entertained by different races, or in different stages of culture, as to the universe and the leading matters of human interest that it contains. It would seem that Dr. Bastian, whose great range of knowlege in matters ethnological is well known, and who is also a voracious and somewhat undiscriminating reader of books on all possible subjects connected with the history of human thought, has accumulated huge commonplace-books to illustrate his favourite project. The small-type sections which make up a large part of the volume are simply chunks from these note-books, to all appearance entirely undigested. Commonplace-books have always a tendency to become chaotic, especially in the hands of a man who reads so widely and miscellaneously as Dr. Bastian has done; but we have never seen anything quite so formless as these pages. In themselves many of the extracts given are interesting or curious, but the disorder in which they stand is simply bewildering. Moreover, there are no exact references to chapter and verse of the authors quoted, and verbal excerpts stand side by side with brief jottings and condensed indications such as a man may make for his own use, but which are so many enigmas to the reader. There has evidently been no verification and no revision of the notes originally made by the author for himself, and many of them, therefore, are not only obscure, but not quite accurate; while others were not worth printing at all. The last remark is specially applicable to a vast number of quotations from ancient and modern metaphysicians, into whom Dr. Bastian has evidently dipped at hazard, without having any clear conception of the history of philosophy as a whole. On half a page we find Proclus, Anaximander, Philolaus, Aristotle, the Pythagoreans, F. A. Müller, Spencer, Schelling, Samuel ben Gebirol, and Anaxagoras. Who can hope to be instructed by such a

jumble? The large-type sections that connect these masses of confused notes are still more perplexing. Here, also, the commonplace-book predominates, but the extracts are worked up into some semblance of a continuous exposition. It is very seldom, however, that one can read a page on end without losing the thread. The reason soon becomes obvious. What is offered as a book is really nothing more than a transcript of rough jottings, in which Dr. Bastian had from time to time recorded his ideas in a form just sufficient to preserve a record for his own use. The sentences are often not even grammatical, and in brief the volume is only the roughest of rough note-books printed without revision. In spite, therefore, of the enormous labour and learning which it attests, the whole must be pronounced a failure, for the elementary reason that it is not a book. We trust that the publication may be useful to the author in helping him to get his superabundant material better under control, and so to produce hereafter something that is a book and can be read.

The ethnological picture-book is designed for young people, and its illustrations of cosmogonic and cosmographic ideas, of various conceptions of the future life and so forth, are well calculated to excite their curiosity and stimulate their interest in such things.

OUR BOOK SHELF.

Experimental Chemistry for Junior Students. By J. Emerson Reynolds, M.D., F.R.S. Part IV. Organic Chemistry. (London: Longmans, Green, and Co., 1887.)

THIS volume on organic completes the author's course of experimental chemistry. Whatever may be the opinions on the three previous volumes, there is no doubt this is the most rational attempt to treat organic chemistry practically—as a thing for students actually to do—that has as yet appeared. There is scarcely an experiment in the book that a student will be unable to do from the description given, and the order in which they are taken and general arrangement is the natural order of synthesis, proceeding from the less complex and easy to the most complex and least known.

The author begins with destructive distillation, and the production of alcohols, their salts, &c. The fourth chapter deals with metallic compounds or organo-metallic bodies. In the description of the manufacture of zinc ethide the method of making from zinc, C_2H_5I, and iodine might have been given, as the action is much quicker than with the Cu—Zn couple and the yield greater. The current of CO_2 can also be dispensed with advantageously. Two experiments here we must take exception to as being rather dangerous for beginners—sealing up sodium with zinc ethide, and in Experiment 691 making mercuric ethide as a sort of starting-point material. The author cautions against inhaling the vapour of this substance, as it is "supposed to be poisonous." We thought it was quite settled that it is about the most dangerous substance one has to deal with; and we certainly do not agree with the author that the method of employing mercuric ethide for making zinc ethide is the easiest of all methods for making the last-named substance.

In the remainder of the little book there is nothing either in arrangement or process to which objection can be taken, and undoubtedly it should be most useful to students attending a course of organic lectures. As a rule English students stop off with organic before they have really made its acquaintance; very few indeed continue its study long enough for it to be of any use to them. Most of the works in England where "organic chemistry" is the rule are obliged to obtain the services of German chemists; the English student's acquaintance with the subject generally stopping at the knowledge that there are such things as hydrocarbons, or "hydrocarbides," as the author of this book calls them.

Perhaps when such practical instruction is given in our schools as the course outlined by this book, we may begin to produce students who can go into "a works" and be trusted not only to follow a process but to originate new ones. W. R. H.

The Farmer's Friends and Foes. By Theodore Wood, F.E.S. (London: Swan Sonnenschein, Lowrey, and Co., 1888.)

THIS is a well-meant and well-put-together little volume, giving an account of the life-history of most of the animals which, for good or for evil, come across the path of the British agriculturist. Throughout, the attempt is made to prove that, when it is necessary for the saving of a crop to destroy any animal, it is far better to trust to Nature, as being more competent, than to man; but then this seems to beg the whole question, as it presumes that man has not already very much interfered with Nature's regulations.

The volume is, in part, the result of personal investigation, but the author quotes freely from all our best-known writers on the subjects of which he treats.

The figures are good. A table of contents would have added to the usefulness of the work, especially as the index is not very detailed. The volume may be safely placed in the hands of all interested in the subject.

The Story of Creation. By Edward Clodd. (London: Longmans, Green, and Co., 1888.)

THE author of this book does not pretend to make his readers acquainted with new facts and ideas. His object is to present a popular exposition of the theory of evolution, using the word evolution in its widest sense. The work is divided into two parts—one descriptive, the other explanatory. In the descriptive part he begins with a chapter on matter and power. He then considers the distribution of matter in space, and gives a general account of the sun and the planets, of the past life-history of the earth, and of present life-forms. In the explanatory part he discusses the questions relating to inorganic evolution and to the origin of life and life-forms, and sets forth in logical order the arguments which are held to establish the truth of Darwin's theory of the origin and development of species. A final chapter is devoted to social evolution, including the evolution of mind, society, language, art and science, morals, and theology. The book is vigorously written, and well illustrated; and readers who have had no special scientific training will find that it enables them to understand and appreciate some of the greatest and most fruitful generalizations of modern science.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Botanists and the Micromillimetre.

I NOTICE that in a review of a "Manual of British Discomycetes" which appeared in NATURE on February 9 (p. 340), and apparently also in that work itself, the word *micromillimetre* is used as equivalent to the thousandth of a millimetre.

I have made some inquiry, and am told that it is now commonly employed by biologists, and especially by botanists, with that meaning.

As it would be very unfortunate if the same word were habitually used in different senses by students of different branches of science, may I be allowed to point out that, according to the definitions of the C.G.S. system, a micromillimetre is the millionth of a millimetre.

In the well-known Report of the Committee of the British Association for the "Selection and Nomenclature of Dynamical and Electrical Units" it is laid down that the prefixes *mega* and *micro* are to be employed "for multiplication and division by a million."

This ruling has been generally accepted not only by scientific men, but also by those engaged in commerce. Megohm and microfarad are terms which are used in contracts, and are universally understood to mean a million ohms and a millionth of a farad respectively. It will be hopeless to try to introduce scientific systems of measurement into the affairs of daily life if scientific men themselves disregard the rules on which those systems are framed.

It would also be particularly confusing if the micromillimetre were wrongly used by microscopists. In its proper sense it is the most convenient unit in which to express molecular magnitudes. It has been employed for that purpose by Sir William Thomson and others in England, and also by physicists abroad. If the micromillimetre of the microscopist is 1000 times too large, all sorts of mistakes will be rife as to the relative dimensions of molecules and of the smallest objects visible with the microscope.

The proper name for the thousandth of a millimetre (μ) is the *micromētre*, and though the similarity of this word to *micromĕter* is no doubt a drawback, it is not likely that confusion could often arise between them.

If, therefore, I am rightly informed as to the custom of botanists in this matter, I beg respectfully to suggest that they should bring their nomenclature of units of length into conformity with the definitions of the C.G.S. system. Otherwise there will be a permanent confusion between the *micrometre* (μ) and the micromillimetre ($\mu\mu$). ARTHUR W. RÜCKER.

Science Schools, South Kensington, February 17.

"The Teaching of Elementary Chemistry."

ALLOW me to draw the attention of the chemical section of your readers to a few highly misleading passages in the two books reviewed under the above heading in NATURE of January 19 (p. 265).

On p. 65 of the "Elementary Chemistry" we read:—

"Hence when sodium and water interact, a portion of the hydrogen which was combined with oxygen is evolved as hydrogen gas, and another portion enters into combination with the sodium and the oxygen to produce caustic soda."

On pp. 116–17 is to be found the following astounding passage:—

"To prepare Cl_2O, mercuric oxide (HgO) is heated in a stream of dry chlorine. When mercuric oxide is heated it is decomposed into mercury and oxygen; therefore by passing chlorine over heated mercuric oxide we carry out a reaction in which oxygen is produced in presence of chlorine."

In the "Practical Chemistry," under Experiment 1, Chapter II. (p. 6), occurs the following warning to the student:—

"Do not remove the lid at any time for more than a second or so, else some of the magnesia will be volatilized and lost;" while on p. 285 of the "Elementary Chemistry" we read:—

"No compound of Mg has been gasified."

Even this contradiction is excelled by one on pp. 62 and 63 of the "Practical Chemistry," which is not so manifestly a slip. We read (p. 62):—

"The reactions between aqueous solutions of alkalies and the three elements, chlorine, bromine, and iodine, are similar; compounds of similar compositions and similar properties are produced under similar conditions."

Under Experiments 19 and 20, which follow, the student is told to treat cold solutions of potassium hydroxide with chlorine and bromine respectively. The well-known changes are described, and the bleaching properties of the solutions after addition of a little acid, are to be tried. Then follows (p. 63):—

"Exp. 21. Perform an experiment similar to 19 and 20, but use *iodine* in place of chlorine or bromine: the liquid which is produced does not bleach. No compound of iodine analogous to KClO and KBrO has been obtained."

Truly this is "seeing things as they are" with a vengeance.

Z.

Natural Science and the Woolwich Examinations.

MAY I be allowed, as one of the most experienced science masters in the public schools, to say a word in reply to Mr. Gurney's letter in NATURE of this week (February 16, p. 365)?

There seems to me a general fallacy running through that letter arising from "the absence (on the writer's part) of clear discrimination" between the nature and methods of mathematical science (which, as J. S. Mill taught us long ago, are mainly *deductive*) and experimental science (which proceeds by *inductive* methods). It is on this ground mainly, coupled with the extent to which it cultivates the faculty of *observation*, that we claim for it a special educational value.

After an educational experience at least as extensive as Mr. Gurney's, I join issue with him most distinctly on this point. I am afraid there lurks behind Mr. Gurney's depreciation of the educational value of science the disappointment which other mathematicians have experienced in finding that the man who takes to experimental science as a mere excursion-ground for the diversion of the mathematician is not infrequently brought to confusion by Nature. *Science is something more than measurement.* Mr. Gurney's notion that mathematics and a knowledge of French and German are a sufficient groundwork for true scientific knowledge is such a confession on his part of the small value he attaches to experimental demonstration and to laboratory training (or to field-work in the case of geology) as is sufficient to put him out of court as a witness on this question. Nor do I think that he is competent to speak with any authority on the work done in the public schools. If he fancies that the best boys of the public schools go to Powis Square to finish their education, he is labouring under a delusion.

The whole argument of the letter is retrogressive; nor is it strengthened by the writer's condemnation of a "smattering" of science, which is no discovery. But I maintain that a boy can, by the age of eighteen, get a sound groundwork in science laid, though not by the cramming system; and that to this the term does not apply at all. Again, he condemns premature specialization of a boy's studies in favour of science, while he inconsistently advocates a much narrower specialization in favour of mathematical studies.

How far Mr. Gurney is from being convinced by his own arguments is shown by the fact that in the concluding paragraph of his letter he practically surrenders the point on which the whole question turns.

In conclusion I would commend to his careful consideration the letter which appears in the *Times* to-day from the head master of Clifton College, whose competence to form a judgment on the educational aspect of this question I suppose no one doubts. A. IRVING.

Wellington College, Berks, February 17.

WITH your kind permission, and in consideration of the importance of the matter, I crave leave for space for a few remarks in addition to those contained in my reply to Mr. Gurney.

(1) Looking at the history of education in this country, we can account for, though we deplore the existence of, a prevalent notion, a sort of fashionable superstition, which regards scientific studies as outside the range of what is called "culture"; a superstition for which some of those who have spoken in the name of science are not altogether unanswerable, but which derives its chief strength from that profound ignorance of natural science—its nature, its methods, and its object—upon which so large a proportion of educated Englishmen seem rather to pride themselves than otherwise. There can be but little doubt that this has been turned to account as an influence in favour of the contemplated scheme.

(2) It is in no spirit of hostility to literary studies that one writes in these terms; on the contrary, it is as a lover of literature of the better sort that one would gladly see the literary spirit in this country, as in Germany, strengthened and braced by the strong atmosphere of scientific criticism, and a little more first-hand acquaintance with things as they are, which is the true

aim of science. It is impossible to estimate the good that might be done in this direction, if only the Universities had the wisdom and the courage to insist upon a knowledge of some one branch of science for all degrees, as was strenuously advocated years ago by Charles Kingsley.

(3) It seems a great pity that such a change as is contemplated should be adopted just now, since within the last three or four years some of us who are teachers of standing and experience have gladly recognized considerable improvement in the examinations in science as they are conducted by the Civil Service Commissioners. It would appear that the *cramming* of these subjects has been considerably handicapped, if one may judge from the considerable increase in the number of o's affixed to the names of candidates in the published lists *pari passu* with a considerable increase in the number of marks gained by one's own pupils, who have had the same teaching and laboratory training as those of previous years.

(4) It is surely fairly within the province of Parliament to consider the question whether it is expedient or conducive to the common weal, that science shall be placed at such a disadvantage that young men, who are candidates for the more scientific branches of the military service, shall be strongly tempted to eschew all preliminary training in science, as they certainly will be unless the regulations are somewhat modified.

Four years ago the action of a single Member of Parliament (Sir John Lubbock), backed up by the influence of the Councils of the Royal Society and the British Association, was effectual in securing a reconsideration of the provisional examination scheme for admission to Sandhurst; so that, although—as ultimately issued—the regulations contained an absurd anachronism in the proportion of marks assigned to scientific subjects, this was reduced to less startling proportions.

Can it be doubted, then, that if on the present occasion the three Members of Parliament who may be said to be the representatives *par excellence* of science in the Legislature (the President of the Royal Society, the President of the British Association, and the Member for the University of London), were to take united action in Parliament, the position of science (so far as the Army Entrance Examinations are concerned) might be changed from one of semi-strangulation to one of free and fair competition, which is all that its most ardent advocates can desire for it? This could be effected to the advantage of the studies of the cadet, by such a simple modification of the published regulations as would be involved in limiting the choice of optional subjects in Class I. to two, and allowing two subjects of Class II. to be taken up. A. IRVING.

Wellington College, Berks, February 20.

The Composition of Water.

PROF. THORPE, in his interesting article on the composition of water (p. 313), alludes to Dr. Scott's very valuable determinations of the ratio of the volumes of hydrogen and oxygen which combine to form water.

If we assume with Dr. Scott that the small amount of impurity present in his gases, and which he estimated in each case after the explosion, was evenly distributed between them, a curious relation may be observed between the amount of impurity present and the calculated ratio of the volumes.

This relation is apparent if we subtract the ratios calculated by Dr. Scott from some fixed number, say 2·000, and compare these differences with the relative amounts of impurity. It will be seen from the following table that the greater the amount of impurity present the greater is the difference of the ratio from the constant number, or, in other words, the lower is the ratio. The impurity is given in volumes per 100,000, and the differences are multiplied by 10,000.

Relative impurity.	Difference of ratio from constant.	Relative impurity.	Difference of ratio from constant.
35	83	102	125
36	55	105	133
41	66	106	140
47	44	146	154
52	32	162	106
56	90	167	224
66	72	188	118
72	46	254	187
75	80	495	540
78	120	498	506
98	73		

The relation is better seen, however, by mapping the results, taking the ratios as abscissæ, and the impurity in volumes per 100,000 as ordinates. The dotted straight line (Fig. 1) passes so well through the points that it leaves ten of them on the one side and eleven on the other.

It seems difficult to believe that this apparent relation can be merely a chance coincidence; the direction taken by the points is too definite. Nor can it well be due to any chemical action

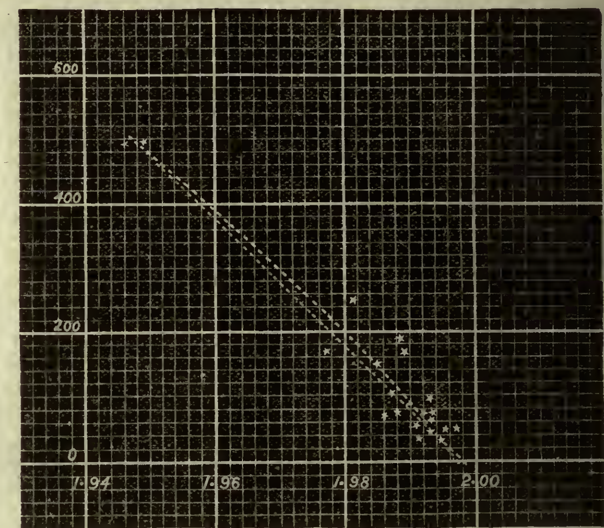


FIG. 1.

between oxygen and nitrogen, for Dr. Scott states that the water produced was free from any acid reaction, and that no trace of the oxides of nitrogen could be detected. The relation is even more marked if we assume that the whole of the impurity was in the hydrogen. This is shown in Fig. 2; the points obviously fall about a line which is nearly, if not quite, straight.

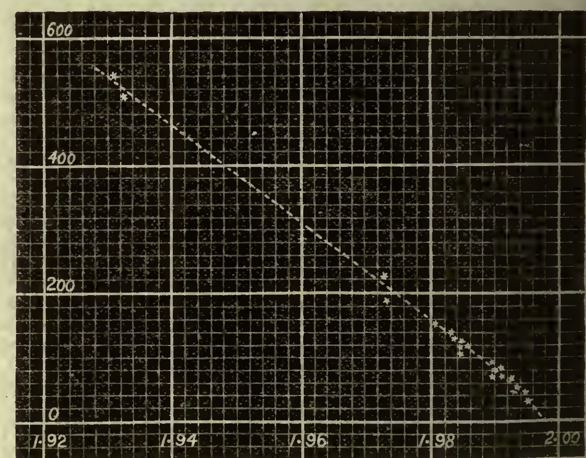


FIG. 2.

If, however, we assume that the whole of the impurity was in the oxygen, and if we neglect the two experiments with the excessive amount of impurity, no such relation is to be observed (see Fig. 3), but the ratios are distributed with fair regularity about a mean value of 1·9965 or 1·9970. The simplest explanation of the facts appears to be that the whole, or at least the greater part, of the impurity was really in the oxygen, and that the apparent relation of the amount of impurity to the ratio

is due to the error introduced into the calculations by referring the impurity to the hydrogen. But, whether this explanation be accepted or not, it is clear that the three lines drawn through the points representing the three series of ratios ought to meet at a point on the horizontal line of zero impurity, for the errors, whether due to chemical action or to calculation, would disappear with the cause that produced them. Hence, if no other source of error is present, the true ratio may be found by taking the most probable point of intersection of the three lines on the horizontal line of zero impurity. It is not easy to determine exactly the position of this point: it probably lies between the values 1·996

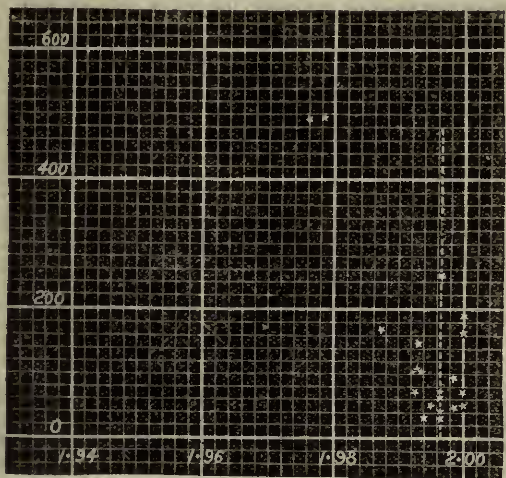


FIG. 3.

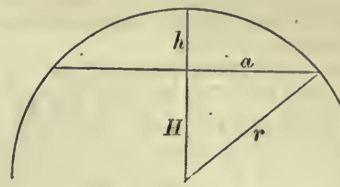
and 1·998, and the true ratio may perhaps be taken as 1·997 Dr. Scott adopts the ratio 1·994, but this appears to me to be certainly a little too low.

Prof. Thorpe shows that the atomic weight of oxygen, calculated from Regnault's densities of oxygen and hydrogen, corrected by Prof. Le Conte, and Dr. Scott's ratio (1·994) for the combining volumes, is 16·029. The ratio 1·997 would make the atomic weight O = 15 985. SYDNEY YOUNG.

University College, Bristol.

The Fog Bow and Ulloa's Ring.

IN the summer of 1875, I made a tour of inspection to our meteorological stations in the surveying-steamer *Hansteen*, Capt. M. Petersen, R.N. During the morning hours of August 7, I was on shore at Gandfjord, on the south side of the Varangerfjord, and measured the height of some terraces there. At 1h. 10m. p.m. we took serial temperatures in the Gandfjord with the deep-sea thermometer. The weather was calm, and a dense fog prevailed. The temperature of the air was 12° C. Leaving the Gandfjord we proceeded northwards. The dense fog continued. At once the fog began to be lighter and the sun to shine through, and a few minutes afterwards we were out of the fog, which was standing as a white wall in the south-west. In the moment the sun appeared, but before we were quite clear of the fog, I saw in the north-east a bow having the shape of a rainbow, but quite white, projected on the fog. With a sextant I measured its amplitude, or the chord along the horizon, and the height of the summit above the horizon—in both cases the middle between the outer and inner edge of the bow. The horizon not being distinctly visible, it is probable that the measures taken do not exactly refer to the true horizon, nor is it certain that the height of the summit was taken from the same horizontal plane in which the amplitude was measured. By the captain's reckoning, the apparent ship's time, at the moment of observation, was 2h. 40m., and the latitude 70° 1′. From these data, and the declination of the sun, I computed the azimuth of the sun at south 46° 5′ west, and its apparent altitude at 31° 12′. Supposing, as the results of the several computations tend to indicate, that the white bow is circular, and has its centre in the anthelic point, we may calculate the angular radius of the bow by three different methods.



Let h represent the height of the summit of the bow above the horizon, a half the amplitude or chord along the horizon, H the dip of the centre of the bow below the horizon, supposed to equal the altitude of the sun, and r the angular radius of the bow. Then we have—

$$r = H + h \quad \ldots\ldots\ldots \quad (1)$$

$$dr = dH + dh \quad \ldots\ldots\ldots \quad (1')$$

$$\cos r = \cos a \cos H \quad \ldots\ldots\ldots \quad (2)$$

$$dr = \frac{\tan a}{\tan r}\, da + \frac{\tan H}{\tan r}\, dH \quad \ldots\ldots \quad (2')$$

$$\cos r = \cos a \cos (r - h)$$

$$\tan r = \frac{1 - \cos a \cos h}{\cos a \sin h}, \text{ or putting}$$

$$\cos a \cos h = \sin^2 M$$

$$\tan r = \cot^2 M \cot h \quad \ldots\ldots\ldots \quad (3)$$

$$dr = \cos (r - h) \frac{\cos r}{\sin h} \tan a\, da - \sin (r - h) \frac{\cos r}{\sin h}\, dh \quad (3')$$

The observations gave $2a = 49°$, $a = 24° 30'$, and $h = 7°$ (or a little more).

From (1) we have $r = 31° 12' + 7° = 38° 12'$.

Putting $dH = \pm 2'$, $dh = \pm 15'$, we have by (1′)

$$dr = \pm \sqrt{2^2 + 15^2} = \pm 15'{\cdot}1 = \pm 0°{\cdot}25.$$

From (2) we have $r = 38° 53'{\cdot}5$,

and by (2′) $dr = 0{\cdot}577\, da + 0{\cdot}770\, dH$, dH being the error in the altitude of the measured chord, or the chord's altitude or depression, reckoned from the horizon.

Putting $da = dH = \pm 15' = \pm 0°{\cdot}25$, we get—

$$dr = \pm 0°{\cdot}24.$$

From (3) we have $r = 41° 8'$,

and by (3′) $dr = 2{\cdot}315\, da - 3{\cdot}074\, dh$.

Putting $da = dh = \pm 0°{\cdot}25$, we get—

$$dr = \pm 0°{\cdot}97.$$

Taking the weights inversely as the squares of the probable errors, we find that the results from (1) and (2) have a weight of 15 times that found by (3), and the mean will be—

$$r = 38° 38' \pm 6'{\cdot}4.$$

From this mean we find that h should have been 38° 38′ − 31° 12′, or 7° 26′ instead of 7°, or somewhat greater than measured, as supposed in my note-book. Computing from (2) we find that we should have calculated with H = 30° 51′ instead of 31° 12′, or the chord has been measured in a level 21′ lower than the horizon, which is highly probable with the fog spreading over the surface of the sea. The measured chord being too great, and the measured height too small, it follows from (3′) that (3) must give the radius by far too large.

The next occasion I had to observe the fog bow was in 1878, on the North Atlantic Expedition, when returning from Spitzbergen. During August 30, our ship, the *Vöringen*, had a rather tedious work in advancing southwards, on account of the foggy weather prevailing the whole day. In the afternoon we had advanced so far south of Bodö as to approach the Sandhorn, a mountain about 3000 feet high, lying to the east of the route.

At 5h. 20m. p.m. I saw an anthelic fog bow, white, with the outer edge reddish, the inner edge bluish. I measured, with the sextant, the amplitude along the horizon at 76°, the sun's altitude at 12°, and the breadth of the bow at 2°. The temperature of the air was about 14° C. The latitude was about 67° 10′.

Assuming the measured chord to lie in the true horizon, we get by (2) from $a = 38°$, H = 12°, $r = 39° 35'{\cdot}5$. But it is highly probable, that the measured chord lay deeper than the

fog-veiled horizon, perhaps some degrees deeper, and it may be quite as possible that I have measured the diameter as a chord. This supposition gives $r = \frac{1}{2}\, 76°$ or $38°$. The mean of these two determinations is $38° \, 48'$, with a probable error surely not less than $\pm 48'$, or half the difference.

At 6h. 1m. p.m., the circumstances were more favourable. I measured $2a = 76° \, 11'$, $H = 8° \, 11'$, the breadth of the bow 2°. The latitude was 67° 7′. From these data we have, by (2) $r = 38° \, 50'$. Assuming the observed amplitude to have been the diameter, which is very probable, we have $r = 38° \, 5''.5$. The mean value is $38° \, 28' \pm 22'$.

If the fog bow, like the rainbow, has always the same diameter, we can join the three values thus found for the radius into a mean result. We have thus, giving the single determinations a weight inversely as the squares of their probable errors—

1875 August 7,	2h. 46m.	$r = 38° \, 38' \pm 6'$
1878 August 30,	5h. 20m.	$r = 38° \, 48' \pm 48'$
1878 August 30,	6h. 1m.	$r = 38° \, 28' \pm 22'$
Mean		$r = 38° \, 38' \pm 1'.4$

The breadth of the bow being 2°, with a probable error of $\frac{1}{10}$ or $\pm 6'$, we get—

for the outermost red ring $r = 39° \, 38' \pm 6'.2$
for the innermost blue ring $r = 37° \, 38' \pm 6'.2$.

At 7h. p.m. the bow stood white against the blue sky, the Sandhorn below it. At 6h. 40m. p.m., and sometimes before, I remarked that my own shadow was visible on the fog wall. In order to get a wider view of the phenomenon, I went up upon the roof of the chart-house, where my eye was 27·5 feet (8·4m.) above the surface of the sea. From here I saw how my shadow distinctly imitated all my movements. The shadow of my head appeared dark on a lighter white ground, and from a certain distance surrounded by a concentric coloured glory, in which the colours were arranged in the order of the spectrum, so that the outermost circumference was red, the middle yellow, and the innermost blue. There was no white band in the glory. With the sextant I measured the radius of the yellow ring, which was the most intense, at 1° 31′. The intensity of the other coloured rings was too feeble to allow their radius to be measured with the sextant. From a comparison with the radius of the yellow ring I judged that of the blue at 1° 15′, and that of the red at 1° 45′, with a possible error on both sides of ± 5′. This phenomenon is Ulloa's Ring.

Taking all my results together, we have the following synoptical table :—

Ulloa's Ring.	Radius of inner	blue	1° 15′ ± 5.
,, ,,	,, ,,	yellow	1° 31′ ± 2′.
,, ,,	,, ,,	outer red	1° 45′ ± 5′.
Fog Bow	,, ,,	inner blue	37° 38′ ± 6′.
,, ,,	,, ,,	middle	38° 38′ ± 1′.
,, ,,	,, ,,	outer red	39° 38′ ± 6′.

The fog bow cannot be the rainbow with three or four inner reflections, as these rainbows, if visible, would not be anthelic, but have, for the red rays, distances from the sun of 42° and 43°. Moreover, the intensity of the fog bow is too considerable to be the result of so many reflections in drops of rain. The ordinary or first rainbow, with one inner reflection, has a radius or distance from the anthelic point of 42° 30′ for the red, and 40° 30′ for the violet rays, which gives, the sun's radius being 16′, its innermost radius like 40° 14′. The outermost red ring of the fog bow has a radius of 39° 38′. Its distance from the ordinary rainbow is consequently only 36′. This space we see sometimes covered by the supernumerary rainbows, caused, as Sir G. B. Airy's investigations have shown, by the interference of the rays leaving the raindrop.

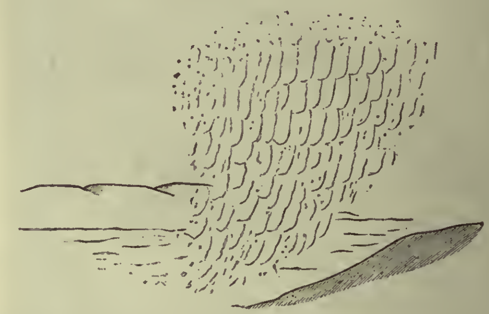
It seems probable that the smallness of the fog globules, as contrasted by the larger size of the raindrops, must enter as an essential part in the explication of the fog bow. In Günther's "Lehrbuch der Geophysik und physikalischen Geographie," ii. p. 151, he speaks of white rainbows, the description of which agrees with the aspect and position of my fog bows, and for which Bravais has given an explanation ("Sur le Phénomène de l'Arc-en-ciel blanc," *Ann. de Chim et Phys.* [3], vol. xxi. p. 348). Not having Bravais' memoir at hand, I may only remark that, as far as I can see from Günther, he assumes the fog drops to be hollow, a supposition which is hardly in accordance with modern investigations.

When I saw the fog bow, I had, I am sorry to say, no polariscope, so that I was unable to investigate the polarization of its light, so characteristic for the rainbow. H. MOHN.

Christiania, January 31.

The Shadow of a Mist.

THE reticulated rippling shadow of the mist described in Mr. Fawcett's letter (NATURE, January 5, p. 224) reminds me of a somewhat parallel phenomenon seen by me a few years ago. I saw a snow-storm some miles away crossing the valley between the Mendips and the Quantocks. It hung like a long, heavy curtain partially obscuring the *bright* western sky. The



light shining through the shower showed a fairly regular pattern. On a reddish-brown ground the darker, because denser, parts of the shower took the form indicated roughly by the accompanying diagram. Was the snow falling in spiral streams, and would a similar explanation apply to the shadow of the mist seen by Mr. Fawcett? HENRY BERNARD.

The English Church, Moscow, January 31.

Instability of Freshly Magnetized Needles.

I MADE no attempt to investigate the fluctuations of the dipping needle. They seemed to me to pass away after a few minutes, and I therefore took that method to get rid of them, supposing that the phenomenon was well known to other observers. The variations that I observed amounted to three or four minutes, I should think. It is true that the dip circle which I used was of an ancient pattern ; as Prof. Rücker says, hardly up to modern requirements. I did not send it back to the maker for adjustment, as Mr. Whipple says he would have done, because it was lent to me, and it was the best I was able to get.

Recognizing the fact that we could not expect to get the best results from our outfit, it was deemed best to make only one set of observations at each station, and multiply the number of stations as greatly as possible. This made it necessary to do the work quickly at some stations in order to adapt our time to that of trains, or in order to get the drudgery involved in camp-life done within the twenty-four hours. It is probable that at some stations we overdid the matter, and that the observations would have been better if more time had been taken. The dip observations I always regarded as least satisfactory. But all of the work has been published in such a way that its value can be estimated by anyone interested, and everyone is welcome to place whatever value he pleases upon it. We did the best we could under the circumstances, and the expense was met by private means.

The dip circle was returned to Washington when we were done with it, so that I am not now in a position to throw any light upon the subject under discussion. For most of the stations at which observations were made, I think the magnetic elements were determined with as great precision as a single observation would give them, and they seem to me to be as valuable as they profess to be, and not very much more. The fact that so little magnetic work had been done in the central part of the United States seemed to me to justify the plan of making the number of stations large, rather than of trying to attain the precision of observatory methods in field work at a few stations.

FRANCIS E. NIPHER.

Microsauria and Dendrerpeton.

In the notice in NATURE of January 12 (p. 244) of Fritsch's new number of his excellent work on the Permian fauna of Bohemia, which has not yet reached me, I observe a reference to *Microsauria*, which would seem to imply that I had included *Dendrerpeton* in that group. Possibly this was not intended by the reviewer, as it certainly could not have been intended by Fritsch, who knows my views quite well; but in case it should be misunderstood I beg to say that I have not held this view, but on the contrary have confined the name Microsauria to species with simple teeth, and have placed *Dendrerpeton* with Labyrinthodonts, though by no means as a typical genus of that group. In my last paper on this subject (Trans. Royal Society, 1882), I expressly exclude the two species of *Dendrerpeton* referred to from Microsauria, and define the latter as having non-plicated teeth (pp. 634-35). I may add, however, that I have always held and now hold that the Microsauria, though in some respects inferior to Labyrinthodonts, embrace in their structures premonitions of the true reptiles not found in the latter.

The study of these creatures was one of those bye-efforts thrust on me by circumstances, and which I have closed up so far as I am concerned in the paper referred to; but I have learned to love the little Microsaurs and to regard them as a hopeful and prophetic group. J. WM. DAWSON.

McGill College, January 26.

A New Historic Comet?

PERMIT me to inform Mr. Knott that the "new historic comet" is not a new comet at all. He will find it as No. 154 in Mr. Chambers's Catalogue No. II. in his well-known "Handbook of Descriptive Astronomy." It is there described, 302 A.D. "in May–June a comet was visible in the morning—(Ma-tuoan-lin: Williams 26)." W. H. S. MONCK.

Dublin, February 10.

The Proposed Teaching University for London.

No one, I am sure, who has carefully read my letters in the *Times* on this subject could agree with the writer of your article that I appear "to consider the dispute as one between the efficiency of 'lectures' on the one hand, and of 'reading' on the other."

The writer of the article has certainly misunderstood my views "upon the matters in dispute," as well as my object in quoting Darwin's dictum on the advantages of "reading" as compared with "lectures." PHILIP MAGNUS.

Exhibition Road, London, S.W., February 10.

Institute of Chemistry.

WITH reference to a circular letter dated the 12th inst., and bearing the signature of Mr. W. Thomson, which has been sent to the Fellows of the Institute of Chemistry, we beg that you will be so good as to allow us to inform the Fellows, through your columns, that we have not been consulted in regard to the action taken by Mr. Thomson, and that we decline to offer ourselves as candidates for election in opposition to the nominations of the Council. BOVERTON REDWOOD.

London, February 20. ALFRED GORDON SALAMON.

CORAL FORMATIONS.

I DESIRE to call attention to a condition of reef that I think has been very little studied, but that may contain a clue to a solution of some of the difficulties that still surround the subject of coral formations generally.

I may as well at once avow myself to be one of those who, on reviewing the later evidence on coral growth, have come to the conclusion that it is sufficient to justify an abandonment of the supposition that subsidence plays a principal part in the production of barrier reefs and atolls, but are at the same time not satisfied with one part of the explanation offered by Mr. J. Murray.

I refer to the great effects attributed by him to the disintegration and solution of dead coral by the chemical action of sea-water, in hollowing out and deepening the large and deep lagoons inside both these classes of reefs.

Mr. Murray's theory on this point, as summarized by himself, is that—

"(1) When coral plantations build up on submarine banks, they assume an atoll form, owing to the more abundant supply of food to the outer margins, *and the removal of dead coral rock from the interior portions by currents and by the action of the carbonic acid dissolved in sea-water.* (2) That barrier reefs have built out from the shore on a foundation of volcanic debris, or a talus of coral blocks, coral sediment, or pelagic shells, and *the lagoon channel is formed in the same way as a lagoon.*"

The italics are mine, and indicate the part of his theory to which from my view, and, I believe, that of others, there are objections, but to which Mr. Murray attaches considerable weight.

Is it necessary thus to invoke the aid of dissolution of the dead coral by chemical action as an important agent in the formation of these deep lagoons and channels? I think not.

An examination of the forms of, and depths on, well-surveyed submerged banks in different regions reveals a considerable number of reefs, which, if their development continues on the same lines as apparently heretofore, must, in the course of time necessary to bring them to the surface, form perfect atolls of large size, inclosing deep lagoons, without any further scooping out by solution.

Many instances occur in the China Sea. The Tizard Bank, in lat. 10° 20′ N., and long. 114° 25′ E., is 32 nautical miles in length, with an extreme breadth of 10 miles, and was well surveyed in 1867. The central portion is very flat and almost void of patches. Its depth is from 30 to 47 fathoms. Its edge is crowned with a coral rim varying from 4 to 10 fathoms in depth, broken here and there by openings, in some cases over 30 fathoms deep. The bank rises steeply from deep water, but, as no sectional soundings were taken, the precise angle of slope is unknown. The rim is composed of coral in luxuriant growth, and it can scarcely be doubted that in time it will reach the surface. In fact, on its periphery of 100 miles, in eight places small patches of reef, three of which bear islets, have already done so.

When the remaining portions of the rim are also awash, the reef will be in all respects an atoll similar to the great Maldive atolls, without any necessity for solvent action enlarging or deepening it.

Eight other banks of similar character, and in various stages, occur not far from this reef.

The great Macclesfield Bank, farther north, over 70 miles in length, and 40 miles in width, is of precisely the same nature, but its development is not so far advanced; the rim being in no spot nearer the surface than 10 fathoms, the water on it varying from that amount to 19 fathoms, while the depth of the inclosed area is from 40 to 60 fathoms. The survey of this bank is not so complete as in the case of some others, but enough has been done to show its character very plainly.

The Prince Consort Shoal (300 miles S.W. of the Tizard Bank) is apparently at a still earlier stage, a few patches of 17 fathoms and a considerable area of 30 fathoms partly inclosing a central area of 40 fathoms depth. The great Seychelles Bank in the Indian Ocean, 200 miles by 100, is very imperfectly known, but in most places the lines of soundings over its edge exhibit this tendency to form a rim. Here, however, the general depth on the bank is not over 30 fathoms. The Amirante Bank is a similar example.

The evidence afforded by these reefs has probably escaped notice from the fact that as published in charts for the purpose of navigation they are mostly shown on a very small scale, in which their character is scarcely apparent. The original manuscript surveys in the records of the Hydro-

graphic Department being not generally available, no opportunity has been afforded for their examination, but on these their form is highly suggestive. It should also be remembered that few submerged banks have yet been surveyed on a sufficiently large scale and in sufficient detail to show their characteristics.

I may, however, add that it is well known to seamen experienced in navigation in coral waters that shallower soundings are frequently found on the edges of banks.

How precisely it comes about that coral is growing on the yet deep rims of these large banks, and that little or none is flourishing in the interior, evidence is yet wanting to show. These, however, are the facts, and the result, so far as the necessity for future scooping out is concerned, seems indisputable. I may nevertheless offer a suggestion.

This condition of reef is apparently only to be accounted for in two ways. Either by subsidence; or by assuming that the animals, be they corals or other lime-secreting organisms, that settle on the bank, do, when it gets, by their accumulation, within a certain distance of the

surface, and under certain conditions of currents and food supply, intercept so much of the food borne in by the currents, that similar life, suitable to that depth zone, cannot be supported in the central area. Thenceforward, the rim alone will grow, and the organisms fitted to live in the successively shallower zones to the surface will only find on it foothold.

This would be the perfect atoll, but, with less nicely balanced conditions, growth would also take place in patches in the central area, as is so often the case.

It is to be observed that the depths on the more or less level tops of these large banks, as also in many perfect atolls, is frequently between 30 and 50 fathoms.

The smaller the bank, the less would be its general depth when the rim is formed, owing to the different proportions in which an area and its periphery, over which the non-consumed food must pass, relatively increase, and assuming that the animals that live nearer the surface consume more than those at greater depths.

I am fully aware that this view of the growth of an

atoll is not new. Darwin says, "A bank at the proper depth beneath the surface would give rise to a reef which could not be distinguished from an atoll formed during subsidence." Murray says, "Very early in the history of such an atoll, and while yet several fathoms submerged, the corals situated in the central parts would be placed at a disadvantage." It does not, however, appear to have been contemplated that the inclosed lagoon would, under any circumstances, without some further agency than the simple growth of the rim, be so deep as it appears to me, from the cases above cited, that it can be ; nor, so far as I can find, have any such instances been before remarked.

Darwin notices the case of the Chagos Bank, but, on the authority of Captain Moresby, he states that the rim is dead, and concludes that life was killed by subsidence, and he apparently also infers that it will not grow to the surface.

I can find no fresh evidence on this particular reef, but for some of the banks in the China Sea I have the independent testimony of two of the officers employed in their survey, Staff-Commanders Tizard and Petley, R.N., that the coral growth is most luxuriant.

Touching for a moment on the point of the formation of banks at a proper distance below the surface ; the investigation of banks in the Atlantic, recently described by Mr. J. Buchanan, shows that banks with almost wall-like slopes are growing up by the accumulation of organisms.

Turning to barrier reefs, similar instances can be found. Off the coast of the island of Palawan, a shallow rim is forming on the edge of the bank which stretches from the island to a distance varying from 15 to 30 miles, having a general depth on it of 40 fathoms. The length of the rim is 250 miles, and it consists of streaks and patches of coral with from 4 to 30 fathoms on them.

On the south of the eastern end of New Guinea, a formation, known to navigators as the sunken barrier, lines the edge of a bank similar to the above, and is of precisely the same character. Its length is 140 miles, and the depth between it and the land varies from 30 to 60 fathoms.

Dr. Guppy has recently pointed out some smaller instances of the same tendency in the Solomon Islands, and has made some remarks on the formation of barrier reefs in the same sense as my suggestions. I am not therefore advancing anything novel, but simply pointing out evidence which tends to show that the principle may be carried further than has hitherto been supposed.

Looking now at the fringing class, how comes it that so many wide reefs of this character exist, which, if Mr. Murray's contention be correct, should surely show more signs of the formation of a lagoon channel than they do?

Take the case of Rodriguez, in the Indian Ocean, with which I am personally intimately acquainted. Here a fringing reef surrounds the western sides of the island for a width of 4½ miles. There are narrow channels it is true, but so shallow that in many places boats cannot pass at low water. The island is situated in the heart of the strong trade winds, and the reefs are exposed to a heavy sea, which, with a rise of tide of nearly 6 feet, gives every facility for scour and rapid change of water.

I am not disputing the fact that calcareous dead organisms are dissolved by carbonic acid. I am no chemist, and moreover the *Challenger* observations amply prove it, but I would ask Mr. Murray if there is not a great difference between the position of small shells falling in water which completely surrounds them whilst they are constantly coming in contact with fresh particles of it, and of the more or less solid mass of a coral reef, which can only be attacked on its upper side to advantage, and where the resultant fine mud covers and protects the remaining rock, especially in the case of lagoon channels, when the bottom is partly composed of detritus from the land.

At the bottom of a lagoon of any depth, moreover, the motion of the water must often be comparatively slight, and the action consequently extremely slow.

The rotten state of the surface of every coral reef awash with the water shows that this disintegration is going on, but the fact that for large areas it remains awash, and must have so remained for ages, seems to me to point to the supposition that the removal of matter is too insignificant to account for the formation of deep lagoon channels in this manner, though doubtless it may explain the shallow pools and creeks found in all fringing reefs.

I have addressed myself solely to one point in this many-sided question, but I may add my opinion that, before any explanation which will fully account for the almost infinite variety of coral formations, can be given, much more knowledge of details of the complex conditions under which they may grow is required.

Certain knowledge of the depths at which different corals and other lime-secreting animals live under varying circumstances ; of the amount of food carried in the different strata and in different waters ; of the effect of the velocity of the currents that bring the food to the banks ; and more accurate surveys on large scales, especially of the shapes and contours of coral reefs, and of their composition, are all wanting. These details must greatly affect coral growth, and the results must greatly vary. On the other hand, similar results may be brought about by different causes.

It may surprise some to learn how little in the preliminary matter of surveys alone has been done in the principal coral-reef regions, especially in the Pacific, which is generally quoted ; and consequently how very inexact our knowledge is of the depth both inside and outside of the majority of atolls in the world, and also of the state of the bottom of the sea, on which it is very possible that many elevations may be found in the condition of those to which I have called attention.

In the Pacific the vast majority of islands have been merely sketched without a single sounding having been taken, either inside or outside lagoons.

I append a few statistics relating to the larger coral groups to show our position in this respect ; merely remarking that the waters of the Fiji and the Society Islands are the only ones which can be said to be in any sense surveyed.

	No. of Islands.	No. surveyed.
Paumotu Islands	74	1 partially.
Ellice ,,	10	none.
Gilbert ,,	16	none.
Marshall ,,	30	none.
Caroline ,,	43	3 partially.
Tonga ,,	6 groups	2 groups partially.

W. J. L. WHARTON.

THE AKKAS, A PYGMY RACE FROM CENTRAL AFRICA.

At the last meeting of the Anthropological Institute, Prof. Flower gave a description of two skeletons of Akkas, lately obtained in the Monbuttu country, Central Africa, by Emin Pasha, and by him presented to the British Museum. Since this diminutive tribe was discovered by Schweinfurth in 1870, they have received considerable attention from various travellers and anthropologists, and general descriptions and measurements of several living individuals have been published, but no account of their osteological characters has been given, and no specimens have been submitted to careful anatomical examination. The two skeletons are those of fully adult people, a male and a female, but unfortunately neither is quite complete. The evidence they afford entirely corroborates the view, previously derived from external measurements, that the Akkas

are among the smallest, if not actually the smallest, people upon the earth. There is no reason to suppose that these skeletons were selected in any way as exceptional specimens, yet they are both of them smaller than any other normal skeletons known, smaller certainly than the smallest Bushman skeleton in any Museum in this country, and smaller than any out of twenty-nine skeletons of the diminutive inhabitants of the Andaman Islands, of which the dimensions have been recorded by Prof. Flower in a previous paper communicated to the Institute. The most liberal calculation of the height of these two skeletons places that of the male at about an inch below 4 feet, and the female at less than an inch above. We may say 4 feet, or 1·219 metre, as the average height of the two, while a living female of whom Emin Pasha has sent careful measurements is but 1·164 metre, or barely 3 feet 10 inches. The results previously obtained from the measurements of about half-a-dozen living Akkas are not quite so low as these, varying from 1·216 to 1·420, and give a mean for both sexes of 1·356, or 4 feet 5½ inches. Schweinfurth's original measurements were unfortunately lost, and the numbers since obtained are quite insufficient for establishing the true average of the race, especially as it is not certain that they were all pure-bred specimens.

In the list given in the third edition of Topinard's "Anthropologie" (1879) only two races appear which have a mean height below 1·500 metres, viz. the Negritos of the Andaman Islands 1·478, and the Bushmen 1·404. Of the real height of the former we have abundant and exact evidence, both from the living individuals and from skeletons, which clearly proves that they considerably exceed the Akkas in stature. That this is also the case with the Bushmen there is little doubt, although the measurements of this diminutive race are less numerous and carefully made.

The point of comparative size being settled, it remains to consider to what races the Akkas are most nearly allied. That they belong in all their essential characteristics to the black or Negroid branch of the human species there can be no doubt, in fact they exhibit all the essential characteristics of that branch even to exaggeration. With regard to the somewhat more rounded form of head (the cephalic index in these examples being 74·4 and 77·9 respectively), Hamy has long since pointed out that in equatorial Africa, extending from the west coast far into the interior, are scattered tribes of Negroes, distinguished from the majority of the inhabitants of the continent by this special cranial character, as well as by their smaller stature. The Akkas are grouped by Hamy and Quatrefages as members of this race, to which the distinctive name of "Negrillo" has been applied. Their small size has naturally led some anthropologists, including Schweinfurth, to ally them to the diminutive African race inhabiting the southern part of the continent—the Bushmen; but beyond certain characters met with in the whole Negroid branch, including the frizzly hair, there is little in common between them. The Bushmen are a very strongly marked race, and both their external appearance and osteological characters are so exceptional that they can never be confounded with any other. The natives of the Andaman Islands have also very distinctive characters, which they do not share with the Akkas, whose position all recent investigations show to be that assigned to them by Hamy as members of the Negrillo division of the Negroid branch of mankind. It is possible that these people gave origin to the stories of pygmies so common in the writings of the Greek poets and historians, and whose habitations were often placed near the sources of the Nile. The name "Akka," by which, according to Schweinfurth, the tribe now call themselves, has, singularly enough, been read by Mariette Pasha by the side of a portrait of a dwarf on a monument of the ancient Egyptian empire.

REV. JOHN HEWITT JELLETT, D.D., D.C.L.

IT is with extreme regret that we announce the death of the Reverend the Provost of Trinity College, Dublin. He died last Sunday evening after a very short illness.

The Provost was present in his usual health at the Spring Commencements, which were held on the 14th inst. in the Hall of Trinity College; on the 16th he was not quite well; on the 18th some dangerous symptoms appeared; and the end came, painlessly and unexpectedly, on the 19th inst.

John Hewitt Jellett was born at Cashel in the county of Tipperary on December 25, 1817. He entered Trinity College, Dublin, when seventeen years of age. Obtaining a Scholarship in 1836, he graduated as a Senior Moderator and Gold Medallist in Mathematics in 1837, and three years afterwards he obtained a Fellowship.

In 1848, he was appointed to the Professorship of Natural Philosophy; in 1870, on the death of the Rev. Dr. Luby, he was co-opted a Senior Fellow of Trinity College, and on the death of the Rev. Dr. Lloyd, in 1881, he was nominated by the Crown to the Provostship.

Twenty years ago he was made one of the Commissioners of National Education in Ireland, and he was President of the Royal Irish Academy from 1869 to 1873.

In 1850, Jellett published his well-known "Treatise on the Calculus of Variations," a subject which had engaged the attention of some of the noblest mathematical intellects of the world. The volume contains improvements of previously existing methods, which, had they been given as so many separate treatises, would in themselves have formed no ordinary title to fame; but the author's aim was rather to compile a memoir which would enable the earnest student to be on a level with the knowledge he had himself attained to, however little that aim might be to his own glory.

For this work the Royal Irish Academy awarded Jellett in 1851 their Cunningham Gold Medal. In 1872 appeared the "Treatise on the Theory of Friction," a work well known and highly appreciated. In addition to these volumes a number of scientific memoirs were from time to time published by him in the Transactions of the Royal Irish Academy, and in Leonville's *Journal de Mathématique*, of which perhaps the more important were on the "Equilibrium and Motion of an Elastic Solid" and "On Researches in Chemical Optics."

Like other well-known mathematicians of the Dublin University, Jellett was as much thought of for his pulpit discourses as for his scientific memoirs. He was of good presence, had a clear articulation and a very persuasive style; and his appearance in the pulpit of the College chapel was always welcomed. When he dwelt on the moral difficulties of the Old Testament, none went their way without being impressed by the straightforward honesty of the man.

Occupying a very conspicuous position in a University city like Dublin, the Provost seemed to command the respect of all. Some perhaps there are who would have preferred that the head of so ancient a seat of learning should have taken no part in modern political strife; but, even amid the feverish excitement of party warfare, the Provost of Trinity College, though he often fearlessly and eloquently put forward his own views, did so without giving offence to any.

Intimately associated, from the year 1834, with the life of the College over which he at the last presided, the Provost knew much of, and was a prime mover in, many of the changes which have marked out a new life in the place. Within its walls he was known and respected, while the tribute to his memory paid by all classes of the Dublin citizens is a striking proof of how he was loved.

This is not the place to dwell on the intense loss his

death brings to those who had experienced the charm of that hidden inner life which was known to his friends as both kind and just, affectionate and sympathetic.

NOTES.

THE retirement of the veteran Prof. Prestwich from the Chair of Geology at Oxford is an event which cannot be chronicled without regret. But it is pleasant to know that he relinquishes the post which he has dignified for so many years to find in the quiet of his country home that leisure and rest to which his long devotion to the cause of science so justly entitles him. He has crowned his professorial career by the publication of the splendid volume which completes his great work on geology. On the very day after the appearance of that volume the electors met at Oxford to select from the numerous candidates a successor to fill his place. His University and the science of geology may both be congratulated on their choice. Prof. Green, whom they have chosen, is one of the most accomplished geologists in the country, one who has been trained in the practical school of the Geological Survey, who has done admirable original work, and who possesses in no common measure the power of luminous exposition. He is gifted, moreover, with a faculty in which geologists are often singularly defective, that of mathematical investigation, and we may hope that one of the results of his transference to Oxford will be to afford him an opportunity of devoting himself to the attack of many geological problems from the mathematical side. He carries with him to his new sphere of labour the best wishes of all to whom the progress of geology and the cultivation of science at the Universities are dear.

ON May 27 next Prof. F. C. Donders, of Utrecht, will be seventy years of age. The law requires that he shall then resign his duties as Professor at the University and as Director of the Physiological Laboratory, and it is thought that men of science in all parts of the world may be glad to take the opportunity of expressing their appreciation of the great services Prof. Donders has rendered to the study of physiology and physiological optics. An influential committee has been formed in Holland for the purpose of giving effect to this idea, and the proposal is that Prof. Donders' name should be connected in a permanent way with the spot where he has lived and worked for more than forty years, by the creation of a fund to be devoted to a scientific purpose, and to be known as the "Donders Memorial Fund." The uses to which the fund will be put, and the rules by which its administration will be governed, will of course be determined in accordance with the wishes of Prof. Donders. The Ophthalmological and Physiological Societies have taken the matter in hand in this country, and we have no doubt that the committees they have appointed will do their work satisfactorily. It ought not to be difficult for them to secure an adequate token of the respect felt in England for an illustrious man of science to whom the medical profession and the public are so deeply indebted. It is proposed that the amounts contributed by the several donors be not specified, but that they be grouped into a common sum for transmission to the Dutch Committee; and that the names of those contributing be inscribed in a suitable form for presentation to Prof. Donders. For this reason smaller as well as larger subscriptions will be acceptable. Subscriptions may be sent to Dr. Gerald F. Yeo, Secretary of the Committee of the Physiological Society (address—King's College, London, W.C.), or to Dr. W. A. Brailey, Secretary of the Committee of the Ophthalmological Society of the United Kingdom (address—11 Old Burlington Street, London, W.). Subscriptions may also be sent to the office of this journal.

AT a meeting of the Royal Society of Edinburgh, held on January 30, Profs. Clausius, Haeckel, and Mendeléeff were elected Honorary Fellows.

SIR JAMES PAGET, F.R.S., has consented to give the annual address to the students of the London Society for the Extension of University Teaching, at the Mansion House, on Saturday, March 3, at 3.30 p.m., under the presidency of the Lord Mayor. The subject of the address will be "Scientific Study."

A MARBLE medallion portrait of the distinguished palæontologist, Dr. Thomas Davidson, F.R.S., first chairman of the Brighton Museum Committee, was unveiled in the geological room of the Free Town Museum, Brighton, by the Mayor, Mr. E. Martin, on Friday, the 17th inst. The medallion, which is much admired, is the work of Mr. Brock, A.R.A. It was presented to the town on behalf of the subscribers by Mr. Edward Crane, chairman of the Museum Committee, who referred in detail to Dr. Davidson's services to science and to the Museum. Sir R. Owen sent a letter regretting that failing health prevented his paying the respect of personal attendance to the memory of his distinguished fellow-worker. Prof. Judd also wrote bearing cordial testimony to the skill and enthusiasm with which Dr. Davidson carried on his researches.

GENERAL PERRIER, the most eminent French authority on geodesy, died at Montpellier on Monday at the age of fifty-five. He had attained the rank of Brigadier-General in the French army, and was at the head of the Geodetic Department at the War Office. He was a member of the Academy of Sciences.

THE relations of science and religion do not form one of those topics which we permit ourselves to discuss in NATURE. At the same time we may call attention to a series of three remarkable articles on "Darwinism and the Christian Faith" recently published in the *Guardian* (January 18, January 25, and February 1, 1888), and now reprinted as a pamphlet. The author is anonymous, but is understood to be an Oxford College tutor, and Honorary Canon of Christ Church. The orthodoxy of the *Guardian* is, we believe, unimpeachable. We notice therefore with gratification that not only is Darwinism thoroughly accepted and lucidly expounded by the writer in the *Guardian*, but that he is an exceptionally well-informed and capable critic, whose scientific knowledge is varied and sound. The publication of these articles in the *Guardian* is a proof that the clergy as a body are not so unwilling to accept new scientific views as might be supposed were we to regard Dean Burgon as a fair sample of his class. The *Guardian's* contributor discusses the difficulty of reconciling the existence of a just, omnipotent, and omniscient God with the existence of pain and the ceaseless "struggle for existence," and *à propos* has a remark tinged with local colour which is worth reproducing. "And yet," he says, "man, who is so wise and good that he is always saying, with King Alphonso of Castile, 'If God had called me to His councils things would have been in better order,' has invented competitive examinations, which mean suffering and pain for all, without even a compensating 'survival of the fittest' or improvement of the race!" We believe that competitive examinations were invented by the Chinese, and introduced into Europe by Jesuit missionaries. The Chinese are celebrated among the nations of the world for the elaborate system of cruel tortures employed in their administration of justice. On the other hand, we owe tea and many other nice things to them.

THE annual winter meeting of the Department of Superintendence of the U.S. National Education Association was held lately at Washington. The most important topics treated were "How and to what extent can Manual Training be ingrafted on our System of Public Schools?" and "How can the Qualifications of Teachers be determined?"

THE Archæological Congress which is to be held at Moscow, in 1890, will have an international character, many German, French, Italian, and other men of science having already been invited to attend. The Congress will consider the following subjects: (1) prehistoric antiquities; (2) historical, geographical, and ethnographical questions; (3) Russian art monuments; (4) ecclesiastical monuments; (5) Slavo-Russian language and writing; (6) Slavo-Byzantine and West European antiquities; (7) Oriental and heathen antiquities; (8) latest progress of archæology.

MRS. ZELIA NUTTALL has been elected a Fellow of the American Association for the Advancement of Science in recognition of her researches in Mexican archæology.

A TRANSLATION of Dr. E. B. Tylor's hand-book of "Anthropology" into Spanish by D. Antonio Machado has just been published in Madrid. The author contributes a special preface drawing attention to the valuable anthropological material still to be found in Spanish America.

THE first number of what will no doubt prove to be an important and valuable periodical has just been issued. It is called *Internationales Archiv für Ethnographie*, and is edited by J. D. E. Schmeltz, of the Ethnographical Museum of Leyden, who has received promises of co-operation from many of the foremost ethnologists and anthropologists in Europe and America. The present number (which contains German, Dutch, and French contributions) opens with a striking article, in German, by Dr. L. Serrurier, Director of the Leyden Ethnographical Museum, on the arrows of New Guinea. Representations of the various types of New Guinea arrows, admirably printed in colours, illustrate this interesting paper.

WE have received the first number of *The American Anthropologist*. This new quarterly periodical is issued under the auspices of the Anthropological Society of Washington, and the editorial committee seek the co-operation of all who are interested in the advancement of anthropological science. In the first number there are papers on the law of Malthus, by Dr. James C. Walling; the development of time-keeping in Greece and Rome, by F. A. Seely; the human hand, by Dr. Frank Baker; and the Chane-abal (four-language) tribe and dialect of Chiapas, by Dr. D. G. Brinton.

THE *American Meteorological Journal* for January contains:—(1) An article by F. Waldo on instruments for measuring atmospheric pressure, showing the differences that exist in the standard barometers of different countries, and that the standards do not always remain constant for a number of years. (2) A paper by Prof. H. A. Hazen on the exposure of thermometers, with a discussion of a new plan proposed by Dr. R. Assmann. The latter paper was presented to the Berlin Academy in November last. (3) On a thirty-day period of the weather, by H. Helm Clayton. The writer considers that the period is strongly substantiated by facts, although at present they remain empirical facts.

THE Deutsche Seewarte has published the seventh volume of the results of meteorological observations for 1° squares of the North Atlantic Ocean. The object is to discuss the observations collected by German and Dutch vessels between latitude 50° and 20° N. in the North Atlantic, adjoining the district of the nine equatorial 10° squares between latitude 20° N. and 10° S., and longitude 10° and 40° W., the observations for which have been discussed by the Meteorological Council. The district now covered by the two institutions embraces 60° of latitude and 30° of longitude, with the exception of one 10° square, which will shortly be published. The German observations are published in a tabular form, showing for each degree the direction of the winds under sixteen points, the number of storms, the mean wind force, pressure, temperature of air and sea, rainfall, and other particulars. The number of observations for each subdivision is sometimes small, but always quoted, and in their present form the observations may be added to subsequently, or amalgamated with those of other countries. A small but important district south of 20° N. (the limit of the German investigation) and west of 40° W. (the limit of the English investigation), embracing the region of the origin of the West India cyclones, has yet to be undertaken by some body, to complete these important contributions to maritime meteorology.

A NEW chloride of gold, Au_2Cl_4, has been prepared by Prof. Julius Thomsen, of Copenhagen (*Journ. für Prakt. Chemie*, 1888, No. 2). The method of preparation is remarkably simple, gaseous chlorine being merely brought into contact with gold in a fine state of subdivision and at a moderately elevated temperature. About 50 grammes of finely divided gold, obtained by precipitating a solution of the trichloride with sulphurous acid, was thoroughly washed, partially dried to the consistency of a thick mud, and placed in a previously weighed glass tube. At its lower end the tube was drawn out and the delivery tube of a chlorine generator sealed on to it. A stopper and exit tube at the upper end completed the arrangement. A rapid stream of chlorine gas (half a litre per minute) was then passed through the apparatus, the lower end of the wider portion containing the gold being gently heated to start the reaction. The whole was then placed in a glass beaker surrounded by cotton-wool in order to prevent too rapid cooling, by which device the heat of the reaction itself was sufficient to complete the combination. At the commencement the absorption of chlorine was perfect, not a bubble escaping, but at the expiration of half an hour the point of saturation was reached. After expelling the uncombined chlorine the tube was again weighed, and the amount of chlorine thus taken up determined. In every experiment the proportion of chlorine to that of gold was found to be very slightly more than two to one, the average ratio being 2·09 to 1·0. The slight excess of chlorine was due to minute spangles of trichloride of gold sparsely disseminated throughout the mass. The simplicity of this mode of preparation and the constancy of the results may perhaps excite wonder that Au_2Cl_4 has not hitherto been as well known as AuCl and $AuCl_3$. As a matter of fact, Prof. Thomsen discovered it several years ago, and published his results, but owing to the adoption of different methods by later workers the conclusions of Prof. Thomsen were not considered confirmed. Now that the work has been repeated and completely verified there is no longer any reason why Au_2Cl_4 should remain in the background. It is interesting theoretically as being the aurous salt of chlor-auric acid, $HAuCl_4$.

FROM the Annual Report of the New York State Department of Public Instruction, it appears that during last year over 31,000 teachers were employed in the State of New York, and that of this number only 5821 were males. The number of children of school age was 1,763,115; the total enrolment, 1,037,812; the average attendance, 625,610. Mr. Draper, the Superintendent, the author of the Report, says that the attendance in the schools does not keep pace with the growth of the population, and that the uneducated class is increasing.

COLONEL LE MESSURIER has just brought out a third edition of his useful pocket hand-book on the "Game, Shore, and Water Birds of India." The utility of this unpretending little work has been vouched for by the call for its re-issue in an octavo form with the addition of many drawings made by the author during his recent furlough in England, and we are glad to see to what practical use he has turned some of Prof. Flower's exhibits at the Natural History Museum. Colonel Le Messurier writes as a field-naturalist for field-naturalists and sportsmen, without any

great pretensions to scientific knowledge, but there is no doubt that all naturalists will gain useful hints from this little volume, which is profusely illustrated with woodcuts, giving the characteristic features of most of the species.

"THE STATESMAN'S YEAR-BOOK" for 1888 has been published. It contains additions and alterations which largely increase the value of the work, and the statistical and other information has been brought up to the latest available date.

WE have received three issues of the "Annuario" published by the Imperial Observatory at Rio de Janeiro—the issues for 1885, 1886, and 1887. The work is well compiled, and the editor evidently takes great pains to secure that each issue shall be decidedly better than its predecessors. Besides the usual collection of astronomical facts, the work contains useful tables relating to meteorology, chemistry, physics, geography, and other sciences.

THE "Annuaire Géologique Universel" of Dr. Dagincourt, which has just been issued for the third time, has been much enlarged and improved. The new volume contains an exhaustive review of recent work in palæontology and geology.

A FOURTH edition of Prof. Nichol's "Tables of European History, Literature, Science, and Art, from A.D. 200 to 1888" (Maclehose) has been issued. The idea of the work is good, but we cannot say that the scientific tables are always quite satisfactory. In his lists of men of science the compiler includes the names of some writers who have a very inadequate claim to the place he accords to them.

THE Trustees of the Australian Museum have issued a descriptive catalogue, by Dr. R. von Lendenfeld, of the Medusæ of the Australian seas. Speaking of the Scyphomedusæ, Dr. von Lendenfeld says that he has observed three species in New Zealand, three species on the coast of Victoria, and five species in Port Jackson. Two of the latter are identical with the Victorian species. Of the nine species, six have been described by Dr. von Lendenfeld; his specimens of the remaining three were not sufficiently well preserved for description. The difficulty connected with the preservation of these beautiful animals has, he points out, been a great obstacle in the way of a thorough knowledge of them.

THE fifth volume of the collected works of Paul Broca has just been issued by M. Reinwald, in Paris. This volume, which, like the others, is sold separately, is particularly interesting to zoologists. It contains Broca's numerous and important memoirs on the brain of man and primates. It is well illustrated.

M. ZOGRAFF's new work on the structure of the *Acipenser ruthenus*, which appeared in the *Izvestia* of the Moscow Society of Amateurs of Natural Science (vol. lii. fasc. 3), will be most welcome to zoologists. Following the methods adopted by Günther, Johann Müller, and Pallas, and more especially by the Swedish ichthyologist, F. A. Smitt, who has applied the system of numerous measurements used in anthropology to the study of fishes, M. Zograff has undertaken to give anew a complete description of the Russian species of *Acipenser*, and the Central Asian species of *Scaphirhynchus*. He begins his work by a general description of the body of the Russian Acipenseridæ: the varying shapes of their heads; the indexes of length, width, and thickness of the body; the skin and spines; the teeth; the muscles; and the brain. The whole is accompanied by numerous engravings and coloured plates, great attention being given to the minute anatomy of all parts of the different species.

AMONGST the papers contained in the last issue of the Transactions of the Seismological Society of Japan (vol. xi.) is one on earth tremors in Central Japan, by Prof. Milne. The paper is a continuation of one on the same subject read before the same Society in 1883, which was referred to in these columns at the time. In the present paper the writer discusses recent investigations into earth tremors in Italy, describes tremor recorders, with special reference to an automatic tromometer, gives numerous tables of records of the latter instrument, and finally refers to the subject of earth tremors on mountains. The paper is one of great length, and is accompanied by numerous charts and tables, which make it a respectable volume in itself. The conclusions may, however, be given in a brief space. Prof. Milne says that his chief object has been to show the relationship which earth tremors hold to barometrical fluctuations, barometrical gradients, and the wind. He concludes that they are more frequent with a low than a high barometer, but even with the former they may often not be observed; that with a high gradient they are almost always observed, but with a small gradient only seldom: that the stronger the wind the more likely they are to be observed; when there has been a strong wind and no tremors it has often been a local wind, or one blowing inland from the Pacific Ocean; the recorded earthquakes do not appear to be connected with earth tremors, more than that both are more frequent at the same seasons; and tremors are as severe on the summit of a lofty mountain as on the plains. So far as his observations have hitherto gone in Japan, it appears that the majority of earth tremors are movements produced by the action of the wind upon the surface of the earth, and that these may often be propagated to distant places where wind disturbances have not occurred.

ON January 13, at 11.10 p.m., a faint shock of earthquake was felt in the district of Örebro, in Central Sweden. It was not accompanied by any subterranean noise.

AT five o'clock on Sunday afternoon a cyclone broke over Mount Vernon, a town in Illinois, sixty miles to the south-east of St. Louis. Many persons were killed or injured, and five hundred buildings were demolished in a few minutes. The cyclone is said to have come up from the south-west with a rotary whirling motion, sweeping a path five hundred yards wide and several miles long, within which everything was destroyed.

DURING last month the so-called "red after-glow" was observed at sunset in the vicinity of Stockholm. Varying in intensity the glare extended considerably towards the zenith.

THE Finnish Government is on the point of organizing a number of stations along the coast of Finland for the observation of the nature and peculiarities of the drift-ice during the winter months.

A LARGE block of stone with rude drawings and some Runic inscriptions has just been discovered in the island of Tjörn, on the south-west coast of Sweden. It is of particular interest as being the first of its kind found in the southern part of the province of Bohus.

A MEETING has just been held at Tönsberg, in Norway, of those interested in the Arctic seal fisheries in that country, for the purpose of considering the Scottish Fishery Board's proposals that the close time for seal should end on April 10 instead of as at present April 3, and begin on July 10 instead of July 15. Both proposals were unanimously rejected, the reason advanced being that their adoption would tend to ruin the industry, so far as Norway is concerned. The Scotch proposal that young and old seal should be treated alike during the open season was adopted. Finally, the following resolutions were passed: (1) that it was advisable that the close season should end at 6 a.m. on April 3 instead of at midnight; (2) that the law of preservation of seal should be altered so that the area covered by it should range from 60° to 70° N., and from 10° E. to the coast of Greenland. A report of the meeting will be forwarded to the Scottish Fishery Board for their consideration.

THE Norwegian Fishery Promotion Society of Bergen has petitioned the Government for a grant of £15,000 for the development of the deep-sea fisheries of Norway.

THE Merchant Taylors Company have recently voted ten guineas to the Parkes Museum to aid in its work of practical teaching and demonstrating sanitary science.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀), two Rhesus Monkeys (*Macacus rhesus* ♀ ♀) from India, an Alpine Marmot (*Arctomys marmotta* ♀), European, a Vulpine Phalanger (*Phalangista vulpina* ♂) from Australia, presented by Mr. H. Austin Clow, F.Z.S.; three Esquimaux Dogs (*Canis familiaris*) from Greenland, presented by Mr. W. T. Tournay, F.Z.S.; three Derbian Wallabys (*Halmaturus derbianus*) from Australia, presented by Lieut. C. M. Hepworth, R.N.R.; four Alpine Accentors (*Accentor alpinus*), European, presented by the Lord Lilford, F.Z.S.; a Cardinal Grosbeak (*Cardinalis virginianus*) from North America, presented by Mr. Ayerst; four Lion Marmosets (*Midas rosalia*), an Eyra (*Felis eyra*) from Brazil, four Parrot Finches (*Erythrura psittacea*) from New Caledonia, two Common Gulls (*Larus canus*), a Black-headed Gull (*Larus ridibundus*), British, purchased; four Cereopsis Geese (*Cereopsis novæ-hollandiæ*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

MR. TEBBUTT'S OBSERVATORY, WINDSOR, NEW SOUTH WALES.—Mr. John Tebbutt, the well-known and enthusiastic amateur astronomer of New South Wales, has just published a little pamphlet giving a history and description of his private observatory, the work of which, he remarks with justifiable pride, "has proved of sufficient importance to admit of Windsor being placed in the list of observatories in the British and American *Nautical Almanacs*, the *Connaissance des Temps*, and the *Berliner Astronomisches Jahrbuch*." And this distinction has been well earned, for the lists here given of observations made, and of papers contributed to various scientific publications, show the history of the little observatory to have been a most honourable one. Mr. Tebbutt has made all the observations himself, and until 1881 performed all the reductions; latterly he has received occasional assistance in the computations from his son or friends near. His instrumental equipment was for many years of the most modest description: for seven years it consisted principally of a sextant, and a telescope of 1⅝ inches aperture. In 1861 a refractor of 3¼ inches aperture, and in 1864 a transit instrument of 2 inches, were added. In 1872, Mr. Tebbutt became the possessor of an equatorial of 4½ inches, which was his chief instrument until about a year and a half ago, when he bought a fine 8-inch equatorial by Grubb, once the property of the late Dr. Bone, of Castlemaine. The observations made have been principally of comets, for a number of which Mr. Tebbutt has also computed orbits, but daily meteorological observations have been kept up for twenty-five years, the results of which have been published in five parts, and transit observations have been taken regularly for time. Mr. Tebbutt has also done good service to science by his papers on astronomical subjects in various organs of the colonial press, for hitherto the private pursuit of astronomy has been greatly neglected in the Australian colonies, and he has almost stood alone as an amateur observer. It is to be hoped that this record of his labours and his success may call forth many imitators.

PULKOWA OBSERVATORY.—The Report of this Observatory for the year ending May 31, 1887, refers to the heavy loss the institution sustained in the death of Dr. August Wagner. Owing to his death, the work of publication has been somewhat delayed; he had, indeed, finished a memoir on personal and instrumental errors for the introduction of vol. xii., but the materials he left for the stellar and planetary catalogues were not so readily dealt with. Still, it is expected that this volume, and the introduction to vol. xiv., may soon be ready for publication; vols. xv. and xvi., which will contain meridian observations for the period 1872–80, and the catalogue, are ready to follow vol. xiv. through the press. Of vol. viii., the catalogue, forming the first part—meridian observations 1840–69 of Bradley and other stars down to mag. 6—has already been distributed, and the remainder is in hand; vol. x., Prof. Struve's double-star observations, is still incomplete. The observational work of the Observatory has suffered no great change. The 30-inch refractor has been used by Dr. H. Struve for the measurement of the more difficult of Burnham's stars, the fainter satellites of Saturn, and the satellite of Neptune. The old 15-inch has been used by Drs. H. Struve and Hasselberg for photographic experiments, and by Prof. O. Struve for observations of Procyon, which has now been followed through nearly a complete period of its orbital motion.

WOLSINGHAM OBSERVATORY.—The Rev. T. E. Espin reports that during the last year he has continued his sweeping for red stars and stars with remarkable spectra, and that he has published spectra of 126 objects in the *Astronomische Nachrichten*, Nos. 2788 and 2825, of which eighty-six were found in the sweeps. Fifteen of the stars were of Secchi's type IV. Three new variables of long period have been discovered, and the usual observations of variables have been made and forwarded to Prof. E. C. Pickering. The Observatory has been enriched by the present from Canon Slatter of a fine 4·8-inch equatorial by Troughton and Simms. The new edition of Birmingham's Red Star Catalogue will be ready for the printer in a few weeks.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *FEBRUARY* 26—*MARCH* 3.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 26

Sun rises, 6h. 54m.; souths, 12h. 13m. 10·0s.; sets, 17h. 32m.: right asc. on meridian, 22h. 36·3m.; decl. 8° 48′ S. Sidereal Time at Sunset, 3h. 56m.

Moon (Full, February 27, 12h.) rises, 16h. 22m.; souths, 23h. 50m.; sets, 7h. 4m.*: right asc. on meridian, 10h. 15·3m.; decl. 12° 53′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	6 53	12 50	18 47	23 13·3	1 26 S.
Venus ...	5 39	9 58	14 17	20 20·4	19 26 S.
Mars ...	22 15*	3 33	8 51	13 55·0	8 58 S.
Jupiter ...	1 40	5 53	10 6	16 14·6	20 17 S.
Saturn ...	13 50	21 47	5 44*	8 11·6	20 35 N.
Uranus ...	21 8*	2 41	8 14	13 2·6	5 56 S.
Neptune..	9 38	17 18	0 58*	3 42·0	17 57 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Feb. 29,	18 56	*m*
Algol	3 0·9	40 31 N.	Mar. 1,	3 12	*m*
λ Tauri	3 54·5	12 10 N.	,, 3,	1 28	*m*
ζ Geminorum	6 57·5	20 44 N.	Feb. 28,	22 0	*m*
R Canis Majoris	7 14·5	16 12 S.	,, 29,	19 18	*m*
			Mar. 1,	22 34	*m*
U Monocerotis	7 25·5	9 33 S.	Feb. 26,		*m*
δ Libræ	14 55·0	8 4 S.	,, 29,	1 32	*m*
R Ophiuchi	17 1·3	15 57 S.	,, 28,		*M*
U Ophiuchi	17 10·9	1 20 N.	Mar. 1,	0 42	*m*
		and at intervals of		20 8	
X Sagittarii	17 40·5	27 47 S.	Feb. 26,	2 0	*M*
W Sagittarii	17 57·9	29 35 S.	Mar. 2,	5 0	*m*
R Lyræ	18 51·9	43 48 N.	,, 2,		*M*
U Aquilæ	19 23·3	7 16 S.	,, 3,	5 0	*m*
S Aquilæ	20 6·5	15 17 N.	Feb. 26,		*m*
Y Cygni	20 47·6	34 14 N.	,, 27,	19 24	*m*
			Mar. 1,	19 18	*m*
S Cephei	21 36·6	78 7 N.	,, 3,		*M*
δ Cephei	22 25·0	57 51 N.	,, 1,	21 0	*M*

M signifies maximum; *m* minimum.

Occultation of Star by the Moon (visible at Greenwich).

Feb.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
26 ...	7 Leonis ...	... 6½ ...	5 16 ...	6 7	... 112 291

March.	h.	
1 ...	23 ...	Mars in conjunction with and 2° 37′ south of the Moon.
3 ...	19 ...	Mercury in inferior conjunction with the Sun.

Meteor-Showers.

	R.A.	Decl.	
Near δ Virginis ...	... 192° ...	2° N. ...	March 2 and 3.
From Sagittarius ...	... 280 ...	17 S. ...	Very swift; streaks.

THE RELATIONS BETWEEN GEOLOGY AND THE BIOLOGICAL SCIENCES.[1]

IN the remarks which at our last anniversary I had the honour of offering from this chair, I congratulated the students of geology and mineralogy upon the new and intimate relations which, to their mutual advantage, are now growing up between those departments of science. It has, however, been suggested that, while geologists are thus being brought into closer alliance with mineralogists, the strong bonds of union which have so long united us with the biologists are becoming somewhat relaxed, and, indeed, stand in no small danger of actual dissolution.

Highly as I estimate the value of the *rapprochement* between the geological and mineralogical sciences, I for one should regard such a result as far too dearly purchased, if it necessarily involved any interruption of the close relations which have so long subsisted between geology and biology. But I cannot for one moment believe that such a grievous misfortune seriously threatens the cultivators of the two great departments of natural science.

Notwithstanding certain divergencies of opinion which have made themselves heard within an ancient University, and have awakened a faint echo in the halls of our National Museum, I cannot doubt that the teachers of geology and biology will easily discover a *modus vivendi* upon what is, after all, a subject of very secondary importance—the arrangement of natural-history collections.

No one can read recent declarations of the present Director of our National Museum without being impressed by his manifest desire to make the splendid collections under his care reflect, as completely as possible, the present condition of our knowledge of natural history. And if, on the other hand, we turn to the remarks made by the Keeper of the Zoological Department, at Swansea, in 1880, and to those of the Keeper of the Palæontological Department, at Manchester, last year, we shall find in those utterances ample guarantees that, in the arrangement of their collections, questions of practical convenience will not be lost sight of; we shall be satisfied that there is not the smallest danger of revolutionary ideas leading to the removal of "ancient landmarks," or of unattainable ideals being sought through the wholesale commingling of incongruous elements. The collections of our Universities are happily free from the conditions which must always hamper an institution where the interests of popular amusement have to be reconciled with those of scientific work; and it is for the teachers of natural science in those centres of thought to agree upon an arrangement which may best serve to illustrate their courses of instruction.

But while the discussion on museum-arrangement may be regarded as a purely academical one, which, after scintillating for a while in letters and pamphlets, died out in some not very formidable explosions at the recent meeting of the British Association, it may be wise on our part not to pass by quite unnoticed some indications of the attitude of the younger school of biologists towards palæontological science, this attitude having been very conspicuously manifested during the discussion in question.

If I rightly apprehend the views of some of my biological friends, as gathered not only from their published utterances, but also from private conversations, the position they are inclined to take up may be expressed somewhat as follows:—

"Palæontology has no right whatever to separate existence as a distinct branch of science. Fossils are simply portions of animals and plants, and ought to be dealt with as such; for all scientific purposes it is quite immaterial whether the organism which we are called upon to study expired only an hour since or died millions of years ago. Imperfect fragments can only be properly interpreted in the light afforded by the more complete structures found in recent organisms; and hence the naturalist who is engaged in studying a particular group of living organisms is the only person competent to deal with its fossil representatives. In our laboratories and our museums alike, therefore, fossil remains ought to be studied side by side with the living types which most nearly resemble them, and always by the same investigators. This being the case, it is neither necessary nor expedient that there should be a class of students whose chief concern is with extinct forms of life; and as for the geologists, they have really no farther concern with fossils than just to find them, attach a label indicating the period at which they must have lived, and hand them over to the biologist for study and incorporation in his collections. Any action beyond this can only be regarded, indeed, as an act of usurpation on the part of geologists, and must tend, not to the advancement, but to the injury of true science."

Such, so far as I have been able to gather them, are the extreme opinions which some biologists now entertain. It may, perhaps, seem presumptuous on my part to venture to offer a plea for palæontology, but there are considerations which may induce us to regard such a plea as coming better from one whose place in the ranks of the geological army lies nearer the centre than in the biological wing; from one who regards palæontology as the borderland of the geological and biological sciences—a borderland where the cultivators of both ought ever to meet, not for rivalry and aggression, but for the necessities of intellectual commerce and the advantages of mutual help.

The view of palæontology which I have ascribed, I believe not unjustly, to some biologists is one which has just such an amount of truth in it as to render it plausible, but at the same time, as I cannot but believe, is one of those half-truths which are proverbially more dangerous than downright errors. Palæontology is not, as has often been confidently asserted, simply a branch of biology; it is equally a part of geological science, and there are the strongest grounds, both of reason and expediency, for retaining it in that position. All geological science is based on the principle that the past can only be interpreted by the study of the present; Darwin was the intellectual child of Lyell, and the "Origin of Species" was the logical outcome of the "Principles of Geology." No palæontologist, worthy of the name, has ever dreamed of studying fossils except in the light afforded by the investigation of their recent analogues. Indeed, if we were to carry out the aggressive ideas of some biologists to their legitimate consequences, there would be left to us no science of geology at all; for why, it may be asked, should the study of physical processes in the past be carried on separately from the investigation of the same processes as exhibited at the present time? But then, by a strange Nemesis, I fear the same all-devouring physics, after swallowing up geology, would make very short work indeed with biology itself. And there is still in the background another claimant for universal empire in the realms of thought, for are there not some who dream of all sciences ultimately becoming the victims of that new portent of ambition—"geography"?

In considering the present position and future claims of palæontology, I may be permitted at the outset to offer a protest against a class of objections which has sometimes been very unfairly urged against the votaries of that branch of science. It has often been assumed that the students of fossils are contented with a lower standard of excellence than that which is aspired to by the cultivators of other branches of natural history. Now, setting aside for a moment the very important consideration that, owing to the imperfection of the remains which they are called upon to study, palæontologists are confronted by difficulties which do not beset the investigators of recent forms, I maintain that the charge is an altogether unjust one. Palæontologists are no more responsible for the unwise use made of fossils by incompetent persons than are zoologists for the vagaries of shell- and butterfly-hunters, or botanists for the absurdities of fern- and diatom-collectors.

Doubtless there has been much work done in connection with

[1] Address to the Geological Society by the President, Prof. John W. Judd, F.R.S., at the Anniversary Meeting, on February 17.

fossils, as well as with other natural history objects, of which we can only speak with shame and regret as having been undertaken unadvisedly and performed ignorantly,—work which, prompted by an unwise ambition, has been conceived in error and brought forth in presumption.

It would ill become anyone from this chair to speak lightly of the great, the inestimable services rendered to our science by the collectors of fossils. How many interesting and novel forms have been brought to light by their patient efforts! How often has the structure of ob cure types been rendered clear through their constant and persevering endeavours to obtain more perfect specimens! Yet sometimes the very zeal of collectors has led them astray. Despairing of finding systematic zoologists and botanists who could devote the necessary time and attention to the study of objects which they have obtained with so much trouble and pains, they have unwisely undertaken, without the necessary training and knowledge, the naming and description of forms of life which required for their proper interpretation all the skill and experience of the most able comparative anatomist or vegetable morphologist.

I feel sure that, if those who have thus erred, through acting with "a zeal which is not according to knowledge," could realize the injury done to science by such proceedings, they would pause before burdening scientific literature with premature names, imperfect diagnoses, and ill-digested materials. Fossils are, it is true, "the medals of creation," and for the purposes of the historian of past geological times, it may seem that any name, however bad, which can be employed for purposes of reference must be better than none at all. But fossils, it must be remembered, are much more than mere "medals." They are the precious relics of the faunas and floras of bygone times; landmarks—the only ones we can ever hope to discover—which may serve to guide us in tracing the wonderful story of the evolution of the existing forms of life. Reverently—as the mineralogist treats meteorites, those pocket-planets and errant members of the outer universe—should the biologist regard fossils, the fragments of an earlier life, the collateral, if not the direct, ancestors of living types.

So far I am from thinking that the study of fossils ought in all cases to be undertaken by those who are actually engaged in working out their recent representatives, that I believe such a practical abolition of palæontology as a distinct branch of science would tend, not to the advantage, but to the injury, of both biology and geology. And I will venture to set forth my grounds for this conclusion.

It may be remarked at the outset that at a time when all the tendencies of biological science appear to be towards an extreme specialization, it is strange to find that there are advocates for the suppression of what is now so well-developed a department of biological science as palæontology. When the work to be done has become so vast that some biologists feel themselves compelled to restrict their studies and labours to the morphological, or even to the histological department, others to the embryological, the physiological, the taxonomic, or the chorological branches of zoology or botany respectively, why should not some concentrate their efforts upon the elucidation of the ancient forms of life? When the study of a single group, often a very limited group, of animals or plants is sufficient to exhaust the energies of a particular naturalist, it is surely not unreasonable that forms which have become extinct and have left only very imperfect evidence of their structure and affinities, and these requiring peculiar methods for their study, should attract the attention of special investigators.

The study of fossils, we may remark, if it be undertaken by any biologists, must fall to systematic zoologists and botanists, and these have become somewhat rare and out of fashion in modern times; so few in numbers, indeed, do they seem as to be scarcely able to cope with the ever-increasing array of living forms; and it would be a hopeless task if upon them were also cast the overwhelming mass of fossil ones.

Imagine the embarrassment and dismay of a student of living sponges, whose favourite (possibly his only) method of research has consisted in studying with the microscope innumerable thin slices cut from tissues and embryos, if a cartload of chalk-flinks were thrown down at his door, and he were required to interpret the fragments of sponge-skeletons which they contained in every conceivable variety of disguise through peculiar processes of mineralization!

There are, indeed, a variety of special reasons why ordinary systematic zoologists and botanists become, by the very habits acquired in their daily pursuits, singularly ill fitted for dealing with fossil forms.

In studying recent forms the zoologist or botanist is bound to take into consideration, in fixing the systematic position of an organism, not only its skeleton, but all its soft parts, and even the structure and mode of development of its embryo; he may also be called upon to note physiological peculiarities, before he is in a position to arrive at a decision as to its place in the zoological or botanical series. But for the student of fossil forms none of these aids are available, he is compelled to do his best without them. Investigators of the recent Mollusca are, of course, "malacologists," but he who studies the extinct forms of the group must perforce labour under the stigma of being "a mere conchologist." In examining recent vertebrates it is allowable to make every possible use of the aid afforded by a study of the ligamental skeleton, in unravelling their affinities; but he who works on fossil vertebrates is and must remain a pure osteologist. Botanists have been led to the conclusion that for the classification of plants the reproductive organs always afford the safest guides; but palæontologists, alas! are frequently called upon to do their best in deciphering fragmentary remains of the vegetative organs.

It is not, as some biologists would almost seem to imagine, that palæontologists are led by any perversity of mind to reject the light which is afforded to them, or that they are not deeply sensible of the great value and importance of many recent researches in respect to living forms; but simply that they realize—often very sadly realize—the impossibility of availing themselves of the help afforded by such researches, in connection with the very imperfect material with which they are called upon to deal.

If we were to suppose that a surveying ship brought home from a newly-discovered island a heterogeneous mixture of isolated bones and teeth, of shells, bits of stick and fallen leaves, zoologists and botanists might be perfectly justified in refusing to waste their time upon such unsatisfactory materials. But if, subsequently, news arrived that after the departure of the ship the whole island had sunk beneath the ocean, then the circumstances would have completely changed, and no pains and care would be felt to be too great if expended in dealing with such a unique collection, however imperfect it might be. Or, to take a case which has actually occurred, the curators of the Ashmolean Museum were fully justified in ordering the destruction of the moth-eaten dodo skin, so long as they had no reason for doubting that other and better specimens were procurable; but now no labour and pains is considered too great in studying the most imperfect fragment of the bird.

And here I may perhaps be permitted to say a word in defence of what has been treated as an absurd practice on the part of palæontologists—that of giving names to small fragments of organisms. It must be admitted that when subsequent investigation proves that distinct generic and specific names have been given to the root, the stem, the outer and the inner bark, the pith, the foliage, and the fruit of the same plant, the absurdity does seem striking. But it is impossible to defer giving a name to a fossil until all doubts about its structure and affinities have been completely settled by the finding of exceptionally perfect specimens. Nevertheless, it ought certainly to be insisted on that names should be given to very fragmentary fossils only by a competent naturalist, and that he must accept the responsibility of his act. A single tooth of a mammal may afford good grounds for the establishment of a genus and species, while it might be utter folly to treat the tooth of a shark in the same manner.

The remains of many extinct forms are in such a peculiarly mineralized condition as to require special skill and training for their proper interpretation. Skeletal elements which were originally siliceous are now represented by pseudomorphs in calcite, and *vice versâ*. Characteristic structures in bones, shells, or wood may be wholly obliterated, and mineral structures of a strangely deceptive kind may be developed in their place. The curious story of *Eozoon canadense* and its supposed allies is surely a sufficient justification for the existence of palæontologists—that is, of specialists trained equally in the interpretation of biological and petrological structures. Dr. Sorby has shown that whole families of Mollusca may disappear from a fauna because of the unstable condition of the calcic carbonate which composes their shells, and his conclusions have been confirmed by Mr. Kendall.

Prof. Sollas has similarly shown that the absence of the por-

cellanous types of the Foraminifera from the Palæozoic rocks may be due, not to their non-existence when those rocks were formed, but to the fact of their shells being composed of the unstable aragonite.

Such facts as these must convince any unprejudiced person of the absolute necessity, to the naturalist who attempts to study extinct forms, of an acquaintance with the nature of the mineral changes which organic remains undergo. In his interesting memoir upon those curious and enigmatical fossils, the Receptaculitidæ, Dr. Hinde has admirably shown the advantages of this combination of biological and petrographical study.

In this connection I cannot avoid alluding to a very prevalent and, as I cannot help thinking, very erroneous notion, that an intermingled zoological and palæontological collection, however inconvenient, would certainly be very instructive. To this view I offer the strongest protest, for I believe that the mistakes which would arise from the examination of such a collection would far outweigh any instruction to be derived from it.

I fail to see what useful lesson would be taught by swamping a collection of the lizards, snakes, tortoises, and crocodiles living at the present day with the vast slabs containing the relics of Reptilia which have existed in periods ranging from the Permian to the Pliocene. Nor is it apparent to me why the precious remains of *Archæopteryx* should be hidden away among a wilderness of bird-skins.

Any arrangement which could lead to the idea that even the richest collection of fossils is in any way commensurable with the assemblages of specimens that in our museums represent the existing fauna is very greatly to be deprecated. So numerous are the gaps among fossil faunas, owing to the fact that only animals with hard parts, and, as a rule, only those that lived in the sea, had any chance of preservation, that the finest palæontological collections are, and must always remain, extremely fragmentary. We have, in the past, fallen into so many and such grievous errors, by ignoring the imperfection of the geological record, that we may well hesitate before doing anything that would confirm this mischievous delusion.

On the other hand, it may be pointed out that our acquaintance with extinct forms of life has increased to such an extent in recent years that a biologist may well be pardoned for not realizing the vastness and importance of the problems involved in the study of fossils. It can only be a very inadequate idea of the value of palæontological evidence which leads fossils to be regarded (like the fauna and flora of a newly-discovered territory) as simply supplying a few missing links required to fill up gaps in a natural-history classification, or as the appropriate ballast for a Noah's Ark on a scale of national grandeur. Small as may be the whole bulk of a palæontological collection in the eye of the student of recent forms, its great and transcendent value depends on the fact that the objects composing it belong to the faunas and floras of periods widely separated from the present and from one another. The discovery of a new type of reptiles in the Trias is a very different matter from the detection of an equally remarkable form living in New Zealand. The latter may, it is true, be a singular survival of some old type; but the former is an actual landmark in the course of reptilian development; and by the study of the fossil we are actually brought much nearer to the solution of the problems connected with the history of that development than is possible by the study of any recent form.

In pointing out how vast has been the progress of our knowledge in recent years concerning the ancient life of the globe, I may remind you of the estimates made by Prof. Huxley when speaking from this chair a little more than a quarter of a century ago. He then characterized "the positive change in passing from the recent to the ancient animal world" as "singularly small"; and he regarded the extinct orders of animals as not amounting "on the most liberal estimate" to more than one-tenth of the whole number known. The evidence which has been accumulated during the last twenty-five years, however, has modified this estimate in a remarkable manner, as no one would be more ready to admit than the author of it himself.

There is no little difficulty in making a calculation of the proportion of living to extinct orders, owing to the discrepancies in the opinions of zoologists and comparative anatomists as to what are the characters which ought to be considered as of ordinal rank. For my present purpose I very gladly avail myself of the useful "Synopsis of the Animal Kingdom" prepared by Mr. E. T. Newton, which is "founded on the classification proposed by Prof. Huxley, with such modifications as are rendered necessary by recent discoveries."

We may, I think, take the whole number of living orders of animals generally accepted by zoologists at about 108. But in any comparison of these with fossil forms, it is only fair to exclude from our consideration such as possess no hard parts and stand little or no chance of being preserved in a fossil state. Few would be bold enough to doubt that such soft-bodied forms must have existed in the past, or that they probably bore about the same proportion to the forms with hard skeletons as in the existing fauna; even the boldest sceptic on this subject would, I should think, be convinced by such singular accidents as that of the finding of the impression of *Rhizostomites*, one of the Discophoræ, preserved in the soft calcareous mud of the Solenhofen Slate.

Now among the 108 living orders of animals, at least 36 are totally destitute of any hard parts capable of being preserved in a fossil state, and we have thus left 72 living orders with which our comparison of the extinct orders must be made.

What is the number of orders which must be created to receive extinct forms, is a question that has given rise to wide diversities of opinion in recent years. While few naturalists would consider 18 as an excessive estimate, there are others who would probably double that number.

Taking the lower estimate and comparing the 18 extinct orders with the 72 living ones which contain animals with hard parts, we find the proportion of extinct orders to be 20 per cent. of the whole number known at the present time.

But in comparisons of this kind, it must be remembered that there is an unconscious tendency among the students of recent forms of life to *under-estimate* the differences between extinct and living forms. If we take such groups as the *Graptolitidæ*, the *Monticuliporidæ*, and the *Stromatoporidæ*, of the nature of the polyps of which we can know nothing, we can only place them in existing orders on the ground of some very general analogies in the skeleton. How little this may be worth, recent zoological researches, like those of Prof. Moseley on the *Milleporidæ* and the *Stylasteridæ* have amply shown.

The students of existing forms of life have arranged their pigeon-holes; and into those pigeon-holes our unfortunate fossils are too often *made* to go. If there were no other objection to the wholesale commingling of recent and fossil types in a museum, there would be the valid and insuperable one arising from the fact that there are very considerable and important groups of fossils which cannot, without violence, be made to find any place in our accepted classification of existing animals—and perhaps never will.

If, however, we consider the modifications which have been brought about in our views concerning the relations of extinct to living forms by the important discoveries that have been made since 1862, we shall be impressed by the conviction that no comparison of the numbers of living and extinct orders can give any adequate idea of the important influence of palæontological studies upon biological thought. The discovery of transitorial forms, like the *Archæopteryx*, the toothed birds of America, and the reptiles with avian affinities, together with the working out of the rich faunas of the Rocky Mountains, of Pikermi, Quercy, and the Siwaliks, of the Pampean formations of South America, the Karoo beds of South Africa, and the caves of Australia, have already done much towards revolutionizing the ideas held twenty-five years ago by biologists concerning the significance and value of fossil forms. While the recognition of the less specialized precursors of such types as the horse and the elephant have perhaps produced most effect in removing objections to evolutionary doctrines, the light thrown by the study of fossil forms on the manner in which individual structures have arisen, as has been so well shown by Prof. Alexander Agassiz, in the case of the Echinodermata, opens up to us a wide and perhaps far more hopeful field of inquiry. We are, however, only at the beginning of the great task of utilizing the grand palæontological collections of mammals, of reptiles, of fishes, and of the various groups of the invertebrates, for explaining the significance and tracing the origin of the structures found in living types.

While maintaining that studies of this kind demand and justify the concentration of the labours of a special class of investigators, I feel sure that no one will misinterpret my meaning as to the qualifications required by the students of fossil forms. Far from suggesting that the palæontologist may be one destitute of a proper biological training, or that he may be satisfied with an equipment of knowledge which would be insufficient for a systematic zoologist or botanist, I would maintain that no one

has a right to take up the study and description of any fossil group until he has made a very careful and exhaustive study of its nearest living allies; but, in addition to this, he ought also to have made himself acquainted with the peculiar mineral changes which organic remains are liable to undergo. He will, moreover, be far more likely to interpret aright and to make the best use of the materials that come to his hand, if he have at least a general knowledge of what others working on similar materials belonging to other departments of the animal or vegetable world have been able to accomplish, and of the methods which they have followed. Such palæontologists, I insist, have as much right to recognition as any other class of biological specialists.

Still less should I wish it to be implied that I think systematic biologists can afford to be ignorant of the results of palæontological studies, in their own particular fields of labour. One of the most mischievous weeds that have accompanied the evolutionist in his incursions into various parts of the biological field is the preposterous "genealogical tree." We can scarcely turn over the leaves of a modern systematic work without finding it flourishing in full luxuriance. No sooner has the student of a particular group arranged his families, genera, and species, than he thinks it incumbent upon him to show their genetic relations. Very admirably has Prof. Alexander Agassiz pointed out the utter fatuity of such a proceeding. As Lyell used to say, in speaking of such proceedings, the imagination of the systematist, untrammelled by an acquaintance with the past history of the group, "revels with all the freedom characteristic of motion *in vacuo*." If for no other reason, zoologists and botanists ought to study fossil forms in order that, by encountering a few hard facts in the shape of fossils, they may be saved from these unprofitable flights of the imagination.

(*To be continued.*)

SCIENTIFIC SERIALS.

Rendiconti del Reale Istituto Lombardo, December 1887.—On the Tertiary formations near Cape La Mortola, in Liguria, North Italy, by Prof. T. Taramelli. The paper deals specially with the abrupt interruption which occurs in the prevailing Eocene and Secondary systems about this part of the Ligurian coast. This interruption is brought into connection with the great development in Liguria of the marine Pliocene formation, which in the Varo basin and near Ventimiglia stands at a present altitude of over 550 metres above the sea, but which does not occur at all further east in Istria and Friuli, where it is represented by thick alluvial deposits of vast extent.—On the neutralizers of tubercular virus, by Prof. Giuseppe Sormani. In continuation of his previous studies, the author here deals with twenty-one additional substances, or chemical reagents, making eighty altogether. According to their different action on Koch's Bacillus these are grouped in three categories: those that have no effect; those that only attenuate, and those that entirely destroy, the virus. As many as twenty-two, including camphorated chloral, the bromide of ethyl, and the nitrite of ethyl, are found to be effective.—Meteorological observations made at the Brera Observatory during the month of November 1887.

Rivista Scientifico-Industriale, January 15.—The crepuscular tints in connection with the hygrometric state of the atmosphere, by Prof. Costantino Rovelli. Constant observation shows that red and orange tints prevail in a dry, yellow and green in a moist, state of the atmosphere. This suggests a threefold division of the solar spectrum into (1) the region of warm rays transmitted by the lower atmospheric strata, and corresponding to a dry condition of the air; (2) the region of middle rays, yellow and green, more easily diffused and partly transmitted by the air in moist weather; (3) the region of cold rays diffused by an atmosphere abounding in aëriform vapour. The terrestrial dust suspended in the air, by condensing the aqueous vapour, as is now generally accepted, may also tend to produce those occasional after-glows of intense brightness, which have been so often observed after violent volcanic eruptions. The various character and intensity of the tints may all be thus explained by the theory of the eclectic transmission of the coloured rays by the corresponding states of the atmosphere, and partly also by the particles of dust held in suspense.—On the constitution of fogs and clouds, by Prof. F. Palagi. These phenomena are attributed to the presence of minute drops of water with diameter of 1/10 to 1/20 mm. at a temperature above zero. The recent observations made by the author on Mount Titano show that when the temperature falls below zero these globules are converted into minute hexagonal needles and flakes of the same form, the former about 1/20 mm. thick, and from two to ten times longer, the latter from 1/10 to 1/4 mm. in diameter. In their passage from the higher regions through the lower and less cold strata, but still below zero, these simple crystalline forms appear to be transformed by the process of condensation and agglomeration into the stars and flakes of ordinary snow. But when the temperature rises above zero they are again changed to the minute liquid drops of clouds, fog, and rain according to the varying degrees of altitude and temperature.

Bulletin de l'Académie des Sciences de St. Pétersbourg, vol. xxxii. No. 1.—On the effects of the earthquake of February 23, 1887, at the Observatory of Pavlovsk, by Dr. Wild (in German). The effects of the catastrophe having been observed at the Observatories of England, France, Italy, Germany, and Austria, in trepidations of the magnetic instruments, it was interesting to see whether the earthquake was felt as far as St. Petersburg. The results indicate that it was not.—On the genus Hemiculter and a new species of Hemiculterella, by N. Warpachowski (in German).—Russian words used in the Sagai dialect, and their phonetic modifications, by N. Katunoff; and lists of Sagai names of rivers, villages, and tribes, by the same. This little dictionary is highly spoken of by M. Radlof.—Studies, by O. Backlund, about the Pulkowa catalogue of stars, "Positions moyennes de 3542 étoiles," published in 1886 (in German). A detailed comparison of the Pulkowa catalogue with the measurements by Herr Romberg at Pulkowa, as also with the catalogues of Becker, Respighi, and Boss.—Hydrological researches, by Dr. Carl Schmidt.—The temperature-maxima before midday in tropical seas, according to the observations of the corvette *Vityaz*, by M. Rykatcheff (in German). They show the existence of two separate maxima, one of which sets in half an hour before midday and the other half an hour later. More extensive observations are needed.—On the synthesis of albumen in chlorophyll-bearing plants, by Chrapowitzki (in German). The chlorophyll spots must be considered as places where synthesis of both carbohydrates and albumen is going on.—New additions to the Asiatic Museum, by C. Salemann. Summaries of two Persian and three Kagatai manuscripts brought in by M. Pantusoff from the Semiryetchensk province.

THE *Izvestia* of the Russian Geographical Society (1887, iv.), contains most valuable papers and maps. Dr. Junker contributes a report on his seven years' journeys in Equatorial Africa, and his paper is accompanied by a map, 53 miles to the inch, of the region extending for ten degrees on the north of the Equator, between the 22nd and 33rd degrees of longitude. Two papers, by M. Potanin, contain a summary of the information gathered from the natives as to Eastern Tibet (the regions of Amdo and Kam), and the region of Central Mongolia situated between the Nan-shan, the Khangai, Hami, and the Utai-shan. Both papers are accompanied by maps, on a scale of 100 miles to an inch, and the two maps complement one another, so as to give a very accurate idea of the upper Hoang-ho. Of the other papers, one by M. Krasnoff, on the manners of life of the Kirghizes in the Semiryetchensk province, will be welcome to ethnographers. The same number contains also a list of fifteen places in Laponia, the latitudes and longitudes of which have been measured in 1864 by Captain Ernefeld; and, in a separate appendix, tables, by Prof. Sharnhorst, for the calculation of heights from barometrical observations. It is self-evident, although it is too often lost of sight, that the calculation of heights upon observations of the barometer, when it is made by means of logarithms, means a much greater accuracy of results than anything that can be obtained from a few observations of atmospheric pressure during a journey, and that some plainer tables would give the results with an accuracy quite sufficient for the accuracy of the data themselves. M. Sharnhorst's tables are an improvement upon those formerly in use, and ought to be introduced into every manual for travellers, instead of the usual logarithmical tables.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 26.—"Report on Hygrometric Methods. First Part, including the Saturation Method and the Chemical Method, and Dew-point Instruments." By W. N.

Shaw, M.A. Communicated by R. H. Scott, F.R.S., Secretary to the Meteorological Council.

With the exception of certain "absolute hygrometers," the behaviour of which has not yet been sufficiently tested, the determination of the pressure of water-vapour in the air is indirect, and requires a formula of reduction. The formulæ in use are based upon assumptions which are at present not so completely verified by experiment that any hygrometric method can be relied upon to give measures of the pressure of aqueous vapour trustworthy to within 0·1 mm. of mercury. The authority for these statements is given in detail in an account of the hygrometric work done since 1830, appended to the report as Note A.

In the report, the chemical hygrometric method is provisionally regarded as a standard.

The assumptions upon which the formula of the method is based are (1) that it is possible to absorb the whole of the moisture from air by passing it over desiccating substances; and (2) that a numerical value can be assigned to *d*, the specific gravity of aqueous vapour referred to air at the same temperature and pressure. The first assumption is sufficiently nearly accurate for hygrometric observations. With regard to the second, Regnault's direct observations upon steam (free from air) and other evidence point to the value 0·622. The assumption can, moreover, be tested, by applying the chemical method to air saturated at a known temperature, assuming the value 0·622 for *d*, and comparing the results with the table of saturation pressures *in vacuo*. This, however, *assumes Dalton's law to be strictly accurate*, an open question upon which opinion is reserved until further experimental investigation is concluded. Regnault found that the value 0·622 gave results for saturated air which were less than the tabulated pressures, the errors being always of the same sign, but so small in amount that he neglected them in his subsequent work.

The ultimate object of the experiments described in the report was to examine the behaviour of dew-point instruments in air of known state, and for this purpose air was saturated at a known temperature and drawn by an aspirator through vessels in which the dew-point instruments could be placed when required, and subsequently through drying tubes of special pattern. The vapour-pressure was thus obtained at the two extremities of the train of apparatus and the results compared.

The following questions are raised and discussed:—

(1) Were the drying tubes used as efficient as Regnault's? (2) Does the pressure of vapour in the air become changed by passing through the apparatus designed to contain the dew-point instruments, or by the mere presence of those instruments themselves? (3) Do the results of the chemical method agree with the tabulated vapour-pressures *in vacuo* when the air is more or less heated after being saturated? (4) Can the observed differences between the results be obviated by assuming a value for *d* (other than 0·622), which is compatible with values obtained by other methods? (5) Can any reason be assigned for the differences observed by Regnault in the case of saturated air?

(1) The answer to the first question is given in an account of a series of twelve experiments practically repeating Regnault's observations with saturated air. The tabulated results show divergences in the same direction and of the same order of magnitude as those in Regnault's paper. Some incidental points are also discussed—namely, the comparative efficiency of phosphoric anhydride, sulphuric acid, and calcium chloride, and the effect of india-rubber and glass connections between drying tubes. It is shown that the sulphuric acid and phosphoric anhydride tubes are efficient, that as a rule one tube is all that is strictly necessary, but that two should be used to provide for the case of exhaustion of the first tube or too rapid flow of air, and further, that the glass-and-mercury connections between the tubes employed in the second series of experiments cannot be regarded as producing any effect.

(2 and 3) The answers to the second and third questions are furnished by the results of eighty-two experiments with the chemical method upon air saturated at known temperatures by a specially designed "saturater" in a water-bath. The temperatures of saturation lay between 1° C. and 21° C., and, with one exception, were below the temperature of the surrounding air. Each experiment involves upwards of thirty readings of weight, pressure, and temperature. The temperature readings are corrected by means of a special comparison at Kew. Of the eighty-two observations thirty-two are retained as being free from any known disturbing causes, and from them it appears that, with *d* equal to 0·622, the pressure deduced by the chemical method is on the average greater by 0·03 mm. than that given in Regnault's table of vacuum pressures, as recalculated in Landolt and Börnstein's tables. This difference is very small compared with the discrepancies from Dalton's Law observed by Regnault in the case of water vapour.

(4) With regard to the fourth question; if the observations be employed to determine the value which must be substituted for *d*, the value obtained is 0·6245, which agrees very closely with 0·6240, the mean value for the same range of temperature deduced from Clausius's calculations based on thermo-dynamical reasoning. The value 0·622 is probably correct if the air is not nearly saturated; in that case the measure of the pressure of vapour in the air is 2/622 greater than it would be if the same air were reduced in temperature (at constant pressure) until it was saturated.

(5) The one observation of the second series with saturated air gives a result 0·18 mm. smaller than the tabulated pressure, and thus with the twelve experiments of the first series confirms the results of Regnault's observations. To account for this it is suggested that air which is very nearly or quite saturated would deposit some of its moisture on the glass tubes used to conduct it from one vessel to another. This behaviour of nearly saturated air has been already noticed, and it is confirmed by the observations on dew-point instruments, and moreover, by experiments, directly designed for the purpose, quoted in a note.

Details are given of observations with Regnault's hygrometer and Dines's hygrometer when exposed in glass vessels between the saturater and the drying tubes. The two instruments are separately discussed. With Regnault's instrument, after some practice, two different observers obtained practically identical results. In ordinary observations, the observed temperatures of the dew-point were below the temperature of saturation, but seldom by more than 0°·1 C. A considerable amount of uncertainty was shown to be attached to the readings, and by very close inspection readings of the dew-point were obtained above the temperature of saturation, in one case by as much as 0°·7 C.

From the experiments with Dines's hygrometer, it appears that the instrument is likely to give very easy determinations of the dew-point that are within small limits of error; but that, if it be observed with the closest attention, the result will be considerably too high in consequence of the formation of a dew deposit at a temperature above the dew-point, and it may possibly be erroneous in consequence of variations in temperature of the different parts of the box containing the thermometer.

An account is given of Alluard's modification of Regnault's hygrometer, and of Bogen's hygrometer.

A second note, B, is appended to the report, showing the tables used in various countries for the reduction of wet and dry bulb observations.

Chemical Society, February 2.—Mr. W. Crookes, F.R.S., in the chair.—Profs. Geuther, Ladenburg, Landolt, Nilson, Van't Hoff, Wislicenus and M. Lecoq de Boisbaudran were elected foreign members of the Society.—The following lecture was delivered:—The range of molecular forces, by Prof. A. W. Rücker, F.R.S. In discussing the range of molecular forces it is convenient to adhere to the language of the theory of action at a distance, though with full expectation that it will ultimately be replaced by another, such as the vortex-atom theory of Sir W. Thomson, or the granular theory of Prof. Osborne Reynolds, which involves only action in proximity. If we do this, however, it must be admitted that the law of molecular action may be very complicated. It may be granted that we naturally look for simplicity in our fundamental assumptions, but it is certain that we have *a priori* no more right to expect simplicity in the results of the action of a medium than simplicity in its constitution, and that the two are not necessarily obtained together. The largest values of the magnitude of the radius of molecular action which have been published have been deduced from observations on the condensation of gases and vapours on the surfaces of solids. Estimates on this basis made by Müller-Erzbach (*Exner's Rep.*, 1885, xxi. 409) and Kayser (*Wied. Ann.*, 1881, xiv. 450) have ranged between 1500 and 3000 micromillimetres[1] ($\mu\mu$). Such observations are open to many

[1] The micromillimetre is the millionth of a millimetre.

objections. Bunsen (*Wied.*, 1885, xxiv. 335) has shown that CO_2 will not condense on glass unless a film of water be previously formed. Warburg and Ihmori (*Wied.*, 1886, xxvii. 481, and *Wied.*, 1887, xxxi. 1006) adduce reasons for believing that the water film is largely due to uncombined or loosely combined alkalies on the surface. On clean unvarnished metals, washed glass and quartz, the thickness of the water film which can be removed by dry air without heating does not exceed 12 $\mu\mu$. A striking exception is agate, on which films 1640 $\mu\mu$ thick are stated to have been formed. As this substance, however, is composed of alternate layers of quartz and a porous impure opal, the basis for an accurate calculation does not exist. On the whole, it seems that no definite conclusions as to the magnitude of the radius of molecular action (ρ) can at present be drawn from these experiments. Quincke (*Pogg. Ann.*, 1869, cxxxvii. 402), as is well known, by measuring the capillary elevation of liquids between glass plates coated with thin wedge-shaped films, found $\rho = 50\ \mu\mu$. Plateau ("Statique des Liquides," 1873, i. 210) showed that the surface-tension of a soap-bubble, which thinned until its thickness was 118 $\mu\mu$, was unaltered. He concluded that $\rho < 59\ \mu\mu$. Maxwell ("Ency. Brit.," 9th ed., Art. "Capillary Action"), however, though by a confessedly imperfect theory, shows that the surface-tension may not change until the thickness of the film $= \rho$. Hence Plateau's result may mean only that $\rho < 118\ \mu\mu$. Reinold and Rücker (Phil. Trans., clxxvii. Part ii. 1886, 627) have proved that the surface-tension does not alter by 0·5 per cent, when the film is so thin as to show the black of the first order of Newton's colours. This appears at first sight at variance with Quincke's result, but their observations are really in remarkable accord with his. The black and coloured parts of a film are separated by a sharp line, which proves a discontinuity in the thickness (Proc. Roy. Soc., 1887, No. 182, 340). The colours, which correspond to certain thicknesses, which may be called the unstable range of thickness, are always missing. The black part of the film has been proved by Reinold and Rücker (Phil. Trans., Part ii. 1883, 645) to be of a uniform thickness, which differs but little from 12 $\mu\mu$. Sir William Thomson (Proc. Royal Institution) and these observers independently arrived at the conclusion that these curious phenomena are due to the fact that the surface-tension diminishes to a minimum, and then increases again when the thickness is somewhat $>12\ \mu\mu$. The colours of the film prove that the upper limit of the range of unstable thickness is between 96 and 45 $\mu\mu$. Quincke's result indicates that it lies between 100 $\mu\mu$ and 50 $\mu\mu$, according as we adopt Plateau's or Maxwell's views. These calculations are therefore in complete accord. Quincke's result is not an isolated fact, but is supported by observations on soap films. The statement that 50 $\mu\mu$ and the radius of molecular action are of the same order of magnitude may now perhaps rank as an ascertained fact. Another method of investigating the radius of molecular action is based on the phenomena of electrolytic polarization, by observing the change in the difference of potential between a metal and a liquid in which it is immersed, when a gas or metal is deposited on it by electrolysis. In the former case we do not know the density of the gas, in the latter Oberbeck (*Wied.*, 1887, xxxi. 337) concludes that a plate of platinum is completely polarized by a film of another metal of from 3 to 1 $\mu\mu$ in thickness. The method of experiment is, however, open to objections, which are indicated by Oberbeck himself. Measurements of the thickness of the double electric layer of Helmholtz, which is closely related to the distance between two consecutive layers of molecules, have been made by Lippmann (*Compt. rend.*, 1882, xcv. 687), and by Oberbeck and Falck (*Wied.*, 1884, xxi. 157). The values they give vary between 1 and 0·02 $\mu\mu$. Wiener (*Wied.*, 1887, xxxi. 624) has studied the alteration in the phase of light reflected from very thin silver plates deposited on mica. He finds that the effect begins to alter when the thickness is reduced to 12 $\mu\mu$, and that it was possible to detect a silver film the thickness of which did not exceed 0·2 $\mu\mu$. The diameter of a molecule is a conventional term for the mean distance of the centres of two molecules during an encounter. It may therefore be different in the liquid and gaseous states. Sir William Thomson ("Natural Philosophy," Thomson and Tait, Part ii. 295, 1883), as the result of his celebrated discussion of this point, concludes that the mean distances between the centres of molecules in liquids (supposed arranged uniformly) is between 0·07 and 0·02 $\mu\mu$, and that the latter quantity is an inferior limit to the diameter of a gaseous molecule. The diameters of molecules (d) may be calculated if we know the mean free path (L), and the so-called condensation coefficient (v), which is the volume of the molecules contained in a unit volume of the gas. Loschmidt (*Sitzungsber. Wien. Akad. Math. Classe*, lii. abt. 2) and O. Meyer ("Die Kinetische Theorie der Gase," 225, 1887) have calculated d on the assumption that the molecules in a liquid practically fill the whole space it occupies. Exner (*Rep. der Physik*, xxi. 226, 1885), using a formula given by Clausius, $v = (K - 1)/(K + 2)$, where K is the specific inductive capacity, and can be replaced by $v = (n^2 - 1)(n^2 + 2)$, where n is the refractive index, finds values of d about five times smaller. Three independent methods of calculating the diameter of a gaseous hydrogen molecule lead to results between 0·14 and 0·11 $\mu\mu$. The most reliable conclusions which have been reached as to molecular magnitudes may be summed up in the following table, which is reproduced from a diagram exhibited during the lecture.

$\mu\mu$.		
118	Superior limit to ρ	Plateau (Maxwell)
96–45	Range of unstable thickness begins	Reinold and Rücker
59	Superior limit to ρ	Plateau
50	Magnitude of ρ	Quincke
12	Range of unstable thickness ends	Reinold and Rücker
12	Action of silver plate on phase of reflected light alters ...	Wiener
10·5	Thickness of permanent water film on glass at 23° C. ...	Bunsen
4·3	Mean distance between centres of molecules in gases at 760 mm. and 0° C.	O. Meyer
3–1	Thickness of metal films which polarize platinum	Oberbeck
1–0·02	Thickness of electric double layer	Lippmann and Oberbeck
0·2	Smallest appreciable thickness of silver film	Wiener
0·14–0·11	Diameter of gaseous hydrogen molecule	Exner O. Meyer Van der Waals
0·07–0·02	Mean distance between centres of liquid molecules	W. Thomson
0·02	Inferior limit to diameter of gaseous molecule	W. Thomson

—The following papers were read:—A new method of obtaining monohydrazides of α-diketones, by Prof. F. R. Japp, F.R.S., and Dr. F. Klingemann. The authors have prepared von Pechmann's monohydrazide of diacetyl by the action of diazobenzene chloride on sodium methacetate.—The formation of dihydrazides of α-diketones, by the same.—The action of phenylhydrazine on anhydracetophenonebenzil, by Prof. F. R. Japp, F.R.S., and Mr. G. N. Huntly.—The supposed identity of rutin and quercitrin, by Dr. E. Schunck, F.R.S. A comparative examination of rutin obtained from the leaves of *Polygonum fagopyrum* and of quercitrin shows that, though they are extremely similar, yet they differ in composition and in some of their properties. Rutin has the composition $C_{42}H_{50}O_{25}$, and yields, on hydrolysis, one molecule quercetin and three molecules isodulcite, whilst quercitrin $C_{36}H_{38}O_{20}$, as is known, yields, under like conditions, one molecule quercetin and two molecules isodulcite.—The composition of bird-lime, by Dr. E. Divers, F.R.S., and M. Kawakita. Japanese bird-lime prepared from *Ilex integra* contains, in addition to 6 per cent. of caoutchouc and minute quantities of oxalates, the ethereal salts of palmitic acid, and, in small quantity, of a semi-solid undetermined fatty acid. On hydrolysis these yield *ilicylic alcohol*, $C_{22}H_{38}O$, differing only slightly from Personne's ilicic alcohol, and *mochylic alcohol* $C_{26}H_{46}O$. A resinoid body, $C_{26}H_{44}O$, was also separated. When heated with palmitic acid, the two alcohols are converted into compounds just like purified bird-lime. The authors consider bird-lime to be closely allied to the *waxes* in chemical constitution.

ERRATA.—P. 335, line 15 (from top), *for* $3SOH_2SO_4$ *read* $3H_2SO_4$; line 19 (from top), *for* SO *read* SO_2.

Physical Society, January 28.—Prof. W. G. Adams, F.R.S., Vice-President, in the chair.—In opening the proceedings the Chairman referred to the great loss which the Society had sustained by the death of Dr. Balfour Stewart, their late President, and said that his loss would be deeply felt by the whole scientific world.—The following papers were read:—On the effect of magnetization on the thermo-electrical properties of bismuth, by Mr. Herbert Tomlinson.—On the influence of magnetism and temperature on the electrical resistance of bismuth and its alloys with lead and tin, by M. Ed. von Aubel.—On a water-dropping influence machine, by Prof. S. P. Thompson.—On the price of the factor of safety in lightning-rods, by the same. It is here shown, upon certain assumptions, that the safety against fusion varies as total cost $\times \frac{f s}{\rho d l^2 k}$, where f = temperature of fusion of material above atmosphere, s = specific thermal capacity, ρ = specific electric resistance, d = density, k = cost in pence per lb., and l = length of the conductor. If the total cost and length are supposed to be given, the factor of safety $= \frac{f s}{\rho d k}$. Of the common metals iron has the greatest factor of safety, being more than four times that of copper. Such being the case, the author thinks it desirable that the Report of the Lightning-Rod Conference be reconsidered.—On the optical demonstration of electrical stress, by Prof. A. W. Rücker, F.R.S., and Mr. C. V. Boys. A number of lecture experiments were shown illustrating that electrical stress exists in the dielectric separating two charged bodies. The bodies were placed in carbon bisulphide, between two crossed Nicols, and on electrifying them by means of a Holtz machine, light passed through the analyzer. Two concentric cylinders gave a black cross on the screen similar to those seen on interposing a plate of some uniaxial crystal, and a model illustrating a charged Leyden jar was shown.

February 11.—Annual General Meeting.—Dr. J. H. Gladstone, F.R.S., Vice-President, in the chair.—The Chairman read the Report of the Council for the past year, and expressed regret at the losses the Society had sustained by the deaths of Dr. Stewart (their late President), Prof. Kirchhoff, Mr. Coutts Trotter, and Prof. Humpidge. The Council regret that no increase of members has taken place during the past year, and hope that the advantages offered by the Society may be more fully appreciated in future. Obituary notices of Dr. B. Stewart, Mr. Coutts Trotter, and Prof. Humpidge were then read. The Treasurer's Report shows that the financial condition of the Society is very satisfactory. On the motion of Mr. Lant Carpenter, seconded by Mr. Inwards, the Reports were adopted.—The following gentlemen were elected members of Council for the present year:—President: Prof. A. W. Reinold, F.R.S. Vice-Presidents: Dr. E. Atkinson, Prof. W. E. Ayrton, F.R.S., Mr. Shelford Bidwell, F.R.S. and Prof. H. McLeod, F.R.S. Secretaries: Mr. Walter Baily, and Prof. J. Perry, F.R.S. Treasurer: Prof. A. W. Rücker, F.R.S. Demonstrator and Librarian: Mr. C. V. Boys. Other members of Council: Hon. R. Abercromby, R. H. M. Bosanquet, M.A., W. H. Coffin, Conrad W. Cooke, Prof. F. Fuller, W. N. Shaw, A. Stroh, Prof. S. P. Thompson, H. Tomlinson, G. M. Whipple. On taking the chair the new President expressed his sincere thanks for the great honour the Society had conferred upon him. Prof. Fuller proposed a vote of thanks to the Lords of Committee of Council on Education for the use of the rooms and apparatus of the Normal School of Science, which was seconded by Mr. Shaw, and passed unanimously. A cordial vote of thanks to the Council and officers of the past year, moved by Dr. Blaikley, and seconded by Prof. Ramsay, was duly acknowledged by the President. A similar vote, proposed by Mr. Bosanquet, and seconded by Mr. Hadden, to the auditors for the past year, was passed unanimously.—The meeting was then resolved into an ordinary science meeting, at which the following papers were read:—On the limit of refraction in relation to temperature and chemical composition, by Mr. T. Pelham Dale.—Note on the use of the term "resistance" in the description of physical phenomena, by Mr. R. H. M. Bosanquet.

PARIS.

Academy of Sciences, February 13.—M. Janssen in the chair.—On an ancient process for rendering gems and vitrifications phosphorescent, by M. Berthelot. The treatise in which this process is described occurs in the collection of Greek alchemists transcribed in certain manuscripts of the thirteenth and fifteenth centuries (Bibliothèque Nationale, Nos. 2325 and 2327). It contains a series of purely technical receipts analogous to those of the Leyden papyrus, some apparently of great antiquity, explaining certain methods of "colouring artificial precious stones, emeralds, carbuncles, hyacinths, according to the book taken from the shrine of the Temple." Several Egyptian alchemists are mentioned, such as Agathodemon, the pseudo-Moses, Ostanes, and Democritus, and the text leaves no doubt as to the ancient practice of rendering certain gems phosphorescent in the dark by means of surface colouring prepared from substances which are still known to possess such properties.—On the properties of a new hydraulic machine intended for irrigation purposes, by M. A. de Caligny. For this apparatus, which has been for some time in use both at Aubois and on the canal between Mons and La Louvière, the author claims the advantages of great simplicity of structure as well as economy on the first outlay. It may also be utilized for replenishing cattle-troughs, and other secondary uses, at a minimum cost. It was awarded a gold medal at the Antwerp Universal Exhibition.—On the part played by the absorbing power of the soil in the formation of the natural carbonates of soda, by M. Paul de Mondesir. The paper deals with Berthollet's well-known theory regarding the formation of Egyptian natron, and shows that Berthollet's explanation is so far true that the marine salt really furnishes the soda, and carbonate of lime, the carbonic acid. But the reaction is neither direct nor continuous, and is produced in two distinct phases. In the first, the soil reacts on the marine salt, transforming it into chloride of calcium while yielding lime and absorbing soda. In the second, which can set in only after removal of the chloride of calcium, the bicarbonate of lime and the carbonic acid extract the soda from the ground, replacing it with lime. Berthollet's theory is thus left fundamentally intact, but so modified as to become universally applicable. In fact, the carbonate of soda is produced in all permeable calcareous soils in proportion to the quantity of marine salt contained in them.—Observations of the new planet 272, discovered on February 4, at the Observatory of Nice, by M. Charlois. The observations, including right ascension, polar distance, and the positions of comparison stars, extend over the period from February 4 to February 9. When discovered the planet was of 13·5 magnitude.—New observations on the variability of Saturn's rings, by M. E. L. Trouvelot. It is pointed out that the observations made during the last few years by Perkins, Offord, Davis, Stanley Williams, Stroobant, and others, all tend definitely to establish the proposition announced by the author in 1884, that, so far from being stable, the rings of Saturn are on the contrary essentially variable, and subject to constant fluctuations. The same truth is confirmed by the author's own observations made in 1886 and 1887 at the Observatory of Meudon, and here communicated to the Academy.—Theorems on Campbell's algebraic equations and quadratic functions, by Father Aug. Poulain. Newton, or rather Campbell, formulated a very simple rule for determining the existence of the imaginary roots in algebraic equations. The author here proposes a few theorems, by means of which the application of this law may be extended and the accompanying calculations much simplified.—On chemical equilibria, by M. P. Duhem. In a recent note M. H. Le Chatelier announced that the numerical laws of chemical equilibrium, as deduced from the two principles of thermodynamics, may be expressed in a simple way by means of M. Massieu's characteristic function H'. Here it is shown that this law may be thus formulated: The variation imposed on M. Massieu's function H' by a virtual isothermic modification of the system is equivalent to zero. It is further pointed out that the results obtained by M. Le Chatelier are practically identical with those arrived at by the author during a series of investigations spread over several years.—On the mineralizing action of the alkaline sulphides: reproduction of chrysoberyl, by MM. P. Hautefeuille and A. Perrey. During a protracted series of researches on the mineralizing action of the sulphides, the authors have succeeded in obtaining the crystallization of glucine, the separation of alumina and glucine, or inversely the reproduction of the aluminate of glucine, a combination which occurs in nature, and which is known by the name of cymophane (chrysoberyl). A simple process is described by means of which from a combination of glucine and alumina extracted from the emerald the

glucine may be obtained with a loss of not more than 10 per cent., and in such a state of purity that its equivalent has been found equal to 12·58.—Influence of various diets on the interchange of the gases in respiration, by MM. Hanriot and Ch. Richet. Continuing their researches on the respiratory function, the authors find that respiration increases with the increase of food, but only when this consists of the hydrates of carbon; that the interchange of the gases is but slightly affected by a nitrogenous and fatty diet; that feculent substances increase the absorption of oxygen and especially the production of CO_2; that the centesimal proportions of the absorbed oxygen or of the generated carbonic acid varies little during muscular repose; that the proportion of absorbed oxygen averages about 4·2 per cent., and of generated CO_2 about 3·4 per cent. The subject is illustrated by a diagram showing by a graphic process the influence of a nitrogenous and feculent diet on the respiratory functions generally.—Discovery of a worked flint and a mammoth's tusk at Vitry-en-Artois, by M. Ladrière. The position in which these remains were found seems to confirm the author's view that towards the close of the early Quaternary epoch (Mousterian age) *Elephas primigenius* and other large mammals, as well as man, were already spread over the west of Europe.

BERLIN.

Physical Society, January 20.—Prof. von Helmholtz, President, in the chair.—Prof. Oettingen spoke on the interference of electrical vibrations which is produced by the electrical oscillations discovered by Feddersen, during the spark discharge. The discharge oscillations of two Leyden batteries, differing in frequency and amplitude, were allowed to produce an interference in the path of a third spark, and this led to a constant succession of alternatingly increased and diminished intensities of this spark. The phenomenon was analyzed by means of a rotating mirror, which resolved it into its several phases, and the events taking place in each spark were recorded by instantaneous photography. The speaker exhibited a large number of these photographs, both as negatives and as positive reproductions, and explained them fully. In these experiments, as in those described at the previous meeting of the Society on the explosion of an electrolytic mixture of oxygen and hydrogen, Prof. Oettingen had succeeded in obtaining accurate results only when he had replaced the concave rotating mirror by a plane one, whose action he then thoroughly discussed.—Prof. Börnstein exhibited a preparation which he had recently obtained quite by chance, during one of his lectures. When lecturing on the diffusion of liquids, he was in the habit of using a Traube artificial cell. On placing a blue crystal of sulphate of copper in a solution of soluble glass, a precipitate is formed as a film on the surface of the salt, when it comes in contact with the soluble glass. The water from the solution then diffuses through the film, dissolving the salt and stretching the film until it is ruptured at some one point. When this occurs the solution of the sulphate of copper comes again into contact with the soluble glass, a new film is formed at the surface of contact, closing up the aperture, and the diffusion begins again. The film thus grows continually in a tubular form, until it finally permeates the whole solution. When recently repeating this lecture experiment, the speaker noticed that the film did not grow in the usual tubular way, but took the form of flattened parallel membranes which advanced through the solution at right angles to their length. He was at present unable to offer any explanation of this latter phenomenon.—Dr. Budde had recently submitted Clausius's fundamental law of electro-dynamics to a recalculation, while taking into account a large series of special conditions; among these he allowed for the motion of translation of the earth, and found that it had no influence on the validity of the law. At that time he had not calculated the influence of the earth's rotation; he had however, since then, repeated his former work, and gave an account of the results of his calculation, which showed that the rotatory motion of the earth had also no influence on the law. The same speaker finally drew attention to an error which occurs in all text-books, in connection with the determination of the potential of a system of points, and showed how illogical is the usual definition and of deducing of potential energy. Prof. von Helmholtz then directed attention to the fact that he was in the habit of determining potential energy in a different way, and that its derivation from a system of points is fraught with great difficulties.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Contributions to the Palæontology of Brazil: C. A. White (Washington).—Die Entstehung der Arten, 1 Thiel: Dr. G. H. T. Eimer (Fischer, Jena).—Key to Todhunter's Differential Calculus: H. St. J. Hunter (Macmillan).—Annals of the Astronomical Observatory of Harvard College, vol. xiii. Part 2, Zone Observations made with the Transit Wedge Photometer (Wilson, Cambridge, Mass.).—Calendar and General Directory of the Department of Science and Art for the year 1888 (Eyre and Spottiswoode).—Electrical Instrument Making for Amateurs: S. R. Bottone (Whittaker).—Practical Education: C. G. Leland (Whittaker).—Volapük, or Universal Language: A. Kirchhoff (Sonnenschein).—Geology: Chemical, Physical, and Stratigraphical, vol. ii.: J. Prestwich (Clarendon Press).—Observations made during 1883 at the U.S. Naval Observatory (Washington).—Die Prähistorischen Denkmäler der Provinz Westpreussen und der Angrenzenden Gebiete: Dr. A. Lissauer (Williams and Norgate).—The Shell Collector's Hand-book for the Field: J. W. Williams (Roper and Drowley).—My Telescope: A Quekett Club Man (Roper and Drowley).—Through the Yang-tse Gorges: A. J. Little (Low).—Report on the Administration of the Meteorological Department of the Government of India in 1886–87.—Indian Meteorological Memoirs, vol. iii. Part 2 (Calcutta).—A Manual of the Geology of India; Part 4, Mineralogy: F. R. Mallet (Trübner).—Bulletin of the U.S. Geological Survey, No. 39 (Washington).—The Law of the Universe: G. W. Cleverley (Brown, Hull).—Quarterly Journal of the Geological Society, vol. 44, Part 1, No. 173 (Longmans).—Proceedings of the Linnean Society of New South Wales, 2nd series, vol. ii. Part 3 (Sydney).—List of Contributors to ditto, 1st series (Sydney).—Quarterly Journal of Microscopical Science, February (Churchill).—Journal of the Royal Microscopical Society, February (Williams and Norgate).

CONTENTS.

THURSDAY, MARCH 1, 1888.

PHYSICAL SCIENCE AND THE WOOLWICH EXAMINATIONS.

WE are glad to learn that several Members of Parliament are interesting themselves in this important matter, and that Sir John Lubbock and Sir Henry Roscoe have both put down notices of motion calling attention to the changes that it is proposed to make in the regulations for admission to Woolwich. We hope and believe that their efforts will result in a rectification of these ill-conceived regulations.

We have already shown in our previous articles on this subject how completely the new regulations fail to find any justification, so far as their treatment of experimental science is concerned. We have demonstrated, by an examination of the professional course of training which the successful cadets will go through when at the Royal Military Academy, that of the subjects of general education experimental science stands below mathematics alone in practical importance for Woolwich cadets; whilst even a cursory inspection of the results of past examinations is sufficient to reveal the hollowness of the suggestion that in scientific subjects marks may be easily obtained by superficial study or cram. When we consider that the results of applying similar regulations in the case of the Sandhurst examinations are, or ought to be, familiar to the War Office authorities, it is astonishing that their extension to the scientific branches of the army should ever have been seriously contemplated.

The deliberate adoption of this scheme for selecting young men for a highly scientific profession, after the experience of several years had so completely established that it is eminently calculated to reduce the chances of candidates of scientific power to a minimum, can only be regarded as a remarkable example of official blundering. The rectification of the mistake is the more imperatively required because the treatment of natural science—that is, of candidates whose abilities are rather scientific than linguistic or mathematical—in public examinations has hitherto been altogether unsuited to the real wants of the age. Science in examinations being to a great extent a non-paying subject, the quality or even the existence of science-teaching is regarded, at the best, as a matter of secondary importance in many or most of our schools. The question, therefore, deserves the closest attention from all who hold that it is absolutely essential that there shall be a steady and sure advance in the standard of elementary science teaching in this country.

In his reply to Mr. Howorth, the Secretary of State for War is stated to have said that these Woolwich regulations had been considered by a "strong Committee." It would be interesting to know of whom this Committee consisted, and whether it was strong from a military or an educational point of view. Such information as we have been able to obtain leads us to conclude that it was a military Committee, and that though, as such, it was no doubt eminently fitted to come to wise conclusions on military questions—such, for example, as the proper training to be given to successful cadets after their admission to the Royal Military Academy—it was by no means composed of men equally fitted by experience to deal with the other side of the question. It is surprising to find that this important change, which will profoundly affect much of the higher school work of the country, was apparently decided upon without, or almost without, consultation with those most experienced in such questions This helps us to understand how it has happened that regulations not altogether unsatisfactory, and to which many places of education had adapted themselves, often at considerable expense and trouble, are suddenly to be displaced by others that are open to the gravest objections.

The new regulations seem to have almost every order of fault. They will be unfair to the candidates, leading to the rejection of those best fitted for the work to be done. It is to be feared, too, that they will encourage residence and study abroad, with the consequent loss of the valuable moral and physical training that can be had only in England. They will also act prejudicially on the general tendency of school education. We hope we may soon hear that better counsels have prevailed, and that these unfortunate regulations are to be replaced by others more in accordance with modern needs and ideas.

TEA CULTIVATION IN INDIA.

Die Theekultur in Britisch-Ost-Indien, im fünfzigsten Jahre ihres Bestandes, Historisch, Naturwissenschaftlich, und Statistisch. Dr. Ottokar Feistmantel. (Prague: O. Beyer, 1888.)

THE subject of tea cultivation in India is one to which innumerable writers have devoted their attention, and not the least valuable portion of Dr. Feistmantel's work, "Die Theekultur in Britisch-Ost-Indien," is the bibliography of the subject with which, while recording his indebtedness for much of his information to many of the English and German authors enumerated, he commences his remarks. In his preface he explains that in the course of an address on the products and exports of British India, recently delivered by him in Prague, he alluded to the fact that on the Continent of Europe tea was generally known only as either Russian or Chinese, and that it was barely known that India produced a large and annually increasing quantity of high-class teas, which are largely used in London for mixing with and improving China tea. The correspondence which ensued when these remarks were reported by the local press induced him to publish the present work as the result of information he had the opportunity of collecting while serving in India for eight years as palæontologist to the Geological Survey.

It is Dr. Feistmantel's aim to place before the German-speaking peoples of the Continent as complete an exposition of the conditions of the tea industry in India as has already been laid before English-speaking people by other writers; and he therefore begins with an abstract of the early history of the tea-plant in India, the dates of its first discovery as an indigenous shrub, and its first introduction into the different districts in which it is now cultivated. He mentions the first export from India to England in 1838 of twelve chests of tea, which sold for 19*s.* 5*d.* per pound.

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He points out the differences between the indigenous, the "China," and the hybrid varieties of the plant which are cultivated in India, and enumerates the various pseudo-teas which are known either in the frontier countries of India or in other countries: such as *Osyris nepalensis* or *arborea*, in Kumaon, Garhwal, and lately in Kashmir; *Elæodendron persicum*, in Burmah, from which, when mixed with oil, salt, garlic, and assafœtida, is prepared the nauseous compound, to European taste, known as "pickled tea"; *Ilex paraguayensis*, the Paraguay tea, or "Mate," of South America; *Ledum palustre*, or Labrador tea; the Tasmanian tea, made from various varieties of *Melaleuca* and *Leptospermum*; and the Faham tea, *Angræcum fragrans* of Mauritius; and others.

The number of plantations in the various provinces, area under cultivation, and annual yield of tea for all India, are given in detail; and the differences between the various kinds of China and Indian tea, as proved by analysis, are very fully treated of. The principal black teas made in India are flowery pekoe, orange pekoe, souchong, pekoe souchong, congou, and bohea; as also the several varieties of broken leaf, such as broken pekoe, pekoe dust, &c. All these are not, as is commonly supposed, the produce of different plants, but are prepared from one and the same plant, the classification being caused by the difference of age and development of the leaves used for the several varieties. The principal kinds of green tea are gunpowder, hyson, and young hyson, and these are manufactured almost exclusively in the North-West Provinces and Kangra.

It may be accepted as a fact that Indian tea is very rarely adulterated, being packed on the plantation, and shipped direct from the planter to the market; but "China tea" passes through many hands before it is packed for shipment, and is frequently mixed with willow or other leaves, or with artificial colouring-matter. But the adulterated tea is not now readily saleable in London, and is therefore re-exported to the Continent. A direct importation of tea from India to the Continent would insure the purity of the supply.

In a lecture given before the Society of Arts, in May last, by Mr. J. Berry White, and quoted by Dr. Feistmantel, a table is given showing the steady rise of the Indian tea crop from 232,000 pounds in 1852 to 76,585,000 pounds in 1886; and Mr. White estimated that the crop for 1887 would not fall far short of 90,000,000 pounds. The amount of tea exported from India between October 1, 1885, and September 30, 1886, is officially returned as 68,784,249 pounds, of which 66,640,749 pounds went to England. Nearly the whole of this tea is consumed in Great Britain, a small quantity being sent to the Continent mixed with inferior China teas, and consequently sold as China tea. The percentage of Indian tea used in England has also been steadily rising, for whereas in 1865 China tea formed 97 per cent. of the entire consumption, in the first quarter of 1887 the proportion was 51 per cent. of Indian to 49 per cent. of China tea.

Notwithstanding the steadily increasing production in India, China tea is still imported into the country; in 1885–86 about four million pounds were imported, but mainly into Bombay, where none is grown, and much of it for re-export to the Persian Gulf, Afghanistan, and some to Trieste, where it arrives as Indian tea.

Statistics concerning the consumption of tea show that the greatest tea-drinkers are the Australians, who in 1881 consumed 81 ounces per head of the population. England ranked next with 73 ounces, while the United States of America came next with 21 ounces. Russia, Belgium, Holland, and Denmark rank highest among Continental nations as tea-drinkers, but they only consume from 7 to 8 ounces per head of the population.

Dr. Feistmantel fully indorses the prevalent English opinion as to the superiority of Indian to China tea, and attributes its being almost unknown on the Continent mainly to the fact that "China tea" is a much older, and therefore better known, product throughout Europe. Even in England Indian tea took years to establish its reputation. It will in the end be as much appreciated on the Continent as it is in this country if a few merchants and tradesmen in different Continental cities, whose commercial standing will be a guarantee for the purity of the goods they supply, are induced to keep it.

A special chapter is devoted to the cultivation of tea in Ceylon, and shows the marvellous progress made by this new industry in consequence of the coffee disease having caused the conversion of so many coffee plantations into tea plantations. In 1875 only 1080 acres were under tea, whereas in 1885 no less than 102,000 acres were occupied by it, and the exports rose from 282 pounds in 1875–76 to nearly four million pounds in 1884–85. The plantations are principally in the western and southern provinces of Ceylon.

Dr. Feistmantel's work concludes with an interesting chapter on caravan teas, compiled from an article by Herr Walter Japha, published in the *Revue Coloniale Internationale* for September–October 1887.

Some amongst us are apt to feel a certain amount of jealousy at the not infrequent employment of foreigners in Government appointments, and this feeling is perhaps intensified by the knowledge that in this matter, as in Free Trade, there is no apparent reciprocity—for we seldom hear of the employment of Englishmen by Continental Governments; but the present is an instance, and by no means a solitary one, of the great service done to us by foreigners who avail themselves of the information they have collected in the course of their employment by our Government to diffuse among their fellow-countrymen such an intelligent knowledge of the productions of our distant possessions as is calculated to largely benefit our commerce by leading to an extensive demand for the goods of which they write.

It would seem, however, scarcely just that the work of diffusing this knowledge should be left to other nations, seeing that the benefits are to be reaped by ourselves. It is hardly likely that in England it will be recognized, as it is in some other countries, to be part of the duties of any Government Department; but why should it not be part of the work of such a body as the London Chamber of Commerce, or the new Imperial Institute, to disseminate information regarding our Colonial and Indian products among Continental nations, and to translate and circulate any useful works on commercial and kindred subjects, published in foreign languages, among such classes of the community as they would be likely to interest?

J. R. ROYLE.

LIVING LIGHTS.

Living Lights: a Popular Account of Phosphorescent Animals and Vegetables. By C. F. Holder. (London: Sampson Low, Marston, Searle, and Rivington, 1887.)

THIS pleasant volume of 167 pages is intended for young students of science, "their unscientific elders, and the boys and girls in general who have not yet had their interest aroused in Nature's works." The field covered is very wide, and the book is truly Germanic in its meanderings. The author would appear to be under the spell of those who "not only know all that is known by other people, but also all that they themselves imagine, which nobody else can possibly know." When it is said that the results obtained by the expeditions of the *Challenger*, *Talisman*, *Albatross*, *Travailleur*, and *Magenta*, are incorporated, no one can raise the charge of antiquity. The author discusses all possible sides of his subject, from luminous man to cosmic dust in its relation to sun-glow and even luminous paint itself, which was, as is well known, anticipated by the Chinese (oh, Mr. Balmain!). It must not, however, be imagined that the volume is a mere compilation. Quite the reverse; for, while the author embodies much that is original, he incorporates manuscript notes, placed at his disposal by our veteran Gosse, and by luminologists such as Giglioli, Dubois, and others.

Technicalities are for the most part relegated to an appendix, with full references to authorities; the result being that while the book, as a whole, furnishes the specialist with a work of reference the body of it is rendered assimilable by the feeblest tyro. The subject is introduced by a consideration of the bottom of the ocean, which the author naïvely terms the "lower firmament"—an idea which he elaborates in the subsequent chapters, treating of "meteors" and "fixed luminaries" of the sea. We meet with many friends of our youth, such as, for example, M. de Tessan's well-worn picture of the phosphorescent sea at Simon's Town, with its accompanying description.

By way of relieving monotony, anecdotes and similes are freely intercalated with the text. Some of the latter are very happy, as, for example, the comparison drawn between the blind-man and the *Bathypterus* (p. 92). On p. 13 we read: "By having a companion to keep up a continuous motion of the (luminous) water, I have almost been able to read the print of a newspaper by the light of these disintegrated (animal) forms"—a literal stern reality this, sufficient to break the heart of a Ruskin.

The author appears to be suffering under a phosphorescence mania. He leads off with the rather extravagant statement, "Among the revelations of modern science none have a more absorbing interest than those relating to the illumination of the deep sea." He is, moreover, a genuine enthusiast, and, like all such, sees the salvation of his race in his own hobby, for he gives it as his opinion (p. 41) that "the discovery of the secret of phosphorescence, and its practical application to the wants of mankind, would result in revolutionizing present systems; a heatless, inexpensive, inextinguishable light being the perfection of possibilities in this direction." Similar sentiments are expressed in the peroration: these we commend to the physicist.

The book is exceedingly well got up, and illustrated by twenty-six plates, most of which have been especially designed for it. One of these, representing the now famous giant Pyrosoma of the *Challenger*, in size proportionate to that of a man, is especially striking, and the publishers have, very properly, reproduced it on the cover. We would, however, suggest that, in the case of sponges and corals more especially, the animals themselves, and not their mere skeletons, should be delineated; the course here adopted is too suggestive of a "matching" of ordinary museum specimens for the sake of effect. Here and there we note a looseness of style and expression such as is frequently met with in a first issue. The book—strictly a general treatise on luminosity—is a conscientious exposition of a fascinating subject, sound though superficial, and in no sense sensational. We wish it success.

OUR BOOK SHELF.

Food Adulteration and its Detection. By J. P. Battershall, Ph.D., F.C.S. (New York: Spon, 1887.)

THE most striking points of this book are the photographic reproductions of various food-stuffs: starch-grains, fat-crystals, also margarine, milk, tea-leaves, &c. In the introduction Dr. Battershall laments the general inefficient state of the law in America, which would apply very much more forcibly to us, regarding adulteration.

The author does very good service in his introduction, drawing attention to the statistics of recent adulteration. From one table, taken from the work of the Public Analysts' Society in England, it appears the percentage of adulteration has not decreased in any appreciable degree, having been 18·10 per cent. in 1875–76, and 17·47 in 1880, and 16·4 in 1883. The Annual Report of the New York City Board of Health for 1885 furnishes some statistics of adulteration which are by no means pleasant, and show a not very high commercial morality, although the majority are said not to be injurious adulterations—merely fraudulent. The author is quite right when he says "that attempts to awaken public interest in the subject are only of real service as they are conducive to the adoption of more advanced and improved measures for the suppression of the practice."

Generally, the subjects are treated in the book in a very practical manner, and a good deal of information is also contained under each heading. Regarding the adulteration of wines, for instance, a good many interesting receipts for making wines are given, and similarly in the case of spirits and liquors. The section on water is a good *résumé* of processes of water analysis. Prominence is rightly given to Prof. Mallet's very sensible conclusions as to the value of analytical methods in respect to the hygienic character. Dr. Koch's biological method, cultivation in prepared gelatine, is mentioned, and a plate showing the living forms in Croton water and Brooklyn water is given, but we are not frightened by any alarmist theories or statements as to the injurious nature of these organisms; indeed, we are told that the greater number are unobjectionable, and frequently even of service, which is doubtless the case. The really active Bacteria are much less impressive in appearance.

There is a pretty long chapter on legislation in the United States on adulteration, which is not of much use, but is still interesting, to an English reader. The bibliography is very useful. Altogether it is a readable and useful book, and will doubtless meet with a good reception.

W. R. H.

Dynamics and Hydrostatics. By R. H. Pinkerton, B.A. (London: Blackie and Son, 1888.)

THIS is a first course of dynamics intended for the use of science classes and colleges, and specially adapted to the requirements of the Science and Art Examinations in theoretical mechanics. The subject is treated mathematically, but the mathematical knowledge required for an intelligent perusal of the book is limited to elementary algebra and trigonometry. The fundamental units are thoroughly well explained, and, which is saying a great deal, they are used consistently throughout. Every important proposition is followed by a number of good examples fully worked out, and many others are given as exercises.

The book is excellently adapted to the Second Stage of the Science and Art Syllabus, and teachers will not have much difficulty in selecting the portions suitable for students working for the First Stage. It is also well adapted for the use of students working at the subject for the London Matriculation and other University Examinations. But, notwithstanding these qualifications, it is thoroughly conscientious. In fact, from a mathematical point of view, the book leaves nothing to be desired, but in this practical generation a greater number of illustrations from every-day life would not have been out of place. A. F.

Geography for Schools. By Alfred Hughes, M.A. Part I. Practical Geography. (Oxford: At the Clarendon Press, 1887.)

THERE are many signs that the study of geography will in future take a much more important place in the ordinary school course than has hitherto been assigned to it. Even from the point of view of those severely practical persons who care little about the purely intellectual aspects of education, there can be no doubt as to the value of the kind of geographical knowledge with which this book is chiefly concerned; and the subject, if properly treated, is one in which young scholars may easily be led to take genuine interest. The present volume will be of great service to schoolmasters who may wish to make a fresh start in geographical teaching. It is based, as Mr. Hughes explains, on the results of seven years' experience in the modern side at the Manchester Grammar School; and no one who examines the book will be surprised that he has found it possible, within the limits of an ordinary term's geographical course, to give instruction on many classes of problems which are not usually treated at school. He begins with the consideration of latitude and longitude, and with rules for the drawing of maps from the atlas and from memory. He then deals with the measurement of the distance between two places on the earth's surface, and explains the rotation of the earth, with the consequent difference in the time of day at two places on the earth. The remaining subjects are the apparent movements of the fixed stars; the Pole star; Polar distance; the apparent movements of the sun; the seasons; meridian altitude of the sun; declination; the length of day and night at any time and place; the sun's altitude; place of sunrise and sunset; the length of twilight; apparent and Greenwich mean time; movements of the earth; the length of shadows; the distance to be seen from mountain summits; the trade winds; and the calendar. The questions connected with these subjects are discussed in a way that secures the combination of geography, geometrical drawing, arithmetic, and the elementary ideas of geometry; and the author's aim is to induce the student to think for himself, rather than to burden his memory with disconnected facts. It is hardly necessary to say how much better this is than the learning of the names of capes, mountains, rivers, &c., by heart. With such a work in their hands, teachers should be able to make lessons in geography a most useful introduction to the study of some important branches of scientific method.

Key to Todhunter's Differential Calculus. By H. St. J. Hunter, M.A. (London: Macmillan and Co., 1888.)

THIS "Key" will be extremely useful to those who are teaching the subject, but more so to those who are getting it up by themselves. The examples are worked out in a clear and intelligible manner, the geometrical problems being so worded that the student can supply figures to enable him more readily to follow the reasoning. To the chapters on "Curve Tracing" and "Miscellaneous Propositions" the author has added figures; and in the solutions to some of the examples in chaps. xi., xiii., xv., xx., and xxii., improved methods have been adopted, making the book more useful and complete. Great care seems to have been taken to insure accuracy.

Electrical Instrument Making for Amateurs. By S. R. Bottone. (London: Whittaker and Co., 1888.)

IN this little book the author has placed before the reader very good and economical methods of making the more useful pieces of electrical apparatus, using only tools of the simplest kind, such as may be found in any household. The instructions are given in a clear and simple manner, and are illustrated by woodcuts, showing the various parts of the apparatus, with the proportions marked on them. Those who are attending courses of lectures on this subject will find this volume immensely useful, as a more thorough and practical insight is obtained by making and using these instruments, however rough, than by mere reading.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Language = Reason.

PROF. ST. GEORGE MIVART has read my letter on "Language = Reason" in NATURE of February 2 (p. 323) with very great care, and I feel grateful to him for several suggestive remarks. But has he read the heavy volume to which that letter refers—my "Science of Thought"? I doubt it, and have of course no right to expect it, for I know but too well myself how difficult it is for a man who writes books to read any but the most necessary books. I only mention it as an excuse for what might otherwise seem conceited—namely, my answering most of his questions and criticisms by references to my own book.

Prof. Mivart begins by asking why I should have explained reasoning by reckoning.

Now, first of all, from an historical point of view—and this to a man who considers evolution far more firmly established in language than in any other realm of Nature is always the most important—the Latin *ratio*, from which came *raison* and our own *reason*, meant originally reckoning, casting up, calculation, computation, long before it came to mean the so-called faculty of the mind which forms the basis of computation and calculation, judgment, understanding and reason.

Secondly, I began my book on the "Science of Thought" with a quotation from Hobbes, that all our thinking consisted in addition and subtraction, and I claimed the liberty to use the word thinking throughout my own book in the sense of combining. Such a definition of thinking may be right or wrong, but provided a word is always used in the sense in which from the beginning it has been defined there can at all events be no misapprehension nor just cause of complaint on the part of the critic

What I meant by combination, or by addition and subtraction being the true character of thinking, I explained very fully. "Any book on logic," I said, "will teach that all our propositions are either *affirmative* or *negative*, and that in acquiring or communicating knowledge we can do no more than to say that A is B, or A is not B. Now, in saying A is B, we simply add A to the sum already comprehended under B, and in saying A is not B, we subtract A from the sum that can be comprehended under B. And why should it be considered as lowering our high status, if what we call thinking turns out to be no more than adding or subtracting? Mathematics in the end consist of nothing but addition and subtraction, and think of the wonderful achievements of a Newton or a Gauss—achievements before which ordinary mortals like myself stand simply aghast."

Prof. Mivart holds that there are but two forms of intellectual activity: (1) acts of intuition, by which we directly apprehend certain truths, such as, *e.g.*, our own activity, or that A is A; and (2) acts of inference, by which we indirectly apprehend others, with the aid of the idea "therefore."

There is a wide difference between our apprehending our own activity and our apprehending that A is A. Apprehending our own activity is inevitable, apprehending that A is A is voluntary. Besides, the "therefore" on which Prof. Mivart insists as a distinguishing feature between the two forms of thought is present in the simplest acts of cognition. In order to think and to say "This is an orange," I must implicitly think and say, "This is round, and yellow, has a peculiar skin, a sweet juice," &c.; *therefore* it is an orange. The "therefore" represents in fact the justification of our act of addition. We have by slow and repeated addition formed the concept-name orange, and by saying "This is an orange," we say no more than that we feel justified, till the contrary is proved, in adding this object before us to the sum of oranges already known to us. If the contrary is proved, we subtract, and we add our present object either to the class and name of lemons, citrons, &c., or to a more general class, such as apples, fruit, round objects, &c. We ought really to distinguish, as I have tried to show, not only two, but four phases in every act of cognition, viz. sensation, perception, conception, and naming; and I contend that these four phases, though distinguishable, are not separable, and that no act of cognition is perfect without the last phase of naming.

But how is it, Prof. Mivart continues, that different words in our language have one meaning, and different meanings one word? Does not this show that thought and language cannot be identical?

It has been the principal object of all my mythological studies to account not only for the origin of *polyonymy* and *homonymy*, but to discover in them the cause of much that has to be called mythology, whether in ancient tradition, religion, philosophy, or even in modern science. I must therefore refer Prof. Mivart to my earlier writings, and can only mention here a few well-known cases of mythology arising from polyonymy and homonymy.

We can easily understand why people should have called the planet Venus both the morning and the evening star; but we know that in consequence of these two names many people have believed in two stars instead of one. The same mountain in Switzerland is called by the people on the south side *Blackhorn*, by the people on the north side *Whitehorn*, and many a traveller has been misled when asking his way to the one or the other. Because in German there are two words *Verstand* and *Vernunft*, originally meaning exactly the same thing, German metaphysicians have changed them into two distinct faculties, and English philosophers have tried to introduce the same distinction between the understanding as the lower and reason as the higher faculty.

Nothing is really easier to understand, if only we consult the ancient annals of language, than why the same object should have had several names, and why several objects should have had the same name. But this proves by no means that therefore the name is one thing and the concept another. We can distinguish name and concept as we distinguish between the concave and convex sides of a lens, but we cannot separate them, and in that sense we may call them inseparable, and, in one sense, identical.

Lastly, Prof. Mivart starts the same objection to my system of psychological analysis which was raised some time ago in these columns with so much learning and eloquence by Mr. Francis Galton. He appeals to his own experience, and maintains that certain intellectual processes take place without language. This is generally supposed to put an end to any further argument, and we are even told that it is a mistake to imagine that all men are alike, so far as their psychological processes are concerned, and that psychologists should study the peculiarities of individuals rather than the general character of the human intellect. Now, it seems to me that *l'un n'empêche pas l'autre*, but that in the end the object of all scientific inquiry is the general, and not the individual. The true life of language is in the dialects, yet the grammarian aims at a general grammar. In the same way the psychologist may pay any amount of attention to mere individual peculiarities and idiosyncrasies; only he ought never to forget that in the end man is man.

But it does not even seem to me that intellectual processes without language, as described by Mr. Galton and Prof. Mivart, are at all peculiar and exceptional. I have described similar cases, and tried to account for them, in different parts of my book. If Prof. Mivart says that "a slight movement of a finger may give expression to a meaning which could only be thought in words by a much slower process," I went much further by saying that "silence might be more eloquent than words."

Mr. Galton asked me to read a book by Alfred Binet, *La Psychologie du Raisonnement*, as showing by experiments how many intellectual acts could take place without language. I read the book with deep interest, but great was my surprise when I found that M. Binet's observations confirmed in the very strongest way my own position. I had shown how percepts—that is, images—could exist with a mere shadow of language, and that nothing was more wonderful than what Leibniz called the algebra of thought. Now, what do M. Binet's experiments prove? That there are two kinds of images, the *consecutive*, reproduced spontaneously and suddenly, and the *memorial*, connected with an association of ideas. The *consecutive* image, a kind of impression *avant la lettre*, may reappear long after the existing sensation has ceased to act, and it reappears without any rhyme or reason. But how are the memorial images recalled, seen by people, such as M. Binet describes, in a state of hypnotism? Entirely by the word. Show a hypnotized patient her portrait, and she may or may not recognize it. But tell her, in so many words, "This is your portrait," and she will see her likeness in a landscape of the Pyrenees (pp. 56–57). M. Binet is fully aware of what is implied by this. Thus, on p. 58, he writes: "*L'hallucination hypnotique est formée d'un image suggérée par* la parole." So, again, when describing the simplest acts of perception, M. Binet explains how much is added by ourselves to the mere impressions received through the senses by "*ce qu'on croit voir*," by "*ce qu'on croit sentir*," and by "*le nom qu'on croit entendre prononcer*." The facts and experiments, therefore, contained in M. Binet's charming volume seem to me entirely on my side, nor do I see that that thoughtful observer has ever denied the necessity of language or signs of some sort for the purpose of reasoning, nay even of imagination.

I find it difficult to answer all the questions which the Professor has asked, because it would seem like writing my own book over again. However, I shall confess that I have laid myself open to some just criticism in not renouncing altogether the metaphorical poetry of language. I ought not to have spoken of Truth as a kind of personal being, nor of Reason as a power that governs the universe. But no astronomer is blamed when he uses the old terminology of sunrise and sunset; no biologist is misunderstood when he speaks of mankind; and no philosopher is denounced when he continues to use the big I instead of "succession of states of consciousness." If, therefore, I said that I recognized in evolution the triumph of reason, I meant no more than that I could not recognize in it the triumph of mere chance. Prof. Mivart imagines that I misunderstood what the biologist means by the survival of the fittest. Far from it, I understand that phrase, and decidedly reject it. For, either the survival of the fittest means no more than that that survives which is able to survive,—this would be mere truism and a patent tautology,—or, if we take in the whole circumstance of Nature, the survival of the fittest implies some kind of inherent fitness and reasonableness. Prof. Mivart writes: "What there is less reasonable and right in a Rhytina than in a Dugong, or in a Dinornis than in an Apteryx, would, I think, puzzle most of our zoologists to determine; nor is it easy to see a triumph of reason in the extermination of the unique flora of St. Helena by the introduction of goats and rabbits." No doubt, it is not easy to see this. But need I remind Prof. Mivart that many things may be true, though it is not easy to see them? We often do what we think is reasonable and right, though we seem to see nothing but mischief to ourselves and others arising from our acts. Why do we do this? Because we believe in the ultimate triumph of reason

and right, though it may take millions of years to prove that right is right. I have the same faith in Nature; and, taking my stand on this scientific faith, I believe that natural selection must in the end prove rational selection, and that what has vaguely been called the survival of the fittest will have to be interpreted in the end as the triumph of reason, not as the mere play of chance. F. MAX MÜLLER.

Oxford, February 21.

"Coral Formations."

CAPTAIN WHARTON'S paper on coral formations in last week's NATURE (p. 393) will have been read with great interest by all who have examined and studied coral reefs. It is unlikely that any objections will be raised to the illustrations he has brought forward of how the coral plantations may be built up from deeply submerged banks, and eventually formed into complete atolls and barrier reefs at a great distance from continental and other shores. The mode of formation has been dwelt upon by Le Conte and Guppy in the case of barrier reefs, and I have pointed out the same thing in my remarks about the Maldive and similar atoll groups. The instances cited by Captain Wharton are of great value, especially as he has been able to consult large manuscript plans.

Captain Wharton apparently considers that the solution of carbonate of lime by sea-water plays no important part in deepening, widening, and modifying the form of such atolls and barrier reefs; in this I cannot agree with him.

By reference to what is now taking place in Nature, as well as to experiments conducted in the laboratory, it has been shown that the solution of the carbonate of lime of dead shells and skeletons by the sea is as constant and universal as its secretion by the living organisms. From some considerations which I recently laid before the Royal Society of Edinburgh, it is probable that there is more secretion and deposition of carbonate of lime in the ocean, as a whole, than removal by solution, and it is almost certain that at the present time there is a vast accumulation of carbonate of lime going on within the coral-reef regions of the ocean. The amount of secretion becomes less with increasing depth beyond one hundred fathoms, and laboratory experiments under great pressures have shown that the rate of solution becomes greater with increasing depth; but both processes are always in action wherever there are life and growth, death and decay. In some regions secretion is in excess, and there is a formation of calcareous deposits; in others solution is equal to secretion, as over the red clay areas of the ocean. Again, solution may be in excess of secretion, as in the larger and more perfect coral lagoons. The *rôle* of carbonate of lime in the ocean may not inaptly be compared to that of aqueous vapour in the atmosphere over land surfaces. Where precipitation is in excess of evaporation, fresh-water lakes are formed, and rivers carry the surplus water down to the ocean; where evaporation is in excess, there is a formation of inland drainage areas, deserts, and salt lakes.

In small coral atolls the periphery is large relatively to the size of the lagoon, and the secretion of lime and formation of coral sand are greatly in excess of the solution that takes place, hence the lagoon becomes filled up; in it are frequently found deposits of sulphate of lime, guano, magnesian and phosphatic rocks. On the other hand, when a comparatively large atoll reaches the surface, the periphery being small relatively to the size of the lagoon, there is less secretion and formation of coral sand by the living outer surface than is removed in solution from the lagoon; it is in consequence widened, deepened, and reduced to a more or less uniform appearance, while the islands on such reefs never, so far as I know, contain deposits of sulphate of lime, guano, magnesian or phosphatic rocks. On open banks, such as the Macclesfield and Tizard Banks, the coral sand is generally largely made up of bottom-living Foraminifera, Polyzoa, Serpulæ, and Calcareous Algæ, and the bank may be rising from the secretions of these organisms; but when the peripheral reefs reach the surface the conditions become more or less inimical to vigorous growth, and in a perfect atoll the fine calcareous mud is removed at a relatively rapid rate.

My answer to Captain Wharton's question is that in all normal conditions the extent of surface in the shell, coral, or fragment of coral sand exposed to the action of sea-water compared with the mass determines the rate at which these organisms will disappear in solution. It is improbable that this action is extremely slow at the bottom of the deep lagoons. Independently of the mixing by convection currents, even a very slight wind over the surface of the lagoon will set the whole water in motion. This is clearly shown by my observations in the western lochs of Scotland, which are much deeper than any lagoon; a moderate breeze produces motion at a depth of sixty fathoms in a very short space of time. The water mixed up with the mud at the bottom is thus changed long before the point of saturation is reached.

I have never seen any wide extent of fringing reef but what was very deeply cut up with channels, and from Captain Wharton's own description this appears to be the case at Rodriguez. That a ship channel has not there been formed is probably due to the shallow water surrounding the island and the probably rapid growth outward of the reef; the average depth outside the reef is usually less than ten fathoms, and at a distance of two miles seaward it is only from twenty to thirty fathoms. In some instances the large proportion of Calcareous Algæ on the reefs appears to compensate for the removal in solution, and thus to retard the formation of ship channels.

I doubt if any recent writer has attempted to give an "explanation which will fully account for the almost infinite variety of coral formations." It is unnecessary to state that each reef must have peculiarities depending on the nature and form of its foundation, and the meteorological and other conditions of the seas in which the reef is situated; it is only by a careful and detailed study of all these conditions that the peculiarities of any individual reef can be fully explained. At the same time it appears to me beyond doubt that the general and well-known characteristic features and form of coral reefs can be accounted for by reference to certain general considerations, chief among these being the vigorous growth of reef-forming species in positions and at depths where the supply of pelagic oceanic organisms, which form their food, are most abundant, and the removal of dead coral and coral debris wherever this is exposed to the action of sea-water.

Captain Wharton calls attention to our imperfect knowledge of the coral groups of the Pacific, but he understates the case in saying "that the waters of the Fiji and the Society Islands are the only ones which can be said to be in any sense surveyed." Cook, Kotzebue, Duperrey, Beechy, and Wilkes have given running surveys of many of the Paumotus, and we know something about the depths inside and outside of a good many of them. We know much about the islands containing guano. The French have made some excellent charts of the New Caledonia reefs, and the Americans have done the same for some of the Hawaiian Islands. Captain Wharton will acknowledge that we have a splendid survey of the Maldives, the most extensive group of atolls in the world; the islands marked with names in this British Survey number 602. Other groups in the Indian Ocean are well surveyed, and nearly all the Atlantic reefs have been correctly laid down on charts.

I feel sure that all who take an interest in this subject will hope for many more contributions from Captain Wharton's pen on coral formations. JOHN MURRAY.

I HAVE read with great interest the article on coral formations in your last number (p. 393), by Capt. Wharton. It is not because I wish to claim to have anticipitated the views which he gives as to the formation of atoll lagoons and barrier reef lagoons that I am writing to state that at the very date of the publication of Capt. Wharton's article I was engaged in writing a paper on coral formations, based upon a study of living corals at Diego Garcia, and on a consideration of the great submerged atolls known as the Great Chagos Bank and the Pitt and Centurion Banks, situated north and west of that island, in which I arrive at conclusions nearly identical with his. It has seemed to me, as it has to him, that the solution of dead coral rock in the interior of a reef does not sufficiently account for the formation of lagoons, and that the true cause of the atoll and barrier lagoons surrounded either by a reef which is awash, or by a strip of low land, lies in the peculiarly favourable conditions for coral growth present on the steep external slopes of the reef. In Diego Garcia I observed that although the shore reefs are for the most part covered with 1 or 2 feet of water, even at the lowest spring tides, yet their flat surfaces are nearly invariably barren of growing coral. Just at their edges, however, and on the steep external slopes beyond the edges, reef-building corals grow luxuriantly. According to Capt. Moresby, quoted by Mr. Darwin in his book on "Coral Reefs," the flat surface of the rim of the Great Chagos Bank is barren of living corals,

just as are the shore reefs of the neighbouring atoll of Diego Garcia; but the lagoon contains many knolls abundantly covered with living coral, and there is reason to think that living coral also occurs on the external slopes at Diego Garcia. Unlike Capt. Wharton, I do not consider the favourable conditions for coral growth on the external slopes to be connected with a better food supply, for this would be at variance with the existence of thriving coral patches within the lagoon, which, as I have seen at Diego Garcia, bear no relation to the lagoon mouths, through which food-bearing currents might be supposed to enter to the interior. Indeed, at the last-named atoll some of the most luxuriant coral patches are found at the south end of the lagoon, furthest away from the lagoon outlet. The favourable conditions are due, I believe, to the action of currents on coral growth. I noticed at Diego Garcia, and Dr. Hickson has made similar observations in the reefs near North Celebes, that corals do not thrive where they are subjected to the direct action of a strong current, nor do they grow in still water, where they are killed by the sand deposited upon them, but they flourish in places where a moderate current flows over them, not so strong as to dash them to pieces, but strong enough to prevent deposition of sand. Such conditions are found everywhere on the external slopes. At the side where a current impinges directly on a slope, the deeper parts of the current strike the slope first, and are in part thrown upwards over the sloping surface, thus moderating the direct force of the more superficial part of the same current. The main part of the current flows tangentially around the obstruction, and thus affords favourable conditions at the sides of the atoll or reef, and finally, on the side furthest from the current, the back-wash causes weak superficial currents which are also highly favourable to coral growth. Thus the coral grows to the greatest advantage around the periphery of a reef, and, as Capt. Wharton says, a ring-shaped reef is the result, and no theory of solution is required to explain the central depression.

Capt. Wharton states that live coral exists in abundance on the rim of the Tizard Reef. It is not clear whether this means on the external slopes and on the extreme edge of the reef, or on the flat upper surfaces of the reef itself. From what I have observed at Diego Garcia, it appears to me hardly probable that the latter can be the case. Coral debris, torn from the corals growing on the slopes, is always carried across those flat surfaces in such quantity as to destroy any living corals upon them. In some cases corals may grow there, but then there are other favourable conditions neutralizing the effect of the debris. I am hoping soon to publish a full account of my observations at Diego Garcia. G. C. BOURNE.

Anatomical Department, Oxford, February 28.

Natural Science and the Woolwich Examinations.

IN accordance with Mr. Irving's recommendation, I have carefully considered the letter in the *Times* from the head master of Clifton College; but, with all due respect to his distinguished position, I find myself unable to accept his conclusions. Men of science will pardon me, if I ask them to examine facts, rather than to follow blindly even the highest authority.

The obligatory mathematics to be required from candidates for Woolwich are defined as follows in the official regulations, dated December 1887:—

"Algebra up to and including the binominal theorem; the theory and use of logarithms; Euclid, Books i. to iv. and vi.; plane trigonometry up to and including the solution triangles; mensuration; statics—the equilibrium of forces acting in one plane and of parallel forces, the centre of gravity, the mechanical powers; dynamics—uniform, uniformly accelerated, and uniform circular motion, falling bodies and projectiles *in vacuo*. (Analytical methods of solution will not be required.)

"N.B.—A thorough knowledge of each of the above branches of mathematics will be required."

This amount of mathematics is not beyond the reach of a fairly intelligent lad of seventeen who has been properly taught.

The inductive process which leads Mr. Irving to denounce so severely my supposed inappreciation of the value of experimental demonstration, laboratory training, and field work is hardly worthy of so eminent a teacher. Although there are good grounds for my opinion that chemistry, physics, and geology, are not good educational subjects for ordinary lads under sixteen, I am entirely consistent in the expression of my regret that the War Office should have thought it desirable to discourage these sciences. Your able article conclusively proves that these subjects cannot be hastily and superficially learned in such a way as to gain unmerited marks. There are youths with apt intelligences, quick eyes, and skilful fingers, who ought to be allowed the advantage of their scientific capacity in the Woolwich competition. But I am unable to see that Mr. Irving's suggestion would do justice to these. A candidate who offered optional mathematics, one language, and two sciences, would be placed at a great disadvantage with those offering optional mathematics, and three languages, both on account of the lower maximum, and also because, with the same relative proficiency, it is so much harder to score in mathematics and experimental sciences and geology than in languages. I therefore respectfully submit that all who have the interests of science at heart should urge that the maximum should be raised to 3000 marks, but I do not think it would be desirable to allow candidates to take more than one subject from Class II., as it would tend to the neglect of more important studies.

2 Powis Square. HENRY PALIN GURNEY.

International Tables.

I AM instructed by the Meteorological Council to request your insertion of the following notice:—

The International Meteorological Congress, which met at Rome in 1879, recommended that a series of international tables should be prepared and issued.

The work was ultimately intrusted to a Sub-Committee, consisting of Prof. Wild and Prof. Mascart.

The Sub-Committee has prepared a scheme of tables, which has met with a general acceptance among the heads of European meteorological organizations.

The tables will be in royal quarto, and will cover about 400 pages. The price of the work, to be published by Gauthier-Villars, will be 35 francs.

The Council are requested by the gentlemen who have prepared the tables to ascertain the probable demand for the work in this country, and I am therefore to request through your columns that any intending purchaser will send his name to me.

ROBERT H. SCOTT.

Meteorological Office, 116 Victoria Street, London, S.W., February 16.

PLAN OF TABLES.

CHAPTER I.

Section I. Length.

1.	French lines	to mm.	...	0—100 lines
2.	,,	,, English inches	...	,,
3.	French inches and lines	,, mm.	...	20—30 inches
4.	French lines	,, English inches	...	250—350 lines
5.	English inches	,, mm.	...	0—100 inches
6.	,,	,, ,,	...	17—32 ,, (in ·001 inch)
7.	mm.	,, English inches	...	0—100 mm.
8.	,,	,, ,,	...	440—800 ,,
9.	Russian half-lines	,, mm.		
10.	,, ,,	,, English inches		
11.	French feet	,, metres		
12.	,,	,, English feet		
13.	English feet	,, metres		
14.	Metres	,, English feet		
15.	Kilometres	,, English miles		
16.	English miles	,, kilometres		

Section II. Weight.

1.	Grains	to grammes
2.	Grammes	,, grains

Section III. Time and Angular Measure.

1.	Days of year	to decimals of year and to angles
2.	Hours	,, ,, ,,
3.	Minutes	,, ,, ,,
4.	Hours	,, decimals of day
5.	Minutes	,, ,, ,,
6.	Seconds	,, ,, ,,
7.	Minutes or seconds	,, decimal of the hour or minute
8.	Seconds	,, decimals of hour
9.	Longitude	,, time
10.	Time	,, longitude

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1. Variation of gravity with latitude and altitude.
2. Degrees on the meridian.
3. ,, on circles of latitude.
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1. Vapour-tension to 0·1 C. from − 30° C. to + 101° C.
2. ,, ,, 0·2 F. ,, − 20° F. ,, + 214° F.
3. Boiling-point (from 680 mm.—800 mm.) ... metric.
4. ,, ,, ,, ... English.
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6. ,, ,, 212° F. ... ... English.
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10. ,, ,, ... English.

CHAPTER VI.—WIND.

1. Lambert's formula.
2. Natural tangents.
3. Kilometres per hour to metres per second.
4. Metres per second ,, kilometres per hour.
5. Miles per hour ,, metres per second.
6. Metres per second ,, miles per hour.

CHAPTER VII.—MAGNETISM AND ELECTRICITY.

1. English mag. units to C.G.S. units.
2. C.G.S. ,, Eng. mag.

Weight and Mass.

THE review of Kennedy's "Mechanics of Machinery" in NATURE, December 29, 1887 (p. 195), strikes at least one responsive chord on this side of the world. There are some questions in reference to the nomenclature of dynamics which "will not down" until they are "downed" by a convention or agreement between those who have to do with the theory of mechanics and those who have to do mostly with practice, and in this some concessions will doubtless be necessary on both sides. While in hearty sympathy with much that the reviewer says in his discussion of dynamical terms (the book under notice I have not yet seen), I wish to dissent from and to protest against one of his leading propositions.

It must be admitted that in the "vernacular" the word pound is used in two distinct senses—that is, as a unit of force and a unit of mass. Authors of mathematical treatises have sometimes, and perhaps unconsciously, ignored the latter meaning, and at other times have failed to recognize the former.

The proposition of the reviewer is to eliminate the word *mass* altogether and to use weight in its stead. To accomplish this he is obliged to use the word *weight* as meaning what is now generally expressed by the word *mass*. This, it seems to me, would be a grave error. Is it not true that *weight*, as understood by both the "learned and the unlearned" always carries with it the idea of force, the force of attraction between the earth and the particular body under consideration? And is it not also true that there are many problems in the work of the practical engineer in which *mass*, in the ordinarily accepted sense, is the essential element, rather than weight, in the ordinarily accepted sense? In short, in my judgment, the engineer *does* require the word "mass," and he also needs the word "weight." It is a misfortune when one word must be used to mean two entirely different things (as is the case of the word "pound"), and we ought to congratulate ourselves that we have the words "mass" and "weight" so commonly and generally used to represent two distinct ideas. To discard one of them and force the other into its place would be to introduce confusion rather than order. To satisfy the requirements of both mathematical or theoretical and practical convenience I have been accustomed to use the following:—

The word *pound* is used in two senses; it may mean a unit of mass or a unit of force. It is always easy by the context to tell in which sense it is used.

As a unit of force it *has not yet been accurately defined*, but it means, in general, a force equal to the attraction between the earth and a mass of one pound. As this attraction varies slightly, the pound as a unit force cannot be regarded as absolutely constant, but is sufficiently so for practical purposes.

When, by a convention of authorities, the conditions under which this attraction is accepted as equal to one pound are prescribed, it will become an invariable unit.

There are in the English system two units of force, the poundal and the pound. There are also two units of work, the foot-poundal and the foot-pound; each is the work done by the corresponding unit of force working through a distance of one foot.

The ordinary equations of dynamics, when the foot-pound-second units are used, give results in poundals or foot-poundals, which may at once be reduced to pounds or foot-pounds.

The above is open to the objection that the pound as a unit of force is not constant, but the remedy for this is indicated, and the errors introduced are of no moment in "practice."

To lessen the confusion somewhat, I have often used, in writing, the symbol *lb.* to represent the unit of *mass*, and the word *pound* that of force. In my own experience the adoption of these definitions has greatly facilitated the work of students.

I entirely agree with the criticisms made upon the equation so constantly appearing, $w = mg$. To the learner it is generally "confusion confounded," and I would cheerfully join in a "boycott" against it. T. C. MENDENHALL.

Rose Polytechnic Institute, Terre Haute, Indiana, U.S.A., January 26.

ONCE more Prof. Greenhill devotes a large portion of a review to emphasizing and insisting on his peculiar, and I may say extraordinary, mode of regarding the meaning of elementary terms (see NATURE, February 16, p. 361; also December 29, 1887, p. 195).

One must assume, therefore, that these views are regarded by him as useful and conducive to clearness.

I find it difficult to express strongly enough my entire dissent from such a proposition without being apparently impolite.

That engineers are entitled if they see fit to employ as their third fundamental standard a standard of force rather than one of mass, I admit. I do not think the plan satisfactory or clear, but there are temptations towards it, and perhaps no very serious objections. My own experience of engineering students is, however, that they are beautifully uncertain whether to put g into the numerator or the denominator of a new expression, or whether to leave it out altogether; and that they generally get over the difficulty either by asking where it must go, or by seeing which plan will give an answer of most reasonable magnitude. The real rule on engineers' principles would be to put g somewhere into the expression for any quantity with which gravity has nothing to do, and to leave g out whenever gravity is primarily concerned.

But, irrespective of this standing and well-known controversy, Prof. Greenhill's attempt to simplify matters does indeed make confusion worse confounded. He says that in the vernacular the term "weight" does not mean the force with which the earth pulls a body, but does mean the body's mass or inertia.

What kind of "vernacular" can he be thinking of?

Ask any ordinary member of the British public what he or she means by the "weight" of a thing, and you will get answers such as "its heaviness," or "its heft," or the "force required to lift it," or "the difficulty of raising it," or "the pull up you must give it," or any number of such replies; but if he ever got the answer, "I mean the mass of the body, in other words its inertia, a measure of the quantity of matter the body contains," surely he would not be satisfied with this as a fair specimen of the vernacular, but would rather regard it as one of those answers so frequently given to examiners—the product of a mind so tortured by instructors that its common-sense and vernacular are completely atrophied. OLIVER J. LODGE.

The Composition of Water

TWO days after the publication of my letter in NATURE (p. 390), on the composition of water, I received the Manchester

Report of the British Association, in which (p. 668) further experiments by Dr. A. Scott are reported. Dr. Scott has succeeded in reducing the amount of nitrogen present as impurity to 1 part in 15,000, and the ratio of hydrogen to oxygen which he calculates from the newer and more accurate experiments is 1·996 or 1·997 to 1·000. This ratio agrees very well with that deduced by me from the older experiments, but is considerably higher than the ratio previously adopted by Dr. Scott, and quoted by Prof. Thorpe in his article on the composition of water. SYDNEY YOUNG.

University College, Bristol.

ON THE DIVISORS OF THE SUM OF A GEOMETRICAL SERIES WHOSE FIRST TERM IS UNITY AND COMMON RATIO ANY POSITIVE OR NEGATIVE INTEGER.

"Nein! Wir sind Dichter." [1]
—*Kronecker in Berlin.*

A REDUCED Fermatian,[2] $\frac{r^p - 1}{r - 1}$, is obviously only another name for the sum of a geometrical series whose first term is unity and common ratio an integer, r.

If p is a prime number, it is easily seen that the above reduced Fermatian will not be divisible by p, unless $r - 1$ is so, in which case (unless p is 2) it will be divisible by p, but not by p^2.

This is the theorem which I meant to express in the footnote to the second column of this journal for December 15, 1887, p. 153, but by an oversight, committed in the act of committing the idea to paper, the expression there given to it is erroneous.

Following up this simple and almost self-evident theorem, I have been led to a theory of the divisors of a reduced Fermatian, and consequently of the Fermatian itself, which very far transcends in completeness the condition in which the subject was left by Euler (see Legendre's "Theory of Numbers," 3rd edition, vol. i., chap. 2, § 5, pp. 223-27, of Maser's literal translation, Leipzig, 1886),[3] and must, I think, in many particulars be here stated for the first time. This theory was called for to overcome certain difficulties which beset my phantom-chase in the chimerical region haunted by those doubtful or supposititious entities called odd perfect numbers. Whoever shall succeed in demonstrating their absolute non-existence will have solved a *problem of the ages* comparable in difficulty to that which previously to the labours of Hermite and Lindemann (whom I am wont to call the Vanquisher of PI, a prouder title in my eyes than if he had been the conqueror at Solferino or Sadowa) environed the subject of the quadrature of the circle. Lambert had proved that the Ludolphian [4] number could not be a fraction nor the square root of a fraction. Lindemann within the last few years, standing on the shoulders of Hermite, has succeeded in showing that it cannot be the root of any algebraical equation with rational coefficients (see Weierstrass' abridgment of Lindemann's method, *Sitzungsberichte der A.D.W. Berlin*, Dec. 3, 1885).

It had already been shown by M. Servais ("Mathesis," Liège, October 1887), that no one-fold integer or two-fold odd integer could be a perfect number, of which the proof is extremely simple. The proof for three-fold and four-fold numbers will be seen in articles of mine in the course of publication in the *Comptes rendus*, and I have been able also to extend the proof to five-fold numbers. I have also proved that no odd number not divisible by 3 containing less than eight elements can be a perfect number, and see my way to extending the proof to the case of nine elements.

How little had previously been done in this direction is obvious from the fact that, in the paper by M. Servais referred to, the non-existence of three-fold perfect numbers is still considered as problematical; for it contains a "Theorem" that if such form of perfect number exists it must be divisible by fifteen: the ascertained fact, as we now know, being that this hypothetical theorem is the first step in the *reductio ad absurdum* proof of the non-existence of perfect numbers of this sort (see NATURE, December 15, 1887, p. 153, written before I knew of M. Servais' paper, and recent numbers of the *Comptes rendus*).

But after this digression it is time to return to the subject of the numerical divisors of a reduced Fermatian.

We know that it can be separated algebraically into as many irreducible functions as there are divisors in the index (unity not counting as a divisor, but a number being counted as a divisor of itself), so that if the components of the index be a^α, b^β, c^γ, . . . the number of such functions augmented by unity is

$$(\alpha + 1)(\beta + 1)(\lambda + 1). \quad . \quad . \quad . \quad .$$

All but one of these algebraical divisors, with the exception of a single one, will also be a divisor of some other reduced Fermatian with a lower index: that one, the core so to say (or, as it is more commonly called, the irreducible primitive factor), I call a cyclotomic function of the base, or, taken absolutely, a cyclotome whose index is the index of the Fermatian in which it is contained.

It is obvious that the whole infinite number of such cyclotomes form a single infinite complex. Now it is of high importance in the inquiry into the existability of perfect numbers to ascertain under what circumstances the divisors of the same reduced Fermatian, *i.e.* cyclotomes of different indices to the same base can have any, and what, numerical factor in common. For this purpose I distinguish such divisors into superior or external and inferior or internal divisors, the former being greater, and the latter less, than the index.

As regards the superior divisors, the rule is that any one such cannot be other than a unilinear function of the index (I call $kx + 1$ a unilinear function of x, and k the unilinear coefficient) and that a prime number which is a unilinear function of the index will be a divisor of the cyclotome when the base in regard to the index as modulus is congruous to a power of an integer whose exponent is equal to the unilinear coefficient.

As regards the inferior divisors, the case stands thus. If the index is a prime, or the power of a prime, such index will be itself a divisor. If the index is not a prime, or power of a prime, then the only possible internal divisor is the largest element contained in the index, and such element will not be a divisor unless it is a unilinear function of the product of the highest powers of all the other elements contained in the index.

It must be understood that such internal divisor in

[1] Such were the pregnant words recently uttered by the youngest of the splendid triumvirate of Berlin, when challenged to declare if he still held the opinion advanced in his early inaugural thesis (to the effect that mathematic consists exclusively in the setting out of self-evident truths,—in fact, amounts to no more than showing that two and two make four), and maintained unflinchingly by him in the face of the elegant raillery of the late M. Duhamel at a dinner in Paris, where his interrogator—the writer of these lines—was present. This doctoral thesis ought to be capable of being found in the archives of the University (I believe) of Breslau.

[2] The word Fermatian, formed in analogy with the words Hessian, Jacobian, Pfaffian, Bezoutiant, Cayleyan, is derived from the name of Fermat, to whom it owes its existence among recognized algebraical forms.

[3] I find, not without surprise, that some of the theorems here produced, including the one contained in the corrected footnote, have been previously stated by myself in a portion of a paper "On certain Ternary Cubic Form Equations," entitled "Excursus A—On the Divisors of Cyclotomic Functions" (*American Journal of Mathematics*, vol. ii., 1879, p. 357) the contents and almost the existence of which I had forgotten: but the mode of presentation of the theory is different, and I think clearer and more compact here than in the preceding paper; the concluding theorem (which is the important one for the theory of perfect numbers) and the propositions immediately leading up to it in this, are undoubtedly not contained in the previous paper.

I need hardly add that the term *cyclotomic* function is employed to designate the core or primitive factor of a Fermatian, because the resolution into factors of such function, whose index is a given number, is virtually the same problem as to divide a circle into that number of equal parts.

[4] So the Germans wisely name π, after Ludolph van Ceulen, best known to us by his second name, as the calculator of π up to thirty-six places of decimals.

either case only appears in the first power; its square cannot be a divisor of the cyclotome.

It is easy to prove the important theorem that no two cyclotomes to the same base can have any the same *external* divisor.[1]

We thus arrive at a result of great importance for the investigation into the existence or otherwise of perfect odd numbers, which (it being borne in mind that in this theorem the divisors of a number include the number itself, but *not* unity) may be expressed as follows:—

The sum of a geometrical series whose first term is unity and common ratio any positive or negative integer other than $+1$ *or* $1-$ *must contain at least as many distinct prime divisors as the number of its terms contains divisors of all kinds; except when the common ratio is* -2 *or* 2, *and the number of terms is even in the first case, and* 6 *or a multiple of* 6 *in the other, in which cases the number of prime divisors may be one less than in the general case.*[2]

In the theory of odd perfect numbers, the fact that, in every geometrical series which has to be considered, the common ratio (which is an element of the supposed perfect number) is necessarily odd prevents the exceptional case from ever arising.

The establishment of these laws concerning the divisors and mutual relations of cyclotomes, so far as they are new,

[1] The proof of this valuable theorem is extremely simple. It rests on the following principles:—

(1) That any number which is a common measure to two cyclotomes to the same base must divide the Fermatian to that base whose index is their greatest common measure. This theorem need only to be stated for the proof to become apparent.

(2) That any cyclotome is contained in the quotient of a Fermatian of the same index by another Fermatian whose index is an aliquot part of the former one. The truth of this will become apparent on considering the form of the linear factors of a cyclotome.

Suppose now that any prime number, k, is a common measure to two cyclotomes whose indices are PQ, PR respectively, where Q is prime to R, and whose common base is Θ. Then k must measure $\Theta^P - 1$ and also $\dfrac{\Theta^{PQ} - 1}{\Theta^P - 1}$; it will therefore measure Q, and similarly it will measure R; therefore $k = 1$ [unless $Q = 1$ or $R = 1$; for suppose $Q = 1$, then $\dfrac{\Theta^{PQ} - 1}{\Theta^P - 1}$ is unity, and no longer contains the *core* of $\Theta^{PQ} - 1$]. Hence k being contained in R can only be an internal factor to one of the cyclotomes (viz. the one whose index is the greater of the two). [See footnote at end.]

The other theorem preceding this one in the text, and already given in the "Excursus," may be proved as follows:—

Let k, any non-unilinear function of P, the index of cyclotome χ, be a divisor thereto. Then, by Euler's law, there exists some number, μ, such that k divides $x^{\frac{P}{\mu}} - 1$, but the cyclotome is contained algebraically in $\dfrac{x^P - 1}{x^{\frac{P}{\mu}} - 1}$; hence k must be continued in μ, and therefore in P. Also, k will be a divisor of $x^{\frac{P}{k}} - 1$ and of $\dfrac{x^P - 1}{x^{\frac{P}{k}} - 1}$, which contain $x^{\frac{P}{\mu}} - 1$ and χ respectively; consequently, if k is odd, k^2 will not be a divisor of $\dfrac{x^P - 1}{x^{\frac{P}{k}} - 1}$, and *a fortiori* not of χ. [A proof may easily be given applicable to the case of $k = 2$.]

Again, let $P = Qk^i$, where Q does not contain k. Then, by Fermat's theorem, $x^{k^i} \equiv x$ [mod. k]. and therefore k divides $x^Q - 1$; but it is prime to Q. Hence, by what has been shown, k must be an external divisor of this function, and consequently a unilinear function of Q. Thus, it is seen that a cyclotome can have only one internal divisor, for this divisor, as has been shown, must be an element of the index. and a unilinear function of the product of the highest powers of all the other elements which are contained in the index.

For an extension of this law to "cyclotomes of the second order and conjugate species," see the "Excursus," where I find the words *extrinsic* and *intrinsic* are used instead of *external* and *internal*.

[2] A reduced Fermatian obviously may be resolved into as many cyclotomes, less one, as its index contains divisors (unity and the number itself as usual counting among the divisors). But, barring the internal divisors, all these cyclotomes to a given base have been proved to be prime to one another, and, consequently, there must be at least as many distinct prime divisors as there are cyclotomes, except in the very special case where the base and index are such that one at least of the cyclotomes becomes equal to its internal divisor or to unity. It may easily be shown that this case only happens when the base is -2 and the index any even number, or when the base is $+2$ and the index divisible by 6; and that in either of these cases there is only a single unit lost in the inferior limit to the number of the elements in the reduced Fermatian.

has taken its origin in the felt necessity of proving a purely negative and seemingly barren theorem, viz. the non-existence of certain classes of those probably altogether imaginary entities called odd perfect numbers: the moral is obvious, that every genuine effort to arrive at a secure basis even of a negative proposition, whether the object of the pursuit is attained or not, and however unimportant such truth, if it were established, may appear in itself, is not to be regarded as a mere gymnastic effort of the intellect, but is almost certain to bring about the discovery of solid and positive knowledge that might otherwise have remained hidden.[1] J. J. SYLVESTER.

Torquay, February 11.

LORD RAYLEIGH ON THE RELATIVE DENSITIES OF HYDROGEN AND OXYGEN.[2]

THE appearance of Prof. Cooke's important memoir upon the atomic weights of hydrogen and oxygen,[3] induces me to communicate to the Royal Society a notice of the results that I have obtained with respect to the relative *densities* of these gases. My motive for undertaking this investigation, planned in 1882,[4] was the same as that which animated Prof. Cooke—namely, the desire to examine whether the relative atomic weights of the two bodies really deviated from the simple ratio 1 : 16, demanded by Prout's law. For this purpos a knowledge of the densities is not of itself sufficient; but it appeared to me that the other factor involved, viz. the relative atomic *volumes* of the two gases, could be measured with great accuracy by eudiometric methods, and I was aware that Mr. Scott had in view a redetermination of this number, since in great part carried out.[5] If both investigations are conducted with gases under the normal atmospheric conditions as to temperature and pressure, any small departures from the laws of Boyle and Charles will be practically without influence upon the final number representing the ratio of atomic weights.

In weighing the gas the procedure of Regnault was adopted, the working globe being compensated by a similar closed globe of the same external volume, made of the same kind of glass, and of nearly the same weight. In this way the weighings are rendered independent of the atmospheric conditions, and only small weights are required. The weight of the globe used in the experiments here to be described was about 200 grammes, and the contents were about 1800 c.c.

The balance is by Oertling, and readings with successive releasements of the beam and pans, but without removal of the globes, usually agreed to one-tenth of a milligramme. Each recorded weighing is the mean of the results of several releasements.

The balance was situated in a cellar, where temperature was very constant, but at certain times the air currents, described by Prof. Cooke, were very plainly noticeable. The beam left swinging over night would be found still in motion when the weighings were commenced on the following morning. At other times these currents were absent, and the beam would settle down to almost absolute rest. This difference of behaviour was found to depend upon the distribution of temperature at various levels in the rooms. A delicate thermopile with reflecting cones was arranged so that one cone pointed towards the ceiling

[1] Since receiving the revise, I have noticed that it is easy to prove that the algebraical resultant of two cyclotomes to the same base is unity, except when their indices are respectively of the forms $Q(kQ + 1)^h$ and $Q(kQ + 1)^i$, where $(kQ + 1)$ is a prime number, and Q any number (unity *not* excluded), in which case the resultant is $kQ + 1$. This theorem supplies the *raison raisonnée* of the proposition proved otherwise in the first part of the long footnote.

[2] A Paper read at the Royal Society on February 9.

[3] "The Relative Values of the Atomic Weights of Hydrogen and Oxygen," by J. P. Cooke and T. W. Richards, Amer. Acad. Proc., vol. xxiii., 1887.

[4] Address to Section A, British Association Report, 1882.

[5] "On the Composition of Water by Volume," by A. Scott, Roy. Soc. Proc., June 16, 1887 (vol. xlii. p. 396).

and the other to the floor. When the galvanometer indicated that the ceiling was the warmer, the balance behaved well, and *vice versâ*. The reason is of course that air is stable when the temperature increases upwards, and unstable when heat is communicated below. During the winter months the ground was usually warmer than the rest of the room, and air currents developed themselves in the weighing closet. During the summer the air cooled by contact with the ground remained as a layer below, and the balance was undisturbed.

The principal difference to be noted between my arrangements and those of Prof. Cooke is that in my case no desiccators were used within the weighing closet. The general air of the room was prevented from getting too damp by means of a large blanket, occasionally removed and dried before a fire.[1]

In Regnault's experiments the globe was filled with gas to the atmospheric pressure (determined by an independent barometer), and the temperature was maintained at zero by a bath of ice. The use of ice is no doubt to be recommended in the case of the heavier gases; but it involves a cleaning of the globe, and therefore diminishes somewhat the comparability of the weighings, vacuous and full, on which everything depends. Hydrogen is so light that, except perhaps in the mean of a long series, the error of weighing is likely to be more serious than the uncertainty of temperature. I have therefore contented myself with inclosing the body of the globe during the process of filling in a wooden box, into which passed the bulbs of two thermometers, reading to tenths of a degree centigrade. It seems probable that the mean of the readings represents the temperature of the gas to about one-tenth of a degree, or at any rate that the differences of temperature on various occasions and with various gases will be given to at least this degree of accuracy. Indeed the results obtained with oxygen exclude a greater uncertainty.

Under these conditions the alternate full and empty weighings can be effected with the minimum of interference with the surface of the globe. The stalk and tap were only touched with a glove, and the body of the globe was scarcely touched at all. To make the symmetry as complete as possible, the counterpoising globe was provided with a similar case, and was carried backwards and forwards between the balance room and the laboratory exactly as was necessary for the working globe.

In my earliest experiments (1885) hydrogen and oxygen were prepared simultaneously in a U-shaped voltameter containing dilute sulphuric acid. Since the same quantity of acid can be used indefinitely, I hoped in this way to eliminate all extraneous impurity, and to obtain hydrogen contaminated only by small quantities of oxygen, and *vice versâ*. The final purification of the gases was to be effected by passing them through red-hot tubes, and subsequent desiccation with phosphoric anhydride. In a few trials I did not succeed in obtaining good hydrogen, a result which I was inclined to attribute to the inadequacy of a red heat to effect the combination of the small residue of oxygen.[2] Meeting this difficulty, I abandoned the method for a time, purposing to recur to it after I had obtained experience with the more usual methods of preparing the gases. In this part of the investigation my experience runs nearly parallel with that of Prof. Cooke. The difficulty of getting quit of the dissolved air when, as in the ordinary preparation of hydrogen, the acid is fed in slowly at the time of working, induced me to design an apparatus whose action can be suspended by breaking an external electrical contact. It may be regarded as a Smee cell thoroughly inclosed. Two points of difference may be noted between this apparatus and that of Prof. Cooke. In my mnnner of working it was necessary that the generator should stand an internal vacuum. To guard more thoroughly against the penetration of external air, every cemented joint was completely covered with vaseline, and the vaseline again with water. Again, the zincs were in the form of solid sheets, closely surrounding the platinized plate on which the hydrogen was liberated, and standing in mercury. It was found far better to work these cells by their own electromotive force, without stimulation by an external battery. If the plates are close, and the contact wires thick, the evolution of gas may be made more rapid than is necessary, or indeed desirable.

Tubes, closed by drowned stopcocks, are provided, in order to allow the acid to be renewed without breaking joints; but one charge is sufficient for a set of experiments (three to five fillings), and during the whole of the time occupied (10 to 14 days) there is no access of atmospheric air. The removal of dissolved air (and other volatile impurity) proved, however, not to be so easy as had been expected, even when assisted by repeated exhaustions, with intermittent evolution of hydrogen; and the results often showed a progressive improvement in the hydrogen, even after a somewhat prolonged preliminary treatment. In subsequent experiments greater precautions will be taken.[1] Experience showed that good hydrogen could not thus be obtained from zinc and ordinary "pure" sulphuric acid, or phosphoric acid without the aid of purifying agents. The best results so far have been from sulphuric and hydrochloric acid, when the gas is passed in succession over liquid potash, through powdered corrosive sublimate, and then through powdered caustic potash. All the joints of the purifying tubes are connected by fusion, and a tap separates the damp from the dry side of the apparatus. The latter includes a large and long tube charged with phosphoric anhydride, a cotton-wool filter, a blow-off tube sealed with mercury until the filling is completed, besides the globe itself and the Töppler pump. A detailed description is postponed until the experiments are complete. It may be sufficient to mention that there is but one india-rubber connection—that between the globe and the rest of the apparatus, and that the leakage through this was usually measured by the Töppler before commencing a filling or an evacuation.

The object of giving a considerable capacity to the phosphoric tube was to provide against the danger of a too rapid passage of gas through the purifying tubes at the commencement of a filling. Suppose the gas to be blowing off, all the apparatus except the globe (and the Töppler) being at a pressure somewhat above the atmospheric. The tap between the damp and dry sides is then closed, and that into the globe is opened. The gas which now enters somewhat rapidly is thoroughly dry, having been in good contact with the phosphoric anhydride. In this way the pressure on the dry side is reduced to about 2 inches of mercury, but this residue is sufficient to allow the damp side of the apparatus to be exhausted to a still lower pressure before the tap between the two sides of the apparatus is reopened. When this is done, the first movement of the gas is retrograde; and there is no danger at any stage of imperfect purification. The generator is then re-started until the gas (after from two to five hours) begins to blow off again.

In closing the globe, some precaution is required to secure that the pressure therein shall really be that measured by the barometer. The mercury seal is at some distance from, and at a lower level than, the rest of the apparatus. After removal of the mercury, the flow of gas is continued for about one minute, and then the tap between the dry and damp sides is closed. From three to five minutes more were usually allowed for the com-

[1] I can strongly recommend this method. In twenty-four hours the blanket will frequently absorb two pounds of moisture.

[2] From Prof. Cooke's experience it appears not improbable that the impurity may have been sulphurous acid. Is it certain that in his combustions no hydrogen (towards the close largely diluted with nitrogen) escapes the action of the cupric oxide?

[1] Spectrum analysis appears to be incapable of indicating the presence of comparatively large quantities of nitrogen.

plete establishment of equilibrium before the tap of the globe was turned off. Experiments on oxygen appeared to show that two minutes was sufficient. For measuring the atmospheric pressure, *two* standard mercury barometers were employed.

The evacuations were effected by the Töppler to at least 1/20000, so that the residual gas (at any rate after one filling with hydrogen) could be neglected.

I will now give some examples of actual results. Those in the following tables relate to gas prepared from *sulphuric* acid, with subsequent purification, as already described:—

Globe (14), *empty*.

Date.	Left.	Right.	Balance reading.
1887.			
Oct. 27–Nov. 5	$G_{14}+0{\cdot}394$	G_{11}	22·66
Nov. 7–Nov. 8	—	—	22·89
Nov. 9–Nov. 10	—	—	23·00
Nov. 11–Nov. 12	—	—	21·72

Globe (14), *full*.

Date.	Left.	Right.	Balance reading.	Barometer.	Temperature.
1887.				in.	°C.
Nov. 5–7 ...	$G_{14}+0{\cdot}2400$	G_{11}	20·52	29·416	14·7
Nov. 8–9 ...	$G_{14}+0{\cdot}2364$	G_{11}	19·77	29·830	12·3
Nov. 10–11...	$G_{14}+0{\cdot}2360$	G_{11}	19·18	22·807	11·2
Nov. 12–14...	$G_{14}+0{\cdot}2340$	G_{11}	29·51	30·135	10·3

The second column shows that globe (14) and certain platinum weights were suspended from the left end of the beam, and the third column that (in this series) only the counterpoising globe (11) was hung from the right end. The fourth column gives the mean balance reading in divisions of the scale, each of which (at the time of the above experiments) represented 0·000187 gramme. The degree of agreement of these numbers in the first part of the table gives an idea of the errors due to the balance, and to uncertainties in the condition of the exteriors of the globes. A minute and unsystematic correction depending upon imperfect compensation of volumes (to the extent of about 2 cubic centimetres) need not here be regarded.

The weight of the hydrogen at each filling is deduced, whenever possible, by comparison of the "full" reading with the mean of the immediately preceding and following "empty" readings. The difference, interpreted in grammes, is taken provisionally as the weight of the gas. Thus, for the filling of Nov. 5—

$$H = 0{\cdot}154 - 2{\cdot}25 \times 0{\cdot}000187 = 0{\cdot}15358.$$

The weights thus obtained depend of course upon the temperature and pressure at the time of filling. Reduced to correspond with a temperature of 12°, and to a barometric height of 30 inches (but without a minute correction for varying temperature of the mercury) they stand thus—

November	5	0·15811
,,	8	0·15807
,,	10	0·15798
,,	12	0·15792
	Mean ...	0·15802

The hydrogen obtained hitherto with similar apparatus and purifying tubes from hydrochloric acid is not quite so light, the mean of two accordant series being 0·15812.

The weighing of oxygen is of course a much easier operation than in the case of hydrogen. The gas was prepared from chlorate of potash, and from a mixture of the chlorates of potash and soda. The discrepancies between the individual weighings were no more than might fairly be attributed to thermometric and manometric errors. The result reduced so as to correspond in all respects with the numbers for hydrogen is 2·5186.[1]

But before these numbers can be compared, with the object of obtaining the relative densities, a correction of some importance is required, which appears to have been overlooked by Prof. Cooke, as it was by Regnault. The weight of the gas is *not* to be found by merely taking the difference of the full and empty weighings, unless indeed the weighings are conducted *in vacuo*. The external volume of the globe is larger when it is full than when it is empty, and the weight of the air corresponding to this difference of volume must be *added* to the apparent weight of the gas.

By filling the globe with carefully boiled water, it is not difficult to determine experimentally the expansion per atmosphere. In the case of globe (14) it appears that under normal atmospheric conditions the quantity to be added to the apparent weights of the hydrogen and oxygen is 0·00056 gramme.

The actually observed alteration of volume (regard being had to the compressibility of water) agrees very nearly with an *a priori* estimate, founded upon the theory of thin spherical elastic shells and the known properties of glass. The proportional value of the required correction, in my case about 4/1000 of the weight of the hydrogen, will be for spherical globes proportional to a/t, where a is the radius of the globe, and t the thickness of the shell, or to V/W, if V be the contents, and W the weight of the glass. This ratio is nearly the same for Prof. Cooke's globe and for mine; but the much greater departure of his globe from the spherical form may increase the amount of the correction which ought to be introduced.

In the estimates now to be given, which must be regarded as provisional, the apparent weight of the hydrogen is taken at 0·15804, so that the real weight is 0·15860. The weight of the same volume of oxygen under the same conditions is $2{\cdot}5186 + 0{\cdot}0006 = 2{\cdot}5192$. The ratio of these numbers is 15·884.

The ratio of densities found by Regnault was 15·964, but the greater part of the difference may well be accounted for by the omission of the correction just now considered.

In order to interpret our result as a ratio of atomic weights, we need to know accurately the ratio of atomic volumes. The number given as most probable by Mr. Scott, in May 1887,[2] was 1·994, but he informs me that more recent experiments under improved conditions give 1·9965. Combining this with the ratio of densities, we obtain as the ratio of atomic weights—

$$\frac{2 \times 15{\cdot}884}{1{\cdot}9965} = 15{\cdot}912.$$

It is not improbable that experiments conducted on the same lines, but with still greater precautions, may raise the final number by one or even two thousandths of its value.

The ratio obtained by Prof. Cooke is 15·953; but the difference between this number and that above obtained may be more than accounted for, if I am right in my suggestion that his gas weighings require correction for the diminished buoyancy of the globe when the internal pressure is removed.

[1] An examination of the weights revealed no error worth taking into account at present.

[2] *Loc cit.*

NOTES.

The Woolwich Examinations question, to the importance of which we again direct attention in our first article to-day, is not to be allowed to lapse. Three or four Members of Parliament who are interested in science mean to press the Government for some rational change in the rules.

In accordance with the rule which empowers the election of nine persons annually "of distinguished eminence in science, literature, or the arts, or for public services," Dr. Lauder Brunton, F.R.S., has been elected a member of the Athenæum Club.

The extraordinary interest and value of the botanical collections made by Signor Odoardo Beccari during a residence of several years in the Malay Archipelago, and especially in Borneo, are well known to naturalists. For some time past Signor Beccari has been occupied at Florence with the publication of his results in the work with which botanists, whether systematists or morphologists, are familiar under the name of *Malesia*. Owing to the threatened withdrawal of the modest support which the Italian Government have extended to this publication (his collections having been acquired by the State), there is some reason to fear that it may come to an abrupt termination. Under these circumstances the Bentham Trustees have placed at Signor Beccari's disposal the sum of 1000 francs, which they were informed would secure the continuance of the work for one year. In accepting this support Signor Beccari has informed the Trustees that he hesitates the less to do so as it affords the strongest possible proof of the estimation in which his labours are held in the botanical world generally.

Prof. Isaac Bayley Balfour, of the University of Oxford, has been elected Professor of Botany at the University of Edinburgh in the room of the late Prof. Dickson. Prof. Bayley Balfour is the son of the late Prof. Balfour, Prof. Dickson's predecessor in the Chair.

M. T. Ribot has been appointed to the new Chair of Experimental and Comparative Psychology, founded by the Paris Municipal Council at the Collège de France.

Dr. F. L. Patton succeeds Dr. McCosh as the President of Princeton College. *Science* says:—"Dr. Patton is still a young man, being but forty-five years of age, and has yet to put forth to their fullest extent his marvellous intellectual powers. We seriously question whether any College has a President of so high an intellectual stamp as Dr. Patton."

Mr. Griesbach, the well-known geologist to the Afghan Boundary Commission, and Deputy-Superintendent of the Geological Survey of India, has been permitted to take employment under the Ameer of Afghanistan for the purpose of developing the mineral resources of the country.

Mr. H. O. Forbes has just arrived in England from New Guinea. Mr. Forbes succeeded in reaching the foot of the Owen Stanley range, after the very greatest difficulties owing to the broken nature of the country. When he returned to his camp to make the necessary arrangements for ascending the range, he found it had been attacked and his people dispersed by the natives. He had the greatest difficulty in reaching the coast, and narrowly escaped with his life.

M. Edouard Dupont, Director of the Brussels Natural History Museum, has just returned to Belgium, after an absence of eight months for the purpose of visiting the Congo. M. Dupont has made a very careful study of the region between the coast and the mouth of the Kassai, with a special view to its geology and natural history. The detailed results he will shortly communicate to the Brussels Societies.

The Rev. W. H. Dallinger, F.R.S., will on Thursday next (March 8) begin a course of three lectures at the Royal Institution, on microscopical work with recent lenses on the least and simplest forms of life.

The *Times* understands that King's and University Colleges have been informed that the Privy Council will hear them at some date after April 16 next in support of their joint petition for incorporation as the nucleus of a Teaching University for London. The Privy Council have further desired that, as the petition of the two Colleges appears to be substantially at one with that of the Teaching University Association, the Colleges should present a joint case with that Association not later than March 31.

A deputation from the School Boards of England and Wales had an interview last week with Lord Cranbrook and Sir W. Hart Dyke, to press upon their attention some considerations with regard to technical instruction. In the course of his reply to the various statements made, Lord Cranbrook said that the Technical Instruction Bill would be introduced as soon as possible. The Government, he assured the deputation, fully intended, if possible, to pass the measure, and he ventured to ask those who were interested in it, if they did not get all that they required, to be content with a beginning, and not be too anxious to press extreme conclusions which might raise opposition that did not at present exist.

M. Pasteur, having entered the lists as a competitor for the reward of £25,000 offered by the Government of New South Wales for exterminating the superabundant rabbits, has sent three delegates with a supply of "*microbes du choléra des poules*," with which he hopes to win the prize. Whatever may be thought of this particular remedy, there can be no doubt as to the serious nature of the plague of rabbits in Australia. During last August the rabbit inspectors travelled 20,202 miles and destroyed 2,069,128 rabbit scalps, and from January 1 to August 1 they destroyed 10,538,778 rabbit scalps. The New South Wales Parliament lately provided funds for the making of a rabbit-proof fence from Bourke to the Queensland border.

The earthquake which caused so much alarm at Grenada on the 10th ult. was felt in many parts of the West Indies. There were oscillations at Barbados, St. Lucia, St. Vincent, Grenada, Demerara, and Trinidad, and it is said that in many places much damage was done to house property. The earthquake was also felt on the other side of the Gulf of Paria. In Guiria three houses were destroyed and the earth opened in chasms and closed again. At Yrapa the shock was so severe that a terrified old woman threw herself into the sea and was drowned.

About midnight on January 15 a shock of earthquake was felt by a party of five persons on the road four kilometres west of Trysil Church in Central Norway. The shock was accompanied by a dull rumbling noise like that of a heavy cart passing across a bridge.

On the night of January 5 showers of ashes fell in certain parts of Elverum in Central Norway, in some places making the snow quite gray. It is surmised that the fall may have been connected with some volcanic eruption in Iceland, as has formerly been the case in this locality.

According to the *Panama Star and Herald* a huge wave lately struck the beach at Baraçoa, Cuba. After sweeping in fully 400 feet, it flowed back to the ocean. Nearly 300 huts and houses are said to have been destroyed, but no lives were lost, for the people saw the wave coming and fled to the hills. The beach was swept clear of every habitation that stood upon it. The wave was not a tidal wave, but the result of a three days' north wind.

M. L. TEISSERENC DE BORT discusses, in the *Annales* of the French Central Meteorological Office for 1885, part iv. (Paris, 1887), the importance of the high barometric pressures of Asia for weather forecasts over Europe. The paper deals with various types of isobars existing simultaneously over Asia and Europe, illustrated by charts. The result arrived at is that European offices would derive great advantage from daily telegrams from Asia, especially from the stations already in existence in Siberia which report to St. Petersburg by wire. Hitherto the idea has generally prevailed that the movements of the atmosphere from the westward were alone useful for the prediction of weather changes over Europe.

THE Austrian Meteorological Office has just published its *Jahrbuch* for 1886. The service was established in 1847, and the first volume contained observations for 1848–49. The new series of volumes, of which the present is the twenty-third, began with the year 1864. The stations now number 380, including three abroad, and are closer together than in any other of the larger systems; there are no less than nine stations in Vienna alone. Daily observations are published for eighteen stations; for all the others monthly and yearly *résumés* are given. The Hungarian observations are published in a separate volume.

THE *Pioneer* of Allahabad mentions a circumstance connected with two recent cyclonic storms which is worthy of the attention of meteorologists. These storms struck the Scinde desert between January 24 and 30, and passed in a straight line across the continent to Cuttack at the rate of 250 to 300 miles daily. The second continued unbroken across the Bay of Bengal to Burmah. If the line they followed were prolonged straight westward it would reach Vienna, which is about 3600 miles from Scinde. There seems (says the *Pioneer*) to have been an unusually violent atmospheric disturbance in Vienna in the early part of January, so the time and the rate of travelling would agree with the assumption that the storms were identical with that disturbance.

Two remarkable new fluorides of potassium have been discovered by M. Moissan, the isolater of fluorine. Hydrofluoric acid is well-known to be readily capable of combining with neutral fluorides to form fluorhydrates similar to that of potassium, KF . HF; indeed it was by the use of this latter compound that fluorine was eventually so successfully obtained in the free state. Moreover, the formation of such compounds has been completely accounted for by the vapour-density determinations of Kletzinsky and Mallet, who have shown that the composition of the molecule of hydrofluoric acid just above its boiling-point is H_2F_2. But M. Moissan now shows that this double fluoride of potassium is by no means the only one, that two others, KF . 2HF and KF . 3HF may be readily obtained in well-formed crystals. When dry powdered KF . HF is placed in anhydrous hydrofluoric acid, it disappears almost instantaneously, the liquid becoming sensibly warm; in fact, M. Moissan in a few moments dissolved five to six grammes in ten grammes of the acid. On cooling this mixture to −23° C. white crystals separated out; these were rapidly dried between filter paper, transferred to a platinum tube closed by a paraffined cork, weighed and analyzed. The results of the analyses indicated the composition KF . 3HF. The compound was then synthetically prepared by mixing potassium fluoride and the acid in these proportions, evading any sudden rise of temperature; the liquid was subsequently warmed to 85° in a platinum capsule, but not a trace of hydrofluoric acid vapour escaped, although that substance boils at 19°. Hence it was evident that the HF was locked up in chemical combination, and this was soon observed to be the case, for on removing the source of heat, crystals began to form even while the thermometer indicated 68°; on resuming the ordinary temperature of the room, the whole became a mass of interlaced crystals, which analysis proved to be those of KF . 3HF. These crystals are extremely deliquescent, being decomposed by water into the free acid and potassium fluoride, emitting the acid fumes in a humid atmosphere, and dissolving in water with production of the most intense cold. If they are suddenly heated with crystalline silicon, the mass becomes incandescent, and a violent disengagement of silicon tetrafluoride gas occurs. The stability of this fluorhydrate was strikingly shown by placing a few crystals *in vacuo*, when even after two hours the manometer only showed a difference of 0·01 m. In a somewhat similar manner the compound KF . 2HF was isolated and found to be a liquid at 105°, but crystallizing in the cold. It is to the formation of these fluorhydrates that M. Moissan attributes the preservation of his fluorine-isolating apparatus, and the regular evolution of gaseous fluorine during the electrolysis.

DURING the last two or three years an extensive search for natural gas has been made in the United States. In a paper on the subject, just issued by the U.S. Geological Survey, Mr. J. D. Weeks says the results of this exploration indicate:—(1) That along the Atlantic coast, east of the Appalachian Chain, including in this term the Green Mountains, no gas is found, or, if found at all, in such small quantities as to indicate that it is of comparatively recent origin. It is also found in such horizons, and under such conditions, as to give but little evidence that it is in such storage reservoirs as to promise any considerable supply. (2) That the chief sources of the supply of natural gas in the United States are to be found in the Mississippi Valley, and, so far as present explorations show, in that portion of it east of the Mississippi River. The chief localities that had assumed any prominence as gas-centres at the close of 1886 were in South-Western New York, Western Pennsylvania, North-Western Ohio, and Central Eastern Indiana. To these may be added a locality in Michigan and one in Eastern Kansas.

IN the report of the U.S. Commission of Education for the year 1885–86, just issued, it is stated that seldom in the history of the United States have superior institutions of learning occupied so large a share of public attention or given signs of such vigorous and fruitful life as at the present time. Among these institutions are classed schools of science, pure and applied, which, according to the writer of the report, "have greatly increased the provision for superior instruction, extended its province, and borne an important part in the adjustment of its processes to the demands arising from the extraordinary increase of scientific knowledge and its applications to the leading industries of modern times."

WE have received the annual address to the Asiatic Society, Calcutta, delivered by the President, Mr. E. T. Atkinson. Speaking of the Survey of India, Mr. Atkinson says that most of the operations connected with it during the past year have been devoted to remunerative as distinguished from purely scientific investigation. In many districts the survey has been cadastral with a record of rights. The Baluchistan parties have done a considerable amount of large-scale work around Quetta and towards the Khwajah Amran range, and are now engaged on the half-inch survey of that province. The Himalayan party has been working under Colonel Tanner towards Kulu, and the Andaman party has completed the survey of the coasts of the Nicobars.

WE have received the "Geological Record for 1879," containing an account of works on geology, mineralogy, and palæontology, published during the year, with supplements for 1874–78. The volume is edited by Mr. Whitaker and Mr. W. H. Dalton, and published by Messrs. Taylor and Francis. In the preface Mr. Whitaker explains that as the position of editor of the "Geological Record" has proved to be one that can be held only with great

difficulty by a busy man who does not live in London, it has been taken over by Mr. Topley. The "Record" is to be brought up to date by giving the titles only of papers, &c., for the years 1880 to 1887. The portion for 1880 to 1884 is finished, and in great part printed; and so large is the amount of geological literature that in this contracted form (without abstracts) two volumes will be needed for the five years.

RECENT Shanghai papers contain the report of the "Chinese Scientific Book Depot," an institution which was established three years ago for the purpose of facilitating the spread of all useful literature in the native language throughout China, and especially of books, maps, and other publications of a scientific or technical character. It does not publish works, but has merely organized a system by which the translations and compilations on scientific subjects issued by the various Departments of the Chinese central and local Governments, by missionary and other philanthropic Societies, are more widely distributed amongst the Chinese people. The demand for such books is fast increasing, and the establishment of the central depot, with branches at the more important cities, suggested itself three years since. Self-support has been the motto of the institution, and, in order to overcome Chinese prejudices, everything smacking of foreign influence has been eliminated as far as possible. During the second year a branch was opened at Tientsin, and subsequently Hangchow, Swatow, Pekin, Hankow, Foochow, and Amoy were similarly provided. During the three years about £2500 worth of books, maps, &c., have been sold, some of them finding their way to the most distant parts of China, Corea, and Japan. Taking the average price per volume at 4*d.* to 5*d.*, this would give a circulation of about 150,000 volumes of useful literature, chiefly of a scientific and educational character. The shops have also served to some extent as reading-rooms, where inquirers after Western knowledge have been able to sit down and examine any works in which they felt interested. The number of scientific and other treatises already translated or compiled and published in Chinese under foreign management amounts at present to over 200. To these have been added about 250 of the most useful native works, including scientific treatises by the early Jesuit fathers.

SIR EDWARD BIRKBECK, President of the National Sea Fisheries Protection Association, is now promoting in Parliament a Bill, the object of which is to secure reasonably cheap and rapid transport for common kinds of sea fish, in quantities of 1 cwt. and upwards, from the coast to the various inland centres of population, and thus, by securing a plentiful distribution, to render an ine timable benefit alike to the poor of our inland towns and villages and the fishermen of our coast. The Bill does not attempt to interfere with the rates now charged by railway companies for prime fish, nor with quantities of less than 1 cwt. of common fish. Sir Edward Birkbeck should have no great difficulty in securing sufficient support for so moderate and good a measure.

DR. F. NANSEN, of the Bergen Museum, Norway, who thinks of journeying across Greenland next summer from east to west, intends to land on the east coast at Cape Dan (66° N.), and proceed in a north-westerly direction to Disco Bay. He will be accompanied by three men—a Norwegian soldier well known for his prowess on *Ski*, or snow-runners, and two Lapps, probably the same who accompanied Nordenskiöld. In order to qualify himself for the contemplated task, Dr. Nansen is preparing to travel on *Ski* from Bergen to Christiania, right across the mountains of Central Norway, a feat never before accomplished by anyone.

THE additions to the Zoological Society's Gardens during the past week include an African Civet Cat (*Viverra civetta*) from South Africa, presented by Capt. Webster, R.M.S. *Hawarden Castle*; three Barred Doves (*Geopelia striata*) from Batavia, Java, presented by Mrs. G. A. Thomson; a Cape Crowned Crane (*Balearica chrysopelargus*) from South Africa; a Gold Pheasant (*Thaumalea picta*) from China, deposited; a Common Wolf (*Canis lupus* ♀) European, received in exchange; two Red Kangaroos (*Macropus rufus*), two Suricates (*Suricata tetradactyla*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

SOLAR ACTIVITY IN 1887.—The decline in the three orders of solar phenomena, spots, faculæ, and prominences which had been so marked during 1886, and particularly during the latter part of that year, continued in 1887, and although there was no spotless period so long continued as that of November 1886 (see NATURE, vol. xxxv. p. 445), the mean spotted area for the year just passed has been much below that for the year preceding it, and faculæ and prominences have shown a similar falling off. During the first four months of 1887, sunspots were both few and small, and there were several intervals of a week or longer in which no spots were seen at all; January 9–18, February 7–16, March 3–9, April 4–11, being such intervals. There was also very little on the sun from March 10–15, and from March 27 to April 18. But after this a revival set in and a fine group of spots was seen on the sun, May 14–23, appearing again in the three following rotations, June 5–18, July 3–14, and July 30–August 9. The days of greatest spotted area during the year were July 6, 7, and 8, but after this the spots began to decrease again, and were few and small in September, October, and November. August 23 to September 12 was a very quiet period, spots only being seen on about four days; and October 6–17, October 28 to November 4, and November 21 to December 1, were spotless intervals. The last month of the year, however, showed a second rally, a fine group of spots being observed during its first fortnight, and another appearing as the first passed off at the west limb. On the whole the mean daily spotted area for 1887 was about two-fifths of that which it was for 1886. Comparing the results for 1885, 1886, and 1887, with the years preceding the last minimum, 1885 shows a somewhat greater mean daily spotted area than 1874, 1886 than 1875, and 1887 than 1876. If, therefore, the decline continues to proceed as during the last cycle, the next minimum will fall early in 1890.

The following figures, taken from Prof. Tacchini's tables, as given in the *Comptes rendus*, may be compared with those given for 1885 and 1886 (NATURE, vol. xxxiii. p. 398, and xxxv. p. 445):—

	Sunspots.			Faculæ.
	Relative Frequence.	Relative Size.	Mean Daily Number of Groups.	Relative Size.
January	2·87	9·35	1·17	11·52
February	3·35	7·83	1·32	10·09
March	1·00	3·35	0·42	16·00
April	1·12	7·76	0·68	6·80
May	4·18	22·04	1·11	9·29
June	4·15	29·74	1·37	20·37
July	5·07	25·25	1·68	14·11
August	4·60	23·53	1·32	14·29
September	2·47	15·75	0·56	9·23
October	1·27	20·21	0·70	10·53
November	1·70	6·41	0·71	17·30
December	6·68	40·10	1·21	16·84

In general accord with the above figures are Wolf's "relative numbers." These are given below for 1886 and 1887, together with the monthly means of the variations in magnetic declination as observed at Milan. The agreement in the general form of the curves for spot numbers and magnetic variation has not been so close in 1887 as in some previous years, nor is the calculated mean value for the magnetic variation so near the observed as in 1885 and 1886; the values calculated by M. Wolf's formula being 6'·79 for 1886, and 6'·21 for 1887, but the observed being 6'·72 and 6'·61.

	Wolf's Relative Numbers (Zürich). 1886	1887	Variation in Magnetic Declination (Milan). 1886	1887
January	28·4	13·1	4·07	3·71
February	23·6	15·7	4·91	3·69
March	61·8	2·7	8·61	6·99
April	45·9	7·5	9·89	9·33
May	29·0	17·2	9·06	9·30
June	25·7	16·3	8·37	9·55
July	32·9	26·2	9·58	10·25
August	19·0	21·1	8·17	9·07
September	17·1	6·9	7·61	6·08
October	9·5	5·4	6·33	6·03
November	0·0	4·5	2·48	3·07
December	15·1	20·5	1·61	2·23
Mean	25·7	13·1	6·72	6·61

The fluctuations in the numbers and dimensions of the prominences have not been so great as for the spots, but the prominences likewise showed a maximum in July and a decline afterwards. The highest prominence observed by Prof. Tacchini during the year was on July 2, 2½′ in height. Both faculæ and prominences failed to show a depression similar to that so conspicuous in November in the numbers of the spots, or the revival these displayed in December, the faculæ thus according in their behaviour rather with the prominences than with the spots. The following figures, given by the Rev. S. J. Perry in the *Observatory* for February 1888, show the general decline in prominence activity during 1887, as compared with 1886:—

	Mean Height of Chromosphere. ″	Mean Height of Prominences. ″	Mean Extent of Prominence Arc. °
1886	8·05	24·78	13·26
1887	8·13	23·86	9·29

A NEW COMET.—A comet was discovered by Sawerthal on February 18. It was observed at Cape Town, February 18, 14h. 32·5m., in R.A. 19h. 11m. 32·5s., and N.P.D. 146° 3′ 44″. Daily motion, R.A. + 7m.; N.P.D. − 1° 15′. Its physical appearance was as follows:—It was about the seventh magnitude, had a well-defined nucleus, and a tail a degree in length. It was visible to the naked eye.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 MARCH 4–10.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 4

Sun rises, 6h. 40m.; souths, 12h. 11m. 45·0s.; sets, 17h. 44m.: right asc. on meridian, 23h. 2·5m.; decl. 6° 9′ S. Sidereal Time at Sunset, 4h. 36m.

Moon (Last Quarter on March 5, 3h.) rises, 0h. 26m.; souths, 5h. 14m.; sets, 9h. 54m.: right asc. on meridian, 16h. 3·8m.; decl. 15° 53′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury	6 14	12 1	17 48	22 51·6	3 21 S.
Venus	5 35	10 5	14 35	20 55·6	17 37 S.
Mars	21 49*	3 7	8 25	13 56·3	9 2 S.
Jupiter	1 14	5 27	9 40	16 16·5	20 22 S.
Saturn	13 20	21 18	5 16*	8 10·1	20 40 N.
Uranus	20 39*	2 13	7 47	13 1·8	5 51 S.
Neptune	9 11	16 51	0 31*	3 42·4	17 59 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

March.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
4	49 Libræ	5½	0 0	0 30	334 274
6	B.A.C. 6098	6	2 28	3 25	72 200

March.	h.	
4	11	Jupiter in conjunction with and 3° 47′ south of the Moon.
4	14	Mars stationary.
9	22	Venus in conjunction with and 0° 17′ north of the Moon.

Saturn, March 4.—Outer major axis of outer ring = 44″·8; outer minor axis of outer ring = 16″·0; southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
T Arietis	2 42·1	17 3 N.	Mar.	8,	M
Algol	3 0·9	40 31 N.	,,	4, 0 1	m
			,,	6, 20 50	m
R Persei	3 22·9	35 17 N.	,,	5,	M
λ Tauri	3 54·5	12 10 N.	,,	7, 0 20	m
			,,	10, 23 12	m
ζ Geminorum	6 57·5	20 44 N.	,,	4, 22 0	M
			,,	10, 2 0	m
R Canis Majoris	7 14·5	16 12 S.	,,	9, 21 25	m
S Cancri	8 37·5	19 26 N.	,,	6, 20 59	m
δ Libræ	14 55·0	8 4 S.	,,	7, 1 6	m
U Coronæ	15 13·6	32 3 N.	,,	10, 4 7	m
U Ophiuchi	17 10·9	1 20 N.	,,	5, 1 28	m
X Sagittarii	17 40·5	27 47 S.	,,	4, 3 0	M
β Lyræ	18 46·0	33 14 N.	,,	7, 22 0	M
U Aquilæ	19 23·3	7 16 S.	,,	10, 5 0	m
η Aquilæ	19 46·8	0 43 N.	,,	9, 5 0	m
Y Cygni	20 47·6	34 14 N.	,,	4, 19 11	m
			,,	7, 19 5	m
W Cygni	21 31·8	44 53 N.	,,	5,	m
δ Cephei	22 25·0	57 51 N.	,,	10, 22 0	m

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
From Coma Berenices	190	26 N.	March 8.
Near η Libræ	234	17 S.	Swift. March 7.
,, γ Herculis	244	16 N.	Very swift. Mar. 7.

THE RELATIONS BETWEEN GEOLOGY AND THE BIOLOGICAL SCIENCES.[1]

II.

IN the remarks which I have hitherto made, I have confined myself to the purely biological aspects of palæontology. As astronomy exhibits to us the orderly working of physical and chemical laws in other and far distant orbs, so palæontology presents us with the biological phenomena of many and widely-separated periods.

But besides the biological, there are two other aspects in which fossils may be viewed; and in these aspects their relations are almost entirely with zoological science. It is the recognition of this fact which prevents the geologist from acquiescing with the claims of biologists to treat palæontology as nothing more than a branch of their own science.

The assemblage of fossils found in a particular deposit furnishes us with the most valuable evidence concerning the conditions—such as salinity of water, depth, temperature, pressure, &c.,—under which the deposit must have been formed. And, again, in the changes which the materials of fossils can be shown to have undergone we have very accurate data for determining the succession of processes to which the materials of the deposit must have been subjected since their original accumulation.

It is true that this evidence of fossils concerning the conditions under which deposits have been formed, is of a kind which has been sadly misread in the past. Until the study of deposits which are being formed at the present day was taken up in a systematic manner, it was almost hopeless to avoid numerous sources of error; but at the present day the advantages accruing to geology from the results of deep-sea researches, are at least as great as those which by the same means have been conferred upon biology.

It is almost needless to call attention to the fact that there are vast masses of rock, including most of the calcareous and carbonaceous, and many of the siliceous and ferruginous types, of which the materials have been accumulated entirely by the agency of living organisms; it is impossible to study the petrology of such deposits without an acquaintance with the nature and functions

[1] Address to the Geological Society by the President, Prof. John W. Judd, F.R.S., at the Anniversary Meeting, on February 17. Continued from p. 404.

of the organisms by which they were formed. But, even in the case of many arenaceous and argillaceous deposits, living organisms have played a very important part in their formation. Much of the materials of such rocks can be shown to have been used in building the coverings of organisms, to have filled up their dead shells, or to have been passed through their bodies, before being finally buried under other masses. Rocks destitute of all traces of the solid parts of animals often abound with worm-tracks, burrows, or casts.

The study of the processes by which similar rock masses are being formed at the present day constitutes the only safe guide to us in interpreting the structures presented by ancient rock-masses. Geologists look forward with much interest to the publication of those volumes of the *Challenger* Reports, in which Mr. Murray and M. Renard will deal with these important questions.

We may especially call attention to two classes of errors that have had much to do with the false conclusions which have been arrived at concerning the conditions under which various deposits have been formed in past geological times.

In the first place, it has been tacitly assumed that all marine organisms which come from regions bordering the equator must necessarily have lived under tropical conditions. It would be quite as reasonable to treat the mosses and dwarf willows which border the eternal snows of Chimborazo and Kilima-Njaro as tropical plants. Just as mountains rising in equatorial lands to the limit of perpetual snow exhibit on their slopes every gradation of climate from tropical to frigid, so the depths of the oceans, as we now know, exhibit a perfectly similar transition. As we go downwards not only heat, but light also, rapidly diminishes, and many forms which, because they came from equatorial regions, we have hitherto regarded as tropical, we now know to live in icy-cold water as well as in almost utter darkness.

The large size and abundant development of Cephalopods, Crustaceans, and fish we now know, from recent deep-sea researches, to be no evidence whatever of the presence either of warmth or of light; and Sir Joseph Hooker has abundantly shown the fallacy of similar reasoning when applied to plant-life. I feel sure that, when the full consequences of these important considerations come to be appreciated, the apparent anomalies of many of the supposed climatal conditions of past geological times will altogether disappear. For my own part, I have never felt any difficulty in accepting, as fully equal to the explanation of the facts of the case, the Lyellian doctrine of climate being determined by great changes in the relative positions of the land and water of the globe.

The other cause of misconception with respect to the conditions which must have prevailed during the deposition of geological deposits consists in the acceptance of an utterly false proposition, which, though seldom formulated, is often tacitly acted upon; namely, "If two organisms exhibit similarity of structure, their environment must have been the same."

There never has been wanting abundant evidence of the fallacy of this doctrine. The general structure of the piscivorous bear of the Arctic regions, and of the frugivorous bear of the Malay peninsula, the osteology of the deer of Lapland and of India respectively, exhibit no such differences as would lead us to infer their diversity of habits and surroundings. It has long been known that elephants, rhinoceroses, and hippopotami, with lions, tigers, and hyænas, flourished under Arctic conditions. The deep-sea researches have so added to our knowledge concerning the conditions under which different forms of life exist—especially those belonging to marine faunas—as to demand a complete reconsideration of the conclusions usually accepted by geologists. For there is a general consensus of opinion among the naturalists who have studied the different groups of the deep-sea faunas, that, contrary to what might have been anticipated from the very remarkable conditions under which they live, the deep-sea forms belong, for the most part, to the same families, and often indeed to the same genera, as shallow-water forms.

The bearing of this important conclusion upon the great problem of the distribution of marine forms of life is obvious. Botanists have naturally availed themselves of the proved occurrences of colder climates in many areas to explain difficult facts of plant-distribution, such as the occurrence of well-known Arctic species on the tops of mountains in what are now temperate, or even tropical, districts. But zoologists, now that they know it to be possible for littoral forms to stray into abysmal portions of the ocean, and then subsequently, without profound modification, to re-emerge in other littoral areas, may find a clue to some very remarkable facts concerning the distribution of marine forms of life, without having to resort to explanations which seem necessary in the case of the terrestrial forms of life which appear to be more dependent than the marine types on the circumstances of their environment.

The whole problem of the distribution of marine forms of life requires indeed to be worked out afresh on the basis of these new discoveries; and when this is done, the first to profit by the new generalizations will be geologists, who have long been confronted by seemingly insuperable difficulties in connection with this problem.

As for the very prevalent notions that Ammonites and Belemnites, Trigoniæ and Brachiopods, with Ichthyosaurs, Pliosaurs, and Plesiosaurs, could only have lived in warm, if not actually tropical, climates, I know of no grounds whatever for any such belief. The nearest living allies of the invertebrates referred to flourish at considerable depths in icy-cold water; and, seeing that large marine mammals now live amid snow and ice, I cannot understand why the great marine reptiles might not have done the same. Just as little reason is there for inferring that Sigillarids, Lepidodendrids, and Calamites could only have lived in tropical jungles, as there is for the once popular notion that they flourished in an atmosphere supplied with a very exceptional proportion of carbonic acid!

The sooner geologists recognize the fact that all our ideas concerning the distribution of the forms of marine life have been completely revolutionized by the discovery that there are cold and dark abysses, which are tenanted by numerous organisms having many affinities with those which live in shallow water, warmed by a tropical sun and flooded with light, the more likely will they be to avoid the errors into which we have fallen in the past. Not until the exact distribution of life-forms at different depths in the ocean has been much more perfectly worked out than it has been at present, will it be safe to reason with any confidence concerning the distribution of extinct types; and, even then, we shall ever have to be on our guard against the prevalent fallacy which assumes that analogies in structure are indicative of similarities in the conditions of life.

And here it may be remarked that the imperfect methods employed on board the *Challenger* and most other surveying ships leave almost everything yet to be done in the way of determining the limits of depth, temperature, pressure, and other conditions under which the different forms of marine life can flourish. It is much to have obtained so great an insight into the characters of some of the creatures inhabiting the deepest parts of the ocean, and of the peculiar conditions which must exist in some of those places where marine life is abundant. But the work which has yet to be done requires the employment of dredges and nets which can be opened when they have reached a certain depth in the ocean, and which can be closed again before being drawn to the surface. Only by the employment of such apparatus can we hope to avoid those sources of error which vitiate all our present generalizations concerning the bathymetrical distribution of the existing forms of marine life.

When, in addition to these biological studies, we have equally careful determinations of the physical characters of deposits formed at varying depths and distances from the shore, and under diverse influences of tides and currents, we may hope, by combining the physical and biological evidence, to arrive at something like certain conclusions concerning the exact conditions under which various geological formations have been accumulated; for at present our speculations upon the subject are often little better than haphazard guesses.

The conditions which must have prevailed during the deposition of a particular bed having been determined, the present mineral condition of the organic remains becomes a subject of very interesting study; for here we may find a clue which will enable us to unravel the series of physical and chemical changes which must have gone on in the mass, since the first accumulation of its materials. In cases of difficulty of this kind, the condition of alteration of a shell or bone, of which the original composition is known, becomes an especially valuable piece of evidence.

I am convinced that the future progress of geological thought is closely bound up with the increase of our knowledge concerning the conditions under which the various forms of marine life flourish, and under which their remains become embedded in sedimentary deposits; though what has been already accomplished in this direction, it must be admitted, is but small, and much of it will have to be done over again.

We hear much—far too much, as I think—at the present day of an "irrational uniformitarianism." Is not the real source of danger in an exactly opposite direction? Does not the irrationality characterize him who, without attempting to obtain a more complete knowledge of the processes going on during the original deposition and subsequent changes of rock-masses, is ready, as each new difficulty presents itself, to fall back upon some old discredited *Deus ex machinâ* in the form of deluges of water, floods of fire, boiling oceans, caustic rains, or acid-laden atmospheres!

Considering how little we as yet know of many of the conditions under which deposits are being formed at the present day, and remembering how large a part of the little we do know has been acquired only within the last few years, we might pause before declaring that the path upon which geology entered in earnest only some fifty years ago is a wrong one, and that the sooner we begin to retrace our steps the better.

Can we even now be in danger of forgetting that "Slough of Despond," wherein the geologist, laden with a grievous burden of traditional assumptions and irrational theories, so hopelessly floundered, till one Help pointed out a way of escape, and sent him on his way rejoicing, with the "Principles of Geology" in his hand?

The second aspect in which palæontological science presents itself to the geologist, is as affording a key to the chronology of the rock-masses of the globe. We still regard fossils as the "medals of creation," and certain types of life we take to be as truly characteristic of definite periods as the coins which bear the image and superscription of a Roman emperor or of a Saxon king.

But in the application of the principle that "strata are to be identified by their organic remains," we have now to admit as many limitations, and to exercise as much caution, as when judging of the conditions under which rock-masses must have been deposited, from the characters of the fossils which they contain.

With the restricted area of the south-west of England, where William Smith achieved his epoch-making discovery, the doctrine which he announced seemed to be absolutely true; each formation exhibited a peculiar and perfectly characteristic assemblage of organic remains, by means of which it could at once be recognized. The still more detailed studies of strata of the same age, by Hunton and Williamson in Yorkshire, by Marcou in the Jura, and by Quenstedt in Swabia, seemed to show that the principle had a wider application than even its author himself could have imagined, and that zones a few feet or even inches in thickness might be followed over considerable districts; everywhere marked by some particular type of Ammonite or other characteristic fossil.

But the more thorough and systematic study of corresponding formations over wide areas, which was inaugurated by Oppel, and has been carried on by many palæontologists since, has abundantly demonstrated that, striking as is the parallelism of the zones in such a formation as the Lias, when studied in England, France, and Germany, yet the species and varieties found on the same horizon at distant points are in many cases not identical, but merely representative; and, further, that as we pass away from any typical area, the sharp distinction between the several zones seems gradually to vanish.

The same facts come out very strikingly when we study any other great geological period. In the oldest fossiliferous strata, those of the Cambrian, nothing can be more striking than the similarity of the faunas in North America, Britain, Scandinavia, and Bohemia; and yet the species which occur at the several different horizons in these countries are certainly, for the most part, not identical, but only representative. No fact, it seems to me, could more clearly indicate that, even at that early period, there were life-provinces with a distribution of organisms in space quite analogous to that which exists at the present day.

To pass to slightly younger rocks. What can be more striking than the evidence of the limits of two life-provinces, afforded by the Calciferous strata of North America and the similar rocks of Scotland and Northern Europe, which contain the remarkable *Maclureas* and a peculiar assemblage of Cephalopods and other fossils; for these are seen at Girvan to come into close contiguity with the more southern type of Silurian, containing a very different fauna, so well seen in the Lake District and North Wales.

Another striking example of the same kind is afforded by the Cretaceous, of which the Southern type, marked by the abundance of *Hippurites*, *Orbitolites*, and other remarkable forms, comes into close relations, as has been so well shown by Hébert, with the type which yields the ordinary Cretaceous fauna of Central Europe. In these and similar cases which might be mentioned we trace the existence of two approximating marine provinces, like those which at the present day are separated by the Isthmus of Panama.

Profs. Neumayr and Mojsisovics have indeed shown that there are good causes for believing that the distinction between the marine zoological provinces in Triassic and Jurassic times was at least as clearly marked as between the similar provinces of the present day; and the former naturalist has in addition pointed out that within the geographical provinces we have also very recognizable climatic zones.

In the year 1862, Prof. Huxley, speaking from this chair, uttered a much-needed warning against the growing practice among palæontologists of treating geological equivalence as meaning the same actual contemporaneity; and against the assumption, without positive proof, that ancient faunas and floras had an indefinite and even world-wide distribution. Palæontological discoveries during the last quarter of a century in Western North America, in India, in the Cape Colony, Australia, and New Zealand, have abundantly justified these cautions, and have shown how much such a term as "homotaxis" is needed, in order to guard against errors resulting from the abuse of the phrase "geological contemporaneity."

But when Prof. Huxley went on to suggest that "a Devonian fauna and flora in the British Isles may have been contemporaneous with Silurian life in North America and with a Carboniferous fauna and flora in Africa," I think that geologists, with the evidence they have now before them, must take exception to so sweeping a generalization. Finding, as we do, on both sides of the Atlantic the same succession of Cambrian, Ordovician, Silurian, Devonian, and Carboniferous strata, containing strikingly representative, if not identical faunas, it is impossible to doubt their general parallelism; however ready we may be to admit that the migration or development of new forms of life in the two areas need not have occurred synchronously, and that thus a certain amount of overlapping of the periods represented at distant points by the same system may exist.

On the other hand, I believe that the study of fossils from remote parts of the earth's surface has abundantly substantiated Prof. Huxley's alternative suggestion that "geographical provinces and zones may have been as distinctly marked in the Palæozoic epoch as at present." The ever-accumulating mass of palæontological evidence seems to me to be all pointing in this direction; and I confidently anticipate that the palæontological anomalies which in the past have caused so much doubt and difficulty, will, by the establishment of this principle, receive a full and satisfactory explanation.

As long ago as 1846, Darwin, in his "Observations on South America," showed that certain assemblages of fossils presented a blending of characters, which in Europe are only found apart in faunas which are of Jurassic and Cretaceous age respectively. Since that date, the study of the fossil faunas and floras of South Africa, India, Australia, New Zealand, and the Western Territories of North America has furnished an abundance of facts of the same kind, showing that no classification of geological periods can possibly be of world-wide application: that we must be contented to study the past history of each great area of the earth's surface independently, and to wait patiently for the evidence which shall enable us to establish a parallelism between the several records. Attempts to establish a universal system of nomenclature or classification of sedimentary rocks are indeed greatly to be deprecated, for if the zoological and botanical distribution of past geological times were at all comparable to that of the present day, any such universal system must be impossible.

The suggestion made to this Society by Prof. Huxley at a somewhat later date is equally valuable and important. Referring to the fauna of the Trias, he said:—"It does not appear to me that there is any necessary relation between the fauna of a given land and that of the seas of its shores. At present our knowledge of the terrestrial faunæ of past epochs is so slight that no practical difficulty arises from using, as we do, sea-reckoning for land-time. But I think it highly probable that, sooner or later, the inhabitants of the land will be found to have a history of their own."

The growth of our knowledge concerning the terrestrial floras and faunas of ancient geological periods, since these words were written in 1869, has constantly forced upon the minds of many geologists the necessity of a duplicate classification of geological periods, based on the study of marine and terrestrial organisms respectively.

Upon this important question the judicious remarks of my colleague, Dr. Blanford, must still be fresh in the minds of all geologists and biologists. He showed that not only are terrestrial provinces independent of marine ones, but that at the present, as well as in the past, the former are more circumscribed and have an amount of distinctness which does not exist in the case of the latter.

Nor is it difficult, in the present state of our biological knowledge, to give a reason for the existence of this state of things. Between completely separated land-areas, migration can only take place by such accidents as the transport of seeds or eggs, or as the consequence of the great but slow changes in the relations of sea and land. Forms adapted only for living in cold climates are isolated by tracts of low-lying tropical land, and, conversely, tropical forms are divided off from one another, by snow-covered mountain-chains, almost as distinctly as by actual oceans. The fact that well-known Arctic plants are found at the top of mountains in tropical or temperate lands, has seemed to many botanists as quite inexplicable without calling in the agency of a general refrigeration, like that which marked the Glacial period.

But with marine forms of life the case is totally different. The oceans are not only much larger than the continents, but they are all more or less completely connected with one another.

Forms which live at the surface of the ocean may wander freely in all directions, and know but few limitations except those imposed by temperature, absence of food, &c.; forms living at moderate depths may migrate along shore-lines or submarine ridges from one area to another; and even when abysmal tracts of ocean intervene between two littoral faunas, recent researches seem to show that the littoral forms of life may wander into such tracts, and eventually, perhaps, cross them, without undergoing extreme or profound modification. In this way, I think, we may account for the important fact so prominently brought into view by Dr. Blanford, that terrestrial life-provinces are and always must have been more restricted in area, and more sharply cut off from one another, than marine provinces.

With the clear recognition of this principle there falls to the ground one of the most frequently urged objections to the uniformitarian doctrines—that, namely, which is based on the supposed differences in geographical distribution in ancient times as compared with the present. I have always doubted whether there is any evidence to show that the marine life-provinces of Silurian or Carboniferous times were of greater extent than those of the present day.

I believe that the doctrine that strata can be identified by the organic remains which they contain is as sound as when it was first enunciated by William Smith; but the problems of stratigraphical palæontology, as they now present themselves to us, are infinitely more complicated than they could possibly have seemed to him. In every fauna and flora which we are called upon to study, we have to resolve a function of three variables, these being environment, space, and time. Only after the most careful investigation, in the first place, of the complicated effects produced by the varied conditions which we group together under the term environment—temperature, food, absence of enemies, and the innumerable influences which, as we now know, determine the existence and affect the multiplication of living beings; and by the thorough study, in the second place, of the laws of geographical distribution of plants and animals, can we hope to eliminate the effects due to environment and position, and arrive at the conclusion of what must be ascribed to time.

The task will be long, the work to be done arduous, and the efforts to be made prodigious and sustained; but the result is one which is not hopeless and unattainable, or, indeed, even doubtful. But let us by all means remember that the real work is really only just commenced, and that we are very far indeed from our goal.

One of our greatest sources of danger to the progress of geological knowledge at the present day is the impatience which is so frequently shown at the rate of that progress, an impatience which leads to attempts to cut the tangled skeins of research by hasty and ill-considered speculation. Geologists, no less than biologists, need to recollect and keep ever before their minds the important fact that the geological record, although it is one of enormous value, is exceedingly imperfect, and that this imperfection is quite as conspicuous in respect to physical as it is to palæontological data. How sadly is this important truth lost sight of by those who, on the strength of a few isolated facts and fragmentary observations, are prepared to construct maps of large portions of the earth's surface at far distant periods of its history. Such maps are to the geologist what "genealogical trees" are to the biologist—"will-o'-the wisps" leading us aside from the safe paths of scientific induction.

It is, I suspect, from the obvious failure of attempts of this kind—attempts which had better never have been made—that such frequent suggestions of revolt against the principles of uniformitarianism take their origin. For myself, instead of disappointment, I feel a constant surprise that these doctrines have enabled us to explain so much, when our knowledge of the causes still at work around us is still so imperfect; and I am continually impressed by the fact that each new discovery concerning the present order of Nature removes old difficulties in the explanation of the past. In saying that I adhere to the doctrines of uniformitarianism, I, of course, mean the uniformitarianism which Lyell himself taught, and not the absurd travesty of that doctrine sometimes ascribed to him.

The well-grounded conviction which results from observing the triumph of a great principle, when applied in an overwhelming number of cases, and which refuses to abandon that principle at the first appearance of difficulty, is surely not out of place in a student of Nature. It was this scientific "faith" which led Scrope to believe, in spite of difficulties arising from the imperfect knowledge in his day of physics, chemistry, and mineralogy, that massive and schistose crystalline rocks have been formed from ordinary lavas and sediments, when subjected to enormous pressures and complicated earth-movements; which induced Lyell to seek for and find the key to physical changes during past times in the operations going on everywhere around us; and which finally conducted Darwin, by the application of the same principle, in the case of living beings, to the doctrine of organic evolution.

But, alas! this "faith" seems often sadly wanting among us to-day. At a time when the mineralogical constitution of rocks and of the changes which they undergo is becoming daily more clearly revealed, when innumerable researches are throwing fresh light on the great physical processes taking place everywhere in the world around us, and when each department of biological science is contributing new "facts and arguments for Darwin," such scientific pusillanimity on the part of geologists seems, to say the least of it, singularly inopportune.

Doubtless there are difficulties still unresolved; but does not every advance in our knowledge see the removal of some of them? True the task of interpreting the fragmentary record of the rocks is one the end of which seems very far off; but is not every step we take clearly an approximation towards that end?

If any arguments were needed in favour of the continued and close co-operation of geologists and biologists, it would be found in the circumstance that the most important step in the progress of scientific thought which has been accomplished in modern times has been the direct result of a combination of geological and biological researches.

That remarkable biography, for which we are so greatly indebted to Mr. Francis Darwin, is not simply the record of a life, simple, blameless, and noble beyond that of ordinary men, the story of the workings of an intellect, truth-loving, patient, and powerful, above that of all his contemporaries; it is the history of a most wonderful revolution in human thought—one which will perhaps be regarded in future times as the most striking event of the nineteenth century.

The grand secret of Darwin's success in grappling with the great problem of "the origin of species" is found in the fact that he was at the same time a geologist and a biologist. The concentration of the later years of his life upon zoological and botanical researches has led many to forget the position occupied by Darwin among geologists. Not only are his geological writings of the highest value for the wealth of accurate observations which they contain, and the important generalizations which they put forward; but in his more purely biological works the value of his geological training and experience are constantly exemplified.

It was, indeed, a fortunate circumstance that Darwin, after being repelled by the narrow and soulless system of "geognosy" taught by Jameson at Edinburgh, came at Cambridge under the spell of Henslow, a man of most catholic taste, extensive acquirements, and widest sympathy with all branches of natural science. By intercourse with Henslow, Darwin's flagging interest in science was rekindled and kept alive. It is a proud boast for a University to have nourished the intellectual development of Darwin; and as that University has in the past remained faithful to the memory of Newton, making his mathematical teachings the characteristic and leading feature of its studies, so, we may hope, it will in the future aim at that complete union of geological and biological investigation of which Darwin's labours constitute so grand an example.

In the dedication of his "Journal of Researches," Darwin acknowledged "with grateful pleasure" that "the chief part of whatever scientific merit this journal and the other works of the author may possess, has been derived from studying the well-known and admirable 'Principles of Geology';" and well do I recollect how, in almost every conversation I had with him, he would enlarge with warmth of feeling upon his indebtedness to Lyell, not only for his lucid teaching, but for his constant and helpful sympathy. How did he use to speak in terms of reverence of his "master," and extol the magnanimity of one who, though twelve years his senior, had abandoned slowly and cautiously, as was the habit of his mind, yet in the end completely and ungrudgingly, his own conclusions and prepossessions, and had accepted the doctrines of a pupil.

Of Darwin's three geological books, the record of the observations made by him during the voyage of the *Beagle*, it is impossible to speak in terms of praise that will seem, to those acquainted with the merits of those admirable writings, as too high; and some portions of those works, especially the chapters dealing with the great problem of foliation, are, I am convinced, very far indeed from having received from geologists the amount of attention which they deserve.

After Darwin's return to England, in 1836, his attention was for some years almost exclusively devoted to geological researches; and it was to this Society and to its officers that he constantly came for help, advice, and sympathy. He writes at this time, "If I was not more inclined for geology than the other branches of natural history, I am sure Mr. Lyell's and Lonsdale's kindness ought to fix me."

Before reaching England, Darwin had written to Henslow from St. Helena, on July 9, 1836, asking that he might be proposed a Fellow of this Society, and on November 30 of that year he was elected. In the following February he became a member of our Council, and at the next anniversary, in 1838, undertook the duties of Secretary. This office, after he had held it for five years, he was compelled to resign through ill health: but even after he had been driven from London through the same cause, it was the evening meetings of this Society which from time to time tempted him from the seclusion of Down, till at last painful experience proved to him that he must forgo even this too-exciting pleasure. Even after being compelled to lay aside his hammer, when he had taken up scalpel and microscope to study the Cirripedia, he did not forget the fossil forms of the same group.

Whether it was the distribution of organic forms in space, or the order of their appearance in time, which had had most to do in turning Darwin's thoughts into those currents which finally led him to evolution, it would be idle to speculate; but it may safely be asserted that the geological aspects of natural history had at least as much to do with the conception of the origin of species as had the biological.

How warm was Darwin's interest, all through his life, in the progress of every branch of geological research may be gathered from his letters to Lyell and other geological friends. In what he had a presentiment would be, and which actually proved, his latest work, "The Formation of Vegetable Mould through the Action of Worms," he returned in his old age to a geological problem which had occupied him during the years of his most intimate connection with our Society.

No memories can possibly have such fascination for myself as those of the conversations which, during the last seven years of his life, I was privileged to hold with Mr. Darwin upon the current topics of geological interest. It was his habit when he came to town, twice a year, to ask me to meet him, in order to talk over geological questions, and thus I had opportunities for close intercourse and discussion. No researches in our science were too minute, none too remote from the ordinary subjects of his study, to engage his attention and command his sympathies. How keenly did he recall the pleasures of his labours in this Society, and the happiness of the friendships which he had formed here! How generously and with what warmth of appreciation did he ever speak of the labours of those who had succeeded him in endeavouring to carry out the objects of this Society! Of the gentleness, the sympathy, the contagious enthusiasm of the man, I dare not trust myself to speak!

At a time when there is perhaps some danger that the excessive specialization which seems to have become a necessity in both the geological and the biological sciences, may lead to narrowness of view, restriction of aims, and petty jealousies among the workers in circumscribed departments of those sciences, it may be well to remember how Darwin, while engaged in the most minute and detailed investigations upon barnacles, earthworms, or pigeons, upon orchids, primroses, or climbing plants, could ever keep his mind open to the influence of each new discovery in every branch of geological and biological science.

The great principles which lie at the foundation of modern geology and of modern biology are the same; and Darwin did but furnish a new testament to the old covenant already accepted by geologists. Now, more than ever in the history of natural science, is there reason for the warmest sympathy, the most thorough understanding, and the completest union in effort between the cultivators of the geological and the biological sciences. It is not by petulant unfaithfulness to the tried methods of those two sciences, and a readiness to abandon the principles which have led us to such real and important conquests, for the older methods that have been so often discredited and found wanting, that we can hope to advance those sciences.

Lyell once wrote to Darwin as follows: "I really find, when bringing up my preliminary essays in 'Principles' to the science of the present day, so far as I know it, that the great outline, and even most of the details, stand so uninjured, and in many cases they are so much strengthened by new discoveries, especially by yours, that we may begin to hope that the great principles there insisted on will stand the test of new discoveries."

And to this Darwin replied with characteristic enthusiasm:—

"*Begin to hope?* Why the *possibility* of a doubt has never crossed my mind for many a day. This may be very unphilosophical, but my geological salvation is staked upon it! . . . It makes me quite indignant that you should talk of *hoping*."

Fifty years have elapsed since these words were written. How infinitely more complicated seem to us the problems involved in the explanation of the past by the study of the process going on around us at present, than they possibly could have done to the great pioneers of the Uniformitarian doctrines! But the reasons for Lyell's hope and Darwin's confidence are still valid, nay, are stronger than ever. For does not every new discovery remove some difficulty or supply fresh illustrations of these views? May every geologist to-day be endowed with a due share of Lyell's caution; but, for my own part, I see no reason why he should not also possess a full portion of Darwin's faith.

ON THE NUMBER OF DUST PARTICLES IN THE ATMOSPHERE.[1]

At the beginning of the paper, reference is made to the great advance recently achieved by physiologists, regarding our knowledge of the solid matter floating in the atmosphere, as they have already provided us with a considerable amount of information regarding the number of live germs in the air under different conditions; while we have but little information regarding the dead organic and inorganic particles. The following investigation was undertaken in the hope of bringing the physical side of the subject abreast of the physiological; and in this paper is given an account of a method devised by the author for counting the dust particles in the air, and also some results obtained by means of it.

One difficulty presented in this investigation is the extreme minuteness of the particles to be counted; most of them are

[1] Communicated by permission of the Council of the Royal Society of Edinburgh, having been read to the Abstract Society on February 6, by John Aitken, F.R.S.E.

not only invisible, but are beyond the highest powers of the microscope. It was therefore necessary to adopt some method of making them visible. The simplest plan of doing this is to put the air—the particles in which we wish to count—inside a glass receiver, and saturate it with water vapour; then to supersaturate the air by slightly expanding it by means of an air-pump. When this is done, a fog is produced in the receiver, and we know that each fog particle has a dust particle as a nucleus; if then we counted these fog particles we would get the number of the dust particles. By this process, however, we would not by any means have counted all the dust particles present, as the fog particles so formed do not represent nearly all the dust particles. If, after time has been given for these fog particles to settle, another supersaturation be made, the receiver will become packed with another set of fog particles, which would require to be counted; and this process would require to be repeated a great number of times before the last particles would become visible and be counted. It is then shown that if there is only a little dust in the air, so that the particles are wide apart, then only one supersaturation is required to make all of them visible. Further, when there are few dust particles present the fog particles are large, and are easily seen falling like fine rain inside the receiver; and it appeared that if these rain drops could be counted then the solution of the problem promised to be easy.

The following gives a general idea of the method adopted of working out this suggestion. A small glass receiver was connected on the one side with an air-pump and on the other with a cotton-wool filter. Inside the receiver was fixed a small stage, about 1 cm. square, on which the drops were to fall and to be counted. This stage was fixed at a distance of 1 cm. from the top of the receiver, it was ruled into little squares of 1 mm., and was examined through the top of the receiver by means of a magnifying glass. To illuminate this stage a gas flame was used, the light being concentrated on it by means of a globular lens full of water. The air in the receiver was pumped out, and filtered air admitted. This air was perfectly dust-free, and gave no condensation when expansion was made. Into this pure air was admitted a small and measured quantity of the air the particles in which we wished to count. After allowing a short time for the air to get saturated, one stroke of the pump was made, which supersaturated the air, and brought down a shower of fine rain; while making the stroke with the pump, the stage was carefully observed through the magnifying glass, and the number of drops that fell on a square millimetre counted. This was repeated a number of times, and the average number of drops per square millimetre was obtained, and used for calculating the number of particles in the air. For every drop that fell on the square millimetre, 100 fell per square centimetre; and as there is only 1 cm. of air above the stage that number will represent the number per cubic centimetre in the air of the receiver. Then, knowing the proportion in which the air tested was mixed with pure air, and knowing also the amount to which the air was expanded by the pump, we have all the figures necessary for making the calculation of the number of particles in the air under examination.

In constructing the apparatus the first thing to which attention was given was to design the arrangement of stage or platform on which the drops could be most easily seen and counted. The first stage tried was a small piece of glass mirror, ruled on the back into little squares. This seemed at first to give excellent results, the drops being easily seen on its surface; but on attempting to count them its unsuitableness was at once evident—the confusion produced by the reflected images of the drops caused it to be abandoned at once. Then a mirror of very thick glass was tried, the glass being so thick that the reflected images were out of focus, but it did not give satisfactory results. Very thin mirrors made of microscope glass were then tried, but had to be rejected, because, though they brought the drops and their reflections together, they were unsuitable, being too rough and covered with fine specks on their surface; only the most highly finished glass can be used for this purpose. The arrangement was then altered, and a transparent stage lit from beneath was tried. This stage was made of a small piece of carefully selected glass, and had the fine lines etched on its surfaces. It was, however, abandoned, as it did not give such promise as the mirror arrangement. All difficulties in the use of mirrors were at last got rid of by making them of silver, and now silver mirrors are the only kind used. They are very highly polished, care being taken to keep the rubbing marks in straight lines and in one direction; they are ruled with fine lines at right angles to each other and at 1 mm. apart. When a silver mirror is mounted in its place, properly adjusted and lighted, it appears, when seen through the lens, like a black surface on which the lines are quite distinct, and on which the small drops shine out brilliantly and are easily counted.

Some difficulty is experienced in keeping the stage at the proper temperature. If it is too hot, the drops on falling on it do not adhere, but present a beautiful illustration of the spheroidal condition, as they roll over its surface towards the lower side of the stage, and drop into the ruled lines, in which they continue rolling till quite evaporated. On the other hand, if the stage is too cold it gets dewed, and counting becomes impossible. Directions are given in the paper for mounting and keeping the counting stage in the best working condition.

In working the apparatus two methods have been employed of mixing the air to be tested with dust-free air. In one, the dusty air is introduced into a flask which communicates with the test receiver by means of a pipe provided with a stopcock. The small quantity of air that is to be mixed with the pure air in the receiver is displaced from this flask and driven into the receiver by means of a carefully measured quantity of water which is run into the flask. In this way the air to be tested can be measured with a fair degree of accuracy, and as the capacity of the receiver is easily obtained, the experimental errors need not be great.

In the other method of working, the test receiver is connected with a small gasometer, and the air to be tested is mixed with pure air in the gasometer. The gasometer used has a capacity of 20 litres, is carefully graduated and delicately hung, so that the air can be measured in it with a considerable degree of accuracy. In working this arrangement, 1 litre of the air to be tested is generally first mixed with 19 litres of filtered air. After mixing, nine-tenths of the mixture is let out, and the gasometer again filled up with pure air, and the mixture tested in the receiver. If the drops are still too close, more air is let out, and filtered air added till the desired condition is attained. There must not be too many particles present, or all of them will not fall when expansion is made. Till experience is gained, a check on the quantity is easily obtained by admitting filtered air, in place of the air from the gasometer, and seeing if any drops appear on expansion; if none, then the correct number has not been exceeded.

After a satisfactory counting stage had been devised, and the apparatus got into working order, testing began, when at once difficulties presented themselves. The numbers counted in the successive tests of the same air agreed fairly well for a number of times, then all at once the process seemed to break down, and from time to time a great increase in the number was counted, far exceeding the errors of experiment; then all would go right for a time, but only to be followed by failure before long. The first thing suspected for these and for other failures was always the joints of the pipes and the stopcocks, and time after time have the joints been remade with india-rubber solution and stopcocks cleaned and greased, but to find that they are almost never at fault.

It was then suspected that the failure might be due to the filtered air, with which we mixed the dusty air, not being perfectly freed from its dust. The filtering power of cotton-wool was therefore studied, when it was found that 1 inch of cotton-wool will filter perfectly if the air is passed very slowly through it, but that even 12 inches of cotton-wool will not check all the particles if the air is made to rush violently through it. Filters must therefore be tested under exactly the conditions in which they are to be used.

It was, however, found that though the air was only allowed to pass very slowly through even 12 inches of cotton-wool, condensation frequently took place if the expansion and therefore the supersaturation was great. It was thought that in this case the failure might be due to an imperfect action of the filter—that, while it checked most of the dust, yet it allowed the extremely small particles to pass, and that these extremely small particles became active centres of condensation when exposed to the high degree of supersaturation used in the tests. It therefore here became necessary to test whether the *size* of the particles has *practically* any effect on the degree of supersaturation necessary to cause the vapour to condense on them. From the investigations of Clerk Maxwell we have theoretical reasons for expecting that the size of the particles will have an influence of this kind, but at present we cannot say that it is sufficiently great to have a perceptible effect in an experiment such as that described.

To test this point the following experiment was therefore made. A little dusty air was mixed with filtered air, and put into the test receiver, and saturated with water vapour. An expansion of only 2 c.c. was made; this caused the formation of a fog. After these fog particles had settled, the air was returned to the receiver; and after a short time another 2 c.c. expansion was made, when other fog particles appeared. After this had been done a number of times, the density of the fog got less and less, and at last entirely ceased. After this an expansion of 5 c.c. was made; this produced a rainy condensation in the receiver, which appeared a number of times on successive expansions being made, getting less and less dense, and at last it also ceased entirely. After all condensation had stopped with the 5 c.c. expansion, the expansion was increased to 10 c.c., when another shower made its appearance, and after one or two expansions all condensation again ceased. After this condition was attained, an expansion of 150 c.c. was made with the pump, when scarcely one drop made its appearance.

It is concluded that in the above experiment we have distinct evidence that the *size* of the particle does affect the degree of supersaturation required to produce condensation on it. Because, though an expansion of 2 c.c. produced a supersaturation sufficient to cause more than one-half of the particles to become visible, yet it required a higher degree of supersaturation to cause condensation to take place on others. It is also concluded from the experiment that the failure of the air to keep clear, in the tests where high supersaturation was used, was not due to the presence of extremely small particles, as an expansion of 10 c.c. is practically great enough to produce a supersaturation sufficient to cause condensation on the smallest particles.

The failures in the tests not being due to the presence of extremely small particles, it is concluded that they are true cases of condensation without nuclei, similar to those referred to in a previous communication. It was thought that, if they were true cases of spontaneous condensation, they might be checked if the expansion was made slowly and free from shocks. And on the other hand any shock would tend to produce condensation in dust-free air if highly supersaturated. On trying this, it was found that no condensation took place if the stroke of the pump was made slowly and steadily, and that if done quickly, and the piston made to strike the cover of the cylinder violently, then copious showers were always produced in the dust-free air. Here, then, was the key to one of the difficulties, and accounted for the occasional increase in the number of the particles counted; many of the drops having no dust-nucleus. Failure from this cause is now entirely prevented by causing the air on its passage from the receiver to the pump to pass through a small opening, or better through a small cotton-wool filter; this checks all violent rush of air, and shocks, and keeps the filtered air perfectly free from condensation even when highly supersaturated.

Again, the failure of perfectly filtered air to keep free from condensation was frequently observed after the inside of the test receiver had been newly wetted. It looked as if the newly wetted sides had saturated the air more thoroughly, and that the condensation was due to the higher degree of supersaturation which took place when expansion was made. This class of failures was, however, traced to the manner of wetting the inside of the receiver. If it was done roughly, and the water splashed, then many nuclei were manufactured in the receiver; if it was done quietly, none, and no condensation followed. Another cause of failure was traced to a drop of water getting into the pipe by which the air entered, and the inrush of air tearing the water into fine spray, which became active centres of condensation.

As yet no great number of tests of air have been made under different conditions, natural or artificial; but in the following table will be found some of the results obtained by this method of counting.

Dust Particles in the Atmosphere.

Source of the Air.	Number per c.c.	Number per c.in.
Outside Air—Raining ...	32,000 ...	521,000
,, ,, —Fair ...	130,000 ...	2,119,000
Room	1,860,000 ...	30,318,000
,, near ceiling ...	5,420,000 ...	88,346,000
Bunsen Flame ...	30,000,000 ...	489,000,000

In the first column of the table is entered the source of the air; in the second, the number of particles per cubic centimetre; and, for the benefit of those who think in English measures, the number per cubic inch is entered in the third column. The first number in the table, for ordinary outside air, was obtained on January 25, after a wet night. The number given for fair weather is an average got when the weather was in that condition. As yet far too few measurements have been made to enable us to trace any connection between the number of particles and the weather, but it is hoped that something practical may result from observations of this kind. The first number given for the air in a room is the number counted in the air of a room where gas was burning, and taken at a height of 4 feet from the floor; the other number was counted in air drawn from near the ceiling; and the last number was got in the air collected over a bunsen flame.

The value of numbers given in the table has not been carefully considered, and they are not given as absolutely correct; great accuracy, indeed, does not seem possible, when we consider the conditions; and, further, the number is constantly varying. For this reason it has not been thought necessary to make any corrections for temperature and pressure. Though we can get with a fair degree of accuracy the number of particles in the air in the test receiver, yet in all probability the calculated numbers given in the table are rather under than over estimates, as it is difficult to manipulate air without losing much of its dust. For instance, in one hour about one-half of the particles settle out of the air in the gasometer. Though the numbers do seem very large, yet so far as can be judged at present they are fairly correct, and at least represent the kind of numbers we have to deal with. It does seem strange that there may be as many dust particles in 1 cubic inch of the air of a room at night when the gas is burning as there are inhabitants in Great Britain, and that in 3 cubic inches of the gases from a bunsen flame there are as many particles as there are inhabitants in the world.

JOHN AITKEN.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Frank Smart Studentship of Botany, founded at Gonville and Caius College by Mr. F. G. Smart, M.A., M.B., and Mrs. Smart, by the transfer of £2400 Great Eastern Railway 4 per cent. Debentures, is to be awarded for the first time at the beginning of Easter Term. Candidates are to send in their names to the Master of the College, Dr. Ferrers, on or before March 20. The electors are the governing body of the College, acting after consultation with the Professor and the Reader of Botany for the time being in the University. The Studentship is to be open to all members of the University who have taken honours in the first part of the Natural Sciences Tripos, and of not more than five years' standing; but the elected Student must become a member of Gonville and Caius. No competitive examination is to be held for awarding the Studentship. The Student is to apply himself to original investigation in botany, and must be able to show that he is doing so at any time, on penalty of forfeiting the Studentship. The Studentship is to last two years, but may under special circumstances be prolonged for one year more. The regulations of the Studentship are only to be changed, after the death of Mr. and Mrs. Smart, by consent of the Council of the Linnean Society of London. A prize of £6 in books is to be given out of the interest of the fund to the undergraduate student of Caius College who shall distinguish himself most in botany at the annual College examination.

The collection of British birds' eggs made by the late Mr. J. P. Wilmot, of Trinity College, containing a specimen of the great auk's egg, and other specimens figured in Hewitson's "British Oology," has been presented to the University by Lady Caroline and Mr. C. H. Russell, in memory of Mr. George Lake Russell, Lady Caroline's late husband.

Plans are submitted for the proposed new plant-houses at the Botanic Garden, to cost £2760, and of a laboratory in the garden, to cost £250.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 9.—"The Small Free Vibrations and Deformation of a Thin Elastic Shell." By A. E. H. Love, B.A., Fellow of St. John's College, Cambridge.

In this paper the method employed by Kirchhoff and Clebsch for the treatment of a thin plane plate is applied to the case of a

thin shell, or plate of finite curvature. It is proved (1) that only for an inextensible spherical surface is the potential energy-function the same function of the changes of principal curvature as for a plane plate; (2) that in general the shell cannot vibrate in such a manner that no line on the middle-surface is altered in length, because this condition makes it impossible to satisfy the boundary conditions which hold at a free edge; (3) that surfaces of uniform curvature with no bounding edges are the only ones which admit of purely normal vibrations; and (4) that vibrations in which the displacement is purely tangential are possible on all shells whose middle-surfaces are surfaces of revolution bounded by small circles. The possible modes of vibration of the spherical and cylindrical shell receive special discussion. The equilibrium of the shell is also considered.

Linnean Society, February 2.—Mr. Carruthers, F.R.S., President, in the chair.—The President called attention to the loss which the Society had sustained by the deaths of Prof. Asa Gray, Prof. Anton De Bary, and Mr. Irwine Boswell (formerly Syme) which had occurred since the date of the last meeting, and gave a brief review of the life and labours of each.—Mr. C. T. Druery exhibited a collection of abnormal British ferns, and made some remarks on the extraordinary number of named varieties which had been recognized, and which now required to be carefully examined and compared, with a view to some systematic arrangement of them. A discussion followed, in which the President, Mr. J. G. Baker, F.R.S., Dr. Murie, and others took part.—Dr. Amadeo exhibited and made some observations on a new species of *Tabernæmontana*.—A long and interesting paper was then read by Mr. Henry T. Blanford, F.R.S., on the ferns of Simla, based upon a collection which he had himself made there, "not much below 4500 feet, nor above 10,500 feet." His remarks were illustrated by a map, and by an exhibition of a number of the more noticeable ferns collected, many of which were extremely beautiful. Criticisms were offered by Mr. C. B. Clark, F.R.S., Mr. Gamble (Conservator of Forests, Northern Circle Madras) and Dr. William Schlich (Inspector-General of Forests to the Government of India).—A paper was then read by Mr. H. J. Veitch, on the fertilization of *Cattleya labiata*, var. *Mossiæ*, in which the author detailed an elaborate series of observations undertaken with the object of detecting, if possible, the act of fertilization of the ovules, to determine the time that elapses between pollination and that event, and to trace the development of the ovules into perfect seeds. After explaining the structure of the sexual apparatus of *Cattleya labiata* with the aid of drawings showing the separate parts, the processes following pollination were dealt with, first from the development of the rudiment into the perfect ovule, and then the ripening of the ovules into seeds, these processes being also illustrated by drawings made at particular stages. A discussion followed, in which Mr. J. G. Baker, Mr. H. N. Ridley, and others took part, and to their inquiries for further particulars Mr. Veitch replied.—The next paper, by Mr. Joseph S. Baly, contained descriptions of new species of *Galerucinæ*, and being of a technical character was taken as read.

Entomological Society, February 1.—Dr. D. Sharp, President, in the chair.—The President nominated Sir John Lubbock, Bart., M.P., F.R.S., Mr. Osbert Salvin, F.R.S., and the Right Hon. Lord Walsingham, F.R.S., Vice-Presidents for the session 1888 to 1889.—Mr. F. Pascoe exhibited a species of the Hemipterous genus *Ghilianella*, which he found at Pará with the young larva. He said it was the only occasion he ever saw the species with the larva, which was new to Mr. Bates.—Dr. Sharp exhibited some insects collected by Mr. A. Carson on Kavalla, an island in Lake Tanganyika. The Coleoptera were nearly all well-known species, exemplifying the fact that many of the commoner insects of tropical Africa have wide distribution there, some of these species being common to both Natal and Senegal. The most remarkable of the insects was a large Lepidopterous caterpillar.—Mr. Champion exhibited specimens of *Casnonia olivieri*, *Œdichirus unicolor*, *Paussus favieri*, *Colydium elongatum*, *Endophlœus spinulosus*, *Hetærius arachnoides*, *Pseudotrechus mutilatus*, *Singilis bicolor*, *Phyllomorpha laciniata*, recently collected by Mr. J. J. Walker, R.N., at Gibraltar, Tetuan, and Tangier.—Mr. R. South exhibited a remarkable variety of *Polyommatus phlœas*, caught by him in North Devon in 1881.—Mr. R. W. Lloyd exhibited a living specimen of a species of *Ocnera* from Ispahan.—Mons. A. Wailly exhibited, and read notes on, a number of cocoons of *Antheræa assamensis*, *A. roylei*, *Actias selene*, and *Attacus ricini*, lately received from Assam; also a number of nests of cocoons of *Bombyx rhadama*—the silk of which is used by the Hovas in the manufacture of their stuffs called "Lambas"—from Madagascar.—Captain H. J. Elwes read a paper on the butterflies of Sikkim, the result of many years' collecting. He said he had been enabled to complete his observations during the enforced delay at Darjeeling of Mr. Macaulay's Mission to Thibet, of which he was a member. He stated the number of species occurring in this district to be about 530, which is greater than the number hitherto found in any other locality in the Old World. Of these the greater part only occur in the hot valleys at an elevation of 1000 to 3000 feet, and these are for the most part of a purely Malayan character, whilst those found in the middle zone are in many cases peculiar to the Himalayas; and the few species from the alpine parts of the country at 12,000 to 16,000 feet are of a European or North Asiatic type. An important feature in this paper was the numerous observations taken on the habits, variation, seasons of appearance, and range of altitude of the various species, for which the author said he was largely indebted to Herr Otto Möller, of Darjeeling. The paper concluded with an analysis of the species and genera compared with those found in the North-West Himalayas and in the Malay Peninsula. Mr. J. H. Leech, Dr. Sharp, Captain Elwes, and others took part in the discussion which ensued.

Zoological Society, February 7.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's menagerie during the month of January.—Mr. E. G. Loder exhibited and made remarks on a very large African Elephant's tusk, which weighed 180 pounds, and was, as he believed, the largest tusk hitherto authentically recorded.—Mr. A. Thomson exhibited a living specimen of the larval form of Stick-Insect (*Empusa pauperata*) from the Insect House.—Mr. G. A. Boulenger, read the third of his series of contributions to the herpetology of the Solomon Islands. The collection now described have been obtained by Mr. C. M. Woodford during a visit to the islands of Guadalcanar and New Georgia. The author observed that though the collection contained over 200 specimens, only four species were thereby added to the herpetological list of the Solomons, showing that our knowledge of that fauna was approaching completion.—A communication was read from Mr. Arthur G. Butler, containing descriptions of some new Lepidoptera from Kilima-njaro. Some of the specimens described had been collected by the late Bishop Hannington, and others by Mr. F. J. Jackson.—Mr. Frank E. Beddard read a paper upon certain points in the visceral anatomy of the Lacertilia. The paper dealt principally with *Monitor*, in which the presence of a peritoneal fold covering the abdominal viscera and separating them from the lungs was referred to; this membrane was compared with a corresponding structure in the Crocodilia.—Mr. D. D. Daly gave an account of the Birds'-nests Caves of Northern Borneo, of which no less than fifteen were now known to exist in different parts of the North Bornean Company's territories. Most of these were situated in limestone districts in the interior, but two of them were in snadstone formations near the sea-coast.—A communication was read from Mr. R. Bowdler Sharpe, containing the description of a new species of Tyrant-bird of the genus *Elainea*, from the Island of Fernando Noronha. This was proposed to be called *E. ridleyana*, after Mr. H. N. Ridley, who had obtained the specimens during his recent exploration of that island.—Mr. Osbert Salvin, F.R.S., read a note on *Ornithoptera victoriæ*, from Guadalcanar Island of the Solomon Group, and pointed out the characters which separated this species from a closely allied form of the Island of Maleite, proposed to be called *O. reginæ*.

Geological Society, February 8.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—On some remains of *Squatina cranei*, sp. nov., and the mandible of *Belonostomus cinctus*, from the Chalk of Sussex, preserved in the collection of Mr. Henry Willett, of the Brighton Museum, by Mr. A. Smith Woodward.—On the history and characters of the genus *Septastræa*, D'Orbigny (1849), and the identity of its type species with that of *Glyphastræa*, Duncan (1887), by Dr. George Jennings Hinde.—On the examination of insoluble residues obtained from the Carboniferous Limestone at Clifton, by Mr. E. Wethered.

Royal Microscopical Society, February 8.—Annual Meeting.—The Rev. Dr. Dallinger, F.R.S., President, in the chair.—The Report of the Council was read, showing a further increase in the number of Fellows, and in the revenue of the

Society.—Mr. Crisp referred to the great loss the Society had sustained by the death of Dr. Millar, who had always taken a lively interest in the affairs of the Society, and for nearly thirty years had been a member of Council.—Dr. Dallinger delivered his annual address.

PARIS.

Academy of Sciences, February 20.—M. Janssen in the chair.—Third note on the doctrine of probabilities as applied to target practice, by M. J. Bertrand. The object of this paper, which has been prepared at the request of several artillery officers specially interested in the subject, is to present in a form capable of immediate application the results already arrived at as set forth in the previous communications.—On the species of Proneomenia on the coast of Provence, by MM. A. F. Marion and A. Kowalevsky. In a previous note the authors described a new genus of Solenogaster from the Gulf of Marseilles differing from the Proneomenia by its thorny integument. Here he describes four distinct species of the genus Proneomenia which occur on the coast of Provence, and which present features by which they may be readily distinguished from *P. sluiteri* described by Hubrecht. These species, none of which exceed 15 mm. in length, are respectively named *P. vagans*, *P. caulini*, *P. desiderata*, and *P. aglaophenicæ*. Incidental reference is made to a fifth species (*P. gorgonophila*) discovered on the coast of Algeria.—Observations of the new planet Charlois, 272, made at the Observatory of Algiers with the 0·50 m. telescope, by MM. Rambaud and Sy. The observations for right ascension, declination, apparent position, &c., extend over the period February 10–11.—Observations of the same planet are also recorded for February 8–13 made at the Observatory of Marseilles with the Eichens equatorial, by M. Borrelly.—Permanent deformations and thermodynamics (continued), by M. Marcel Brillouin. The chief subjects here discussed are the principle of equivalence, specific and latent heats, and the differential relations between the specific heats.—On the electrostatic attraction of electrodes in water and attenuated solutions, by M. Gouy. The theory of the propagation of electricity in the permanent state suggests the presence of free electricity during the passage of the current, not only on the outer surface of the conductors, but also on the surface separating two conductors of different specific resistance, the electric force necessarily having different values on either side of this surface. The author here endeavours to ascertain whether this hypothetic layer of free electricity on the contact surface might be capable of exercising any electrostatic actions. For this purpose he studies the case of two metallic conductors placed in a moderately conducting liquid and maintained by a pile with different potentials, in order to determine how far they may be acted upon by appreciable forces. His experiments seem to show that these forces really exist, and are in fact much more considerable than could have been foreseen.—On the coefficients of proportionality in radiating heat, by M. L. Godard. The experiments here described seem to show that the coefficients of proportionality given by the study of the diffusion of heat, and confirmed by the spectro-photometric analysis of coloured substances, are the same as the numbers obtained by M. L. Mouton in his researches on the distribution of heat in the normal spectrum of the sun.—Preparation and properties of a bi-hydrofluate and of a tri-hydrofluate of fluoride of potassium, by M. H. Moissan. While hydrochloric acid yields with difficulty the hydrochlorates of chlorides, hydrofluoric acid combines readily with the neutral fluorides to produce hydrofluates of the general formula KFl, HFl. But these compounds, including 1 equivalent of hydrofluoric acid, are not the only ones that may be obtained, at least with the alkaline metals. The author has succeeded in preparing two new combinations containing 2 and 3 equivalents of acid for 1 of fluoride of potassium. These ombinations, abounding in hydrofluoric acid, and capable of being kept in the fluid state at temperatures ranging from 65° to 105° C., may perhaps under certain conditions enable the hydrofluoric acid to react readily on a certain number of organic or mineral compounds.—On a new reagent of the products of saponification of cotton-oil, by M. Ernest Milliau. The chemical reagent here described, which is not observed in the fatty acids of olive-oil, is so sensitive that by its means the presence may easily be detected of 1 per cent. of cotton-oil in olive-oil. All risk of error is removed, as the operation is effected, not on the oil itself, but on the fatty acids free from all impurity. Science has thus supplied the long sought-for means of infallibly detecting any adulteration of olive-oil by cotton-oil in the proportion of from 5 to 20 per cent., as is usually practised in the trade.—On the essence of lavender, by MM. R. Voiry and G. Bouchardat. The results of the analysis of this essence differ in some respects from those hitherto published. The authors have determined the presence of an oxygenated compound identical with eucalyptol, and the almost complete absence of carburets of hydrogen.—The sardine fisheries on the west coast of France in 1887, by M. Georges Pouchet. Last year was characterized by an extreme abundance of sardine on the French fishing-grounds, at the very time when the most opposite reasons were being advanced to account for a supposed gradual disappearance of the species from the French waters. On this point nothing positive can be asserted in the absence of any accurate knowledge of the migrations and spawning-grounds of the sardine.—On the Quaternary station of La Quina, Charente, by M. Emile Rivière. This station of prehistoric man, which lies near the banks of the Voultron in the Canton of La Valette, has recently been carefully explored by the author, who agrees with M. Chauvet in assigning it to the Mousterian (reindeer) epoch. The animal remains include the cave-bear, jackal, wild cat, horse, *Bos primigenius*, *Cervus elephas*, and especially the reindeer, in great abundance. No human bones were found, but there is an abundance of chipped flints, some very fine, and evidently worked on the spot.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Navigation and Nautical Astronomy: W. R. Martin (Longmans).—The Method of Creation: H. W. Crosskey (Sunday Sch. Assn.).—Elementary Physiography: J. Thornton (Longmans).—Life in Corea: W. R. Carles (Macmillan).—Discursive Essays on the Phenomena of the Heavens and Physical History of the Earth, Part 1 (London Literary Society).—Technological Dictionary, 3 vols., English, German, and French: Röhrig and Schiller (Trübner).—Emin Pasha in Central Africa (Philip).—Das Antlitz der Erde, vol. ii.: E. Suess (Tempsky, Wien).—Jahrbuch der k. k. Geologischen Reichsanstalt, Jahrg. 1887, xxxvii. Band, 2 Heft; Abhandlungen der k. k. Geologischen Reichsanstalt, Jahrg. 1887, xi. Band, 2 Abthg. (Wien).—Industrial Instruction: R. Seidel (Heath, Boston).—The Manual Training School: C. M. Woodward (Heath, Boston).—A Pocket-book of Electrical Rules and Tables, 5th Edition: Munro and Jamieson (Griffin).—ii. Jahresbericht (1886) der Ornithologischen Beobachtungstationen im Königreich Sachsen: Dr. A. B. Meyer and Dr. F. Helm (Dresden).

CONTENTS.

THURSDAY, MARCH 8, 1888.

PHYSICAL SCIENCE AND THE WOOLWICH EXAMINATIONS.

WE are interested to learn that the views we have expressed on this subject are probably shared by representatives of military opinion; for we are informed that the treatment of scientific candidates for Line cadetships, under the similar regulations for admission to Sandhurst that were introduced in 1884, met with a very unfavourable reception from at least one of the service journals. At the time of their introduction, the *Army and Navy Gazette* pointed out, as we have done, the serious objections that exist to giving modern languages so great an ascendency as is allotted to them in the present Sandhurst competitions. All that was said on this subject in 1884 applies with much greater force to the proposed mode of selecting officers for the scientific branches of the Army. We do not underrate the value of modern languages to soldiers, or to any other class, but an education in which mathematics and modern languages occupy so dominant a position as they are likely to possess in the education of many of the successful Woolwich cadets of the future [1] is scarcely more defensible than would be the adoption now of the purely classical training of former years. We trust, therefore, that no pains will be spared by those who are interested in this question to further the efforts that are being made to bring about the adoption of a more liberal scheme, which shall encourage early specializing on the part of the candidates to a less degree, and be more just to the particular class whose claims we have urged.

These regulations seem calculated to perpetuate the system of education of which it has been repeatedly complained that "it has too much to do with books and too little to do with things"; and, apart from their unfairness, they will tie the hands of those head masters who are willing, or even anxious, to adapt the work of their schools to the needs of the times, by forcing upon them a narrow curriculum of which they do not approve. This is not only unfortunate but unnecessary, for there is no real obstacle in the way of formulating a scheme of examination that shall both give fair play to all the candidates, and leave the hands of the teachers comparatively unshackled.

Much as the claims of science are still underrated by the unthinking among us, it was hardly to be expected that the representatives of a scientific profession would sanction regulations which will tend to prevent the admission to that profession of youths of scientific power, and which are also calculated to discourage any element of science teaching in the previous education of those who may wish to join it. Complaints of the absence of such training are familiar enough, and regulations intended to encourage such preliminary work are not uncommon. This adds not a little to our surprise at the proposals of the War Office Committee. We regret to perceive in them a fresh illustration of the tendency of Examining Boards to sacrifice the interests of the examined to a desire for simplicity in their schemes of examination, a tendency that constitutes a source of serious danger to proper freedom of education in these days, when admission to all the higher avocations is so jealously guarded by competitive or qualifying examinations.

In the discussion of this subject that has occurred in our columns a statement has been made, and repeated, by one of our correspondents, that requires notice. We allude to the contention that chemistry, physics, and geology are not good educational subjects for boys under sixteen years of age. This is a statement with which very few who have given these sciences a fair trial will agree; moreover, it is not pertinent to the question under discussion. Successful candidates for the Woolwich cadetships are, we believe, on an average, not much less than seventeen and a half years of age, and in future the average of age is more likely to rise than to fall in consequence of the increased severity of the examination in obligatory mathematics. No liberal-minded man will deny that the above-named sciences are exceedingly good educational subjects, between the ages of fifteen and seventeen and a half years, in the case of those who have ability and liking for them, by whom alone they will as a rule be studied among the candidates for Woolwich. Of course there are some for whom such studies are unfitted, but we very much doubt whether the military authorities will greatly regret the rejection of such as these. Their powers are likely to be more profitably employed in other directions.

If we may judge from the memorandum lately issued with the Army Estimates by Mr. Stanhope, we may conclude that the present time affords a good opportunity for urging upon his notice the thoroughly unpractical character of the proposed changes. The frankness with which Mr. Stanhope admits other deficiencies in the system of our military administration encourages the expectation that in this matter also he will act with an equal degree of practical sense, and that it will not be long before we shall hear that the efforts of those who have taken up this matter are bearing fruit.

[1] Since mathematical and modern languages will count for 12,000 marks out of a maximum of 16,500, and as about 5000 will be sufficient for success in future, it is not unlikely that many candidates may deem it safest or easiest to almost confine their studies to these two branches.

PROFESSOR FLEEMING JENKIN.

Papers, Literary, Scientific, &c., by the late Fleeming Jenkin, F.R.S., LL.D. Edited by Sidney Colvin, M.A., and J. A. Ewing, F.R.S. With a Memoir by Robert Louis Stevenson. 2 vols. (London: Longmans, Green, and Co., 1887.)

THIS is a work of great interest to many classes of scientific men, as well as to the public at large. Its contents are of an extremely varied character. Readers of NATURE, as such, are not deeply concerned with discussions of Female Dress in ancient Greece, with Rhythm in English verse, or with the characteristics of Mrs. Siddons as an actress. Nor will they, as a body, care much for the merits and demerits of Trade Unions, the relations of Supply and Demand, or other branches of the would-be science called Political Economy. The literary and economic Journals, on the other hand, will probably regard these as among the more valuable contents of these volumes.

But the Biographical Sketch of Prof. Jenkin is of high interest to all:—first, because it traces the successive advances made by the indomitable perseverance of a brave man in his protracted struggles against difficulties of no common order:—secondly, because it is the work of one of the most remarkable writers of our time, who has thus given fresh proof of the versatility of his genius. The result, however, cannot we think be looked on as wholly satisfactory by those who really knew Prof. Jenkin. The power displayed in the narrative is unquestionable, the various characters stand forward in clear-cut outline, and we seem to see them act out their lives before us as we read. But the weird imagination of the writer has proved too much for him, and some of his "situations" are altogether overcharged.

The late Prof. Jenkin was essentially a frank, straightforward, hard-working, clear-headed, practical scientific man:—and it is in this capacity that he will be held in honourable remembrance by the scientific world. What was the character of his grandmother, or what forms of relaxation he himself sought from study or business, are matters of infinitely less importance. Scientific men would have been glad to learn many things not mentioned here:—*e.g.* the secret of his singularly methodical management of complicated correspondence:—for it is in such matters that they are, as a rule, most sorely tried as well as most miserably inefficient. But his Biographer is a true Artist, for whom business, method, and even science itself have no attractions; except in so far as they may serve occasionally to heighten the lights or to darken the shadows of an ideal picture. And it must be acknowledged that Mr. Stevenson has, in a very remarkable degree, succeeded in the work *as he understood it;* viz. in tracing the behaviour of that wholly unscientific (and therefore imaginary) structureless germ which renders "the biography of the man . . . . only an episode in the "epic of the family."

We are introduced at starting to a powerful but repulsive sketch of a family of country bumpkins, sots and sorners:—culminating in a weak but handsome and well-meaning midshipman of "a simplicity that came near to "vacancy." He married, in the West Indies, the daughter of a somewhat lively lady who "would tie her house slaves "to the bed and lash them with her own hand." Of the daughter we are told that, on occasion, she exhibited "characteristic barbarity." The domestic fate of the poor midshipman can of course be foreseen. "His wife, "impatient of his incapacity and surrounded by brilliant "friends, used him with a certain contempt. She was the "managing partner; the life was hers, not his; after his "retirement they lived much abroad, where the poor "Captain, who could never learn any language but his "own, sat in the corner mumchance; and even his son, "carried away by his bright mother, did not recognize "for long the treasures of simple chivalry that lay buried "in the heart of his father."

Such, we are told, were the parents of Professor Jenkin. Now we would ask in all earnestness *cui bono?* What class of readers is likely to be the better of such information as this? Surely such things, if such there were, ought to have been passed over in silence, or at least reserved to adorn, incognito, a new sensational narrative of the Jekyll and Hyde, or Dynamiter, type!

This powerful but cold-blooded description of monsters, and their atrocities, is succeeded by another quite as realistic whose *motif* is the struggle for existence on the part of the impecunious parents. Here, however, we find some relief in the frank boyish letters from young Jenkin, describing to an old school-fellow what he saw in Paris in the memorable days of 1848. For a few pages the merciless scalpel is allowed to remain inactive:—only to be applied again with fresh vigour, but now to Prof. Jenkin himself. All who knew him were aware that in the course of his singularly errant career he had lived much and happily with rough working men, and that he had made no great efforts to acquire that artificial veneer of "manners" (as it is called) which often serves the vilest of our race as a passport into "Society." But surely a single sentence on the subject would have sufficed any reasonable biographer! Why this Pre-Raphaëlite minuteness and copiousness of detail, except to add to the miserable heap of "Things one would rather not have said"? We gladly leave this aspect of the book with the remark that it affords fresh proof that literary men, even of the highest rank, are not necessarily qualified to be writers of biography, specially of scientific biography.

But there are statements of a darker stamp, such as in fact tend to impeach the sterling honesty which was one of the prominent features of Jenkin's character. The Biographer's story of his Class Certificate in Engineering will, we are certain, find no credence with any one who knew Prof. Jenkin. Under the conditions stated, nothing worthy the name of certificate could have been given by him. The story is susceptible, however, of an easy explanation. The Biographer has already told a similar tale of himself regarding his relations to another of the Edinburgh Professors. We have therefore only a recurrence of one of those half sportive, half serious, fits of introspection which form part of his literary art. Still, we do not like to meet with such things in such a connexion.

An exceedingly interesting and graphic chapter gives, in Jenkin's own words, a sketch of the busy times he had in laying and lifting submarine cables in the Mediterranean and in the Atlantic. His capacity for hard work, and his readiness of resource, appear at once from this singularly modest narrative. Appended to the Biography we have a brief but comprehensive summary of Jenkin's electrical work, drawn up by Sir William Thomson. From this we cannot make extracts. It must be read as a whole. Col. Fergusson has added an excellent sketch of Jenkin's services to general sanitation. Had Jenkin done nothing but this, his name would still be well worthy of remembrance as that of a signal benefactor of humanity.

The other contents of these volumes, so far as they can be discussed here, consist of reprints of some of Jenkin's published papers. Particularly interesting and valuable are two from the *North British Review:*—the first on *Lucretius, and the Atomic Theory*, the second on *Darwin, and the Origin of Species*. Both have important bearings on questions at present prominently before the public, so that it is specially convenient to have them in this easily accessible form. From the second we quote but a single sentence, of itself quite sufficient to confirm the above statements:—

"Any one of the main pleas of our argument, if "established, is fatal to Darwin's theory."

This is not, as some might hastily suppose, the self-laudation of a flippant "paper-scientist"; it is the deliberate statement of a clear-headed man who took nothing for granted, and who never wrote on anything till he felt convinced that he understood it.

We next come to a thoroughly practical Essay on *Scientific and Technical Education*, a subject on which Jenkin was peculiarly qualified to speak. The following extract may be taken as a specimen. Jenkin has been alluding to the willing and valuable assistance which a Teacher often receives from his higher practical students in conducting some new research; and proceeds to say:—

"The rank and file—the ordinary well-meaning student "who will never become a leading light in science—is "worthy of our attention. If he is well educated he may "become a successful manufacturer, contractor, engineer, "or farmer, and sensibly increase the power and wealth "of our country. It seems to me that this student is not "so well provided for in our scientific teaching as is "desirable. And the main question I propose for dis- "cussion is, how we are to improve the education of this "second-best young man. My own answer put briefly, "is that we can teach him systematically the art of "measurement. We cannot give him the hunger for "knowledge, the acute logical discrimination, nor the "imaginative faculty required for research; but we can "teach him how to ascertain and record facts accurately; "we can bring home to him the truth that no scientific "knowledge is definite except that based on the numeri- "cal comparison which we call measurement; we can "teach him the best modes of making that comparison "in respect of a vast number of magnitudes, and in teach- "ing this we shall teach him to use his hands and eyes. "This practical teaching gives clear conceptions to the "minds of many who receive a verbal definition as a "mere string of dead words. I should be glad if it were "generally proclaimed that the elementary training in all "our science laboratories should be a training in the art "of measurement. I wish that the classes were called "measurement classes. Then a student of ordinary in- "telligence would know that by entering a given class he "would learn how to measure those magnitudes with "which he will have to deal in after life. The attempt to "measure them will lead him to consider their nature, "and he will approach scientific study in the class room "with a faith in the reality of science which no verbal "exhortation will ever give him. You may define the "absolute unit of electrical resistance as accurately as "you will, and your definition shall affect the average "brain to no perceptible extent; but a young man of "very ordinary education and intelligence can learn to "measure resistances in ohms, and having learnt this, an "ohm becomes a reality to him. Not only does the "knowledge he has acquired make him a more valuable "assistant to the engineer and contractor, but having "acquired a working faith in the existence of ohms, he "is prepared to take some trouble to understand the "scientific definition."

Prof. Ewing reprints in full, in the last division of the work, three characteristic papers selected from Prof. Jenkin's writings on *Applied Science*:—and he gives in brief but clear abstract, and with full references, the contents of all. This part of the work seems to be very well done, and it forms a sort of commentary on, as well as complement of, the short article of Sir W. Thomson's to which we have already alluded. These handsome volumes will be specially welcomed by practical scientific men, but as we said at starting, there is much in them of interest and value to all.

P. G. T.

OIL ON TROUBLED WATERS.

Le Filage de l'Huile. Par le Vice-Amiral G. Cloué. (Paris: Gauthier-Villars, 1887.)

THAT the great effect produced by oil in smoothing troubled waters should have been so well known in times past as to have passed into a proverb, and yet that no general practical use of this effect should have been made until the last few years, is a remarkable instance of the tardiness of mankind to apply the benefits that natural phenomena provide. To the Hydrographic Office of the United States is mainly due the credit of bringing into prominence, and forcing on the notice of seamen, in various publications, the great importance of this property of oil under circumstances when life and property are endangered by breaking seas, and the extreme facility and trifling expense of its employment. Thanks to the efforts of the Americans, the facts are now well known to all English-speaking mariners, and many are the instances of the successful use of oil; but, nevertheless, the prejudices of many are still against it.

The Admiralty, in 1886, issued a memorandum on the subject to the fleet, largely founded on the American publications. Admiral Cloué has done a like service for his countrymen, and has written the best and most complete essay on the subject, in the little *brochure* before us. Drawing on the mass of experiences collected by the American Office, and giving them due credit for their action, he reports additional striking cases which have occurred during the last year or two, and suggests many practical means of employing oil under circumstances other than those yet tried, or where it has to some extent failed.

The facts are briefly these. In the heaviest gales at sea, when breaking seas are a source of danger to small or heavily laden vessels, or an inconvenience and discomfort to larger or more seaworthy ones, a very small quantity of oil, skilfully applied to suit the circumstances, spreads upon the surface of the water with marvellous rapidity, and forms a perfect breakwater, the raging waves being instantaneously transformed into a harmless swell, which quietly lifts the ship without any of the violent shocks and blows caused by the impact of an almost wall-like mass of water about to break. Spray alone comes on board in place of the sheets of water and green seas which often do so much damage. Admiral Cloué calculates, from a number of instances where the quantity of oil used and the speed of the vessel are given, that the film of oil which causes this marvellous and beneficent effect can be little more than 1/100,000 of a millimetre in thickness!

Experience already goes to show that a small quantity of oil is more efficacious than a free application of it, the film apparently spreading more quickly. Less than half a gallon an hour seems to secure the largest ship from being boarded by the waves.

The ordinary method of its application is to hang small canvas bags, containing about a couple of gallons of oil, so

as to dangle or float on the water, the bags being pierced with small holes by a sail needle, through which the oil slowly exudes. These bags are placed in different positions, according to whether the ship is flying before the tempest, or lying-to comparatively motionless. This simple appliance is therefore within the means of every ship, and there can be little doubt that already many vessels owe their immunity from damage, and in some cases even their safety, to its employment.

Among remarkable instances of saving life, is one, cited by Admiral Cloué, of the boats of a ship burnt in 1885, 800 miles from the Seychelles Islands, in which the crew were making their way to land. A cyclone was encountered, which raised a terrific sea, but the boats, provided with oil by the prescience of the captain, weathered it out in perfect safety for sixty hours, riding to a floating anchor of their masts and oars, to which was attached a bag of oil.

Our author points out that from the time of Pliny oil has been thus used, but only by small communities, or by individuals, whose efforts to bring it into general use have always failed. Benjamin Franklin presented a paper on the subject to the Royal Society of London, which is printed in the Philosophical Transactions, 1774, but it remained without fruit.

Experiments were carried out in this country in 1883 by Mr. Shields, at Peterhead and Folkestone, with a view of diminishing the heavy sea at the entrance of these harbours. These experiments were successful, but at the expense of a great quantity of oil; the fact being that the conditions of breaking seas in shallow water are totally different from those in the open ocean.

Admiral Cloué remarks on the great utility of oil when wrecks have to be boarded; and suggests that the builders of rock lighthouses, when their work is delayed by the difficulty of landing material, might find it to be of much service.

The general application of oil is in fact yet in its infancy, and everyone must welcome any such good collection of facts, and of suggestions tending to extend its sphere of usefulness, as that given in "Le Filage de l'Huile."

W. J. L. WHARTON.

OUR BOOK SHELF.

Comparative Morphology and Biology of the Fungi, Mycetozoa, and Bacteria. By A. De Bary. Translated by Henry E. F. Garnsey, M.A. Revised by Isaac Bayley Balfour, M.A., M.D., F.R.S. (Oxford: Clarendon Press, 1887.)

ANYONE acquainted with the numerous researches of De Bary, published in German, will readily indorse Prof. Balfour's remark in the preface to this English translation, viz. "it brings within reach of all English-speaking students the most thorough and comprehensive treatise upon these groups which has appeared in any language," and after perusing this volume we should add that "a finer volume, and a more handsomely and exhaustively illustrated one," is not known in the literature of this subject.

The book seems to us more like a well and comprehensively arranged collection of classical monographs on Fungi and allied organisms, written by a master mind, translated by a scholar, and revised and edited by a practical worker and teacher of these subjects.

It is difficult to pick out any one chapter in which this is not conspicuous. The array of facts, and of phenomena as to form, growth, and development of Fungi, and minute details bearing important relations to one another and to the whole, are told with singular lucidity and in comprehensive sequence; and numerous suggestions that at once engage and invite the reader's and student's inquisitive mind are everywhere, almost on every page, to be met with. As the title of the book indicates, the subjects of Fungi, Mycetozoa, and Bacteria are each separately treated in the first, second, and third parts of the volume respectively.

As was to be expected from De Bary's researches, the first part forms the bulk of the volume. As far as our present knowledge of the ever-enlarging subject of the thallus, spores, and development of Fungi goes, hardly anything could be added to make the book complete both for students and workers; but we venture to think that in Chapter V., besides the important bibliography added to the description of the different groups of Fungi, an appendix setting forth briefly the various species hitherto recognized, not only in name but also in distinguishing characters, would be a valuable addition.

This is still more the case in the third part—Bacteria. We doubt whether this will advance the knowledge of the student beyond a general insight into the nature and mode of life of Bacteria, though he will find here a most valuable and suggestive account of the different modes of spore-formation.

The illustrations are very numerous and well rendered. The bibliography in the first part (Fungi) is carefully and judiciously arranged.

As to the translation little need be said. It is excellent, and the book reads more like an original than a translation, if it were not that one is repeatedly reminded of the contrary by the presence, after an exact rendering in English, of the original German. There seems to be really no necessity to put (p. 1) after filamentous Fungi (*Fadenpilze*); (p. 2) after compound Fungus body (*Zusammengesetzter Pilzkörper*); (p. 4) after sprouting Fungi (*Sprosspilze*); (p. 73) endogenous spore-formation (*Endogene Sporenbildung*); (p. 84) solution or gelatinous swelling (*Auflösung, gallertige Verquellung*).

Why should (on p. 110) to "tube germination" be added (*Schlauchkeimung*); to "sprout germination" (*Sprosskeimung*); to "germ tube" (*Keimschlauch*)?

It is different with "abjunction" and "abscission" explained on p. 61 in a footnote, for here confusion might arise as to the exact meaning of the German "*Abgliederung*" and "*Abschnürung*."

The "Explanation of Terms" at the end of the volume is in this respect most welcome.

E. KLEIN.

Emin Pasha in Central Africa. A Collection of his Letters and Journals. Edited and Annotated by G. Schweinfurth, F. Ratzel, R. W. Felkin, and G. Hartlaub. Translated by Mrs. R. W. Felkin. (London: George Philip and Son, 1888.)

THE personal interest connected with this volume is even greater than its scientific interest. Emin Pasha already ranks as one of the heroes of the modern world, and the record of the bare facts of his career has all the fascination of a good romance. Appointed in 1878 to be Governor of the Equatorial Province, he ruled his territories with astonishing vigour and discretion, so that in 1882 he was able to report that slave-dealers had been wholly banished from his borders, and that the people subject to him were prosperous and contented. The troubles in the Soudan created for Emin many most formidable difficulties, but his courage never failed him, and we may hope that long before this time he has been stimulated to fresh hope and activity by aid received from Mr. Stanley. The letters translated in this volume begin with one dated Dufilé, July 16, 1877, and include several received

by friends in the course of last year. They bring out indirectly all the qualities of Emin's character, and no one can read them without being filled with admiration for his sustained enthusiasm, his inexhaustible energy, and his unaffected simplicity and modesty. He has been too much occupied with official duty to devote as much time as he would have liked to scientific investigation; but he is an ardent student of zoology, botany, and ethnography, and he says enough to show that we may expect from him hereafter very important contributions to our knowledge of all these subjects. So far as the present volume is concerned, the most valuable of the letters, from a scientific point of view, are those relating to the various tribes whose habits and customs he depicts. His descriptions are remarkably vivid, and are evidently the result of much careful observation. His description of the Wanyoro, for instance, is a model of what such a piece of work ought to be. The writer omits no characteristic that is likely to be suggestive to anthropologists, while he has taken care not to spoil the general effect of his sketch by the intrusion of unnecessary details. Dr. Felkin's introduction is written with perfect tact and judgment, and Mrs. Felkin has done her work as a translator admirably. An excellent map has been prepared by Mr. Ravenstein, who has also done good service by marking the latitude and longitude of every place mentioned in the index and glossary.

Colour. By A. H. Church, M.A. (London: Cassell and Co., 1887.)

IN a work which has been limited to somewhat less than 200 pages, there has of necessity been a good deal omitted which would have been found in a larger work. In the part devoted to the production of the spectrum, the details are almost absent in some particulars and perhaps are rather too full in others. The subject of polarized light is also dismissed too briefly. There are one or two statements to which exception can be taken. The first is where the author states (p. 44) that "calorescence may be regarded as a variety of fluorescence." The introduction of the term calorescence at all is a mistake; but it is a greater mistake to mix it up with what is a really distinctive phenomenon.

Another is at p. 78, where the author says, when speaking of a person who is "red" colour-blind, that "the nerve fibrils which in the normal retina receive the sensation of red are not, indeed, wanting, but transmit to the brain the same sensation as that transmitted by the second set of fibrils, the green." This doctrine is rather against facts: the fibrils are either wanting or else are paralyzed, as the total amount of light perceived by the red colour-blind person in white light is less than that perceived by the normal-eyed person. The sensations of the green and blue primary colours are on the average equal in both, but the normal-eyed person has in addition the red sensation. If the fibrils which in the normal-eyed person respond to the red respond to the green in the red colour-blind person, this would not be the case.

With these and one or two minor exceptions the work is to be recommended for accuracy; and the author may claim to have accomplished what he states in his preface he has endeavoured to do, viz. "to present and to explain in a concise yet popular form many of the chief facts connected with the origin, the phenomena, and the employment of colour."

Astronomy for Amateurs. By J. A. Westwood Oliver. (London: Longmans, Green, and Co., 1888.)

THIS volume, to quote the preface, "is intended to afford the amateur astronomer, possessed of limited instrumental means, but yet anxious to devote his labours to the furtherance of astronomical science, such hints and suggestions as will help him to direct his efforts into the channels which experience has indicated as best fitted to his qualifications and equipment." Its pages are accordingly entirely devoted to practical astronomy, theories of every description being disregarded. The different branches of the subject are dealt with by well-known specialists, Mr. Oliver's share in the work being chiefly editorial. The fundamental chapter on the telescope and observatory, which is full of practical information, is appropriately contributed by Sir Howard Grubb. Mr. Maunder contributes an instructive chapter on the sun; Mr. Gore deals with variable stars, of which an admirable list is given; and Mr. Denning gives directions to those who are anxious to distinguish themselves as comet-discoverers. The chapter on the moon is very detailed, and, with the index map, will be of great service to observers of our satellite. Special stress is in all cases laid upon the importance of adapting the ends to the means. The book is thoroughly practical throughout, and Mr. Oliver deserves the thanks of all who are interested in the progress of astronomy, for bringing together such an excellent series of papers. Celestial spectroscopy and photography are reserved for a forthcoming volume, which we sincerely hope will not be behind the one already issued.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Micromillimetre.

PROF. RÜCKER'S note in NATURE, of February 23 (p. 388) induces me to ventilate a suggestion in nomenclature which, among other advantages, might reconcile the practice of botanists and biologists with the C.G.S. system by leading to the disuse of the prefixes *mega-* and *micro-* in favour of *self-significant* prefixes.

It is not improbable that, in spite of Prof. Rücker's protest, the arbitrary definition of the prefixes *mega-* and *micro-*, laid down in the C.G.S. system may come, or continue, to be disregarded in different departments of science, until the ambiguity thence arising necessitates their disuse, as the disuse of the words *billion*, *trillion*, &c., is necessitated by their different senses in English and French. Be this as it may, it is certainly desirable that those who are not in the daily habit of speaking of *megohms*, *megadynes*, *micromètres*, &c., should be saved the necessity of recalling, or hesitation in realizing, the precise meaning of the prefixes.

Instead of denoting *decimal multiples* by Greek, and *decimal parts* by Latin, prefixes to the name of the unit, let the multiples be denoted by the addition of a termination *-n* (say), with a suitable vowel, and the parts by that of a termination *-t* (say), and let the *order* of multiples and parts alike be denoted by numeral prefixes indicating the *power* of ten by which the unit is multiplied or divided, or, what is the same thing, the distance of the digit denoting it from the units digit.

Thus, starting from the *metre*, instead of the scale—

metre { decametre, hectometre, kilometre, &c., / decimetre, centimetre, millimetre, &c.,

we might adopt the following:—

metre { metron (or monometron), dimetron, trimetron, tetrametron, &c., / metret (or monometret), dimetret, trimetret, tetrametret, &c.

Then the *micromètre* (the botanists' *micromillimetre*), would become the *hexametret*; the *megohm*, the *hexohmen*; the *megadyne*, the *hexadynen*, &c.

As an aid to the memory, such a system would be valuable, reinforcing the *visual* memory, which has (I think) in many cases to be relied on, by a corresponding *oral* reading. Thus, the unit of attraction of gravitation in the C.G.S. system is about $6\frac{1}{2} \times 10^{-8}$ dynes, that is, in the proposed language, $6\frac{1}{2}$

octodynets. So, too, Joule's equivalent J, which is about "$4 \cdot 2 \times 10^7$ ergs per gramme-degree Centigrade" (Everett's "Units and Physical Constants"), would be more easily remembered as 4·2 *heptergons*. Again, the velocity of light would be (approximately) expressed as 3 *octometrons* per second, or 3 *decavelons*, if the word *vel* were adopted for the unit speed in the C.G.S. system—namely, that of 1 centimetre (or *dimetret*) per second. I have chosen these instances, as cases where the prefixes mega- and micro- would be of little use as aids to expression or memory.

The system I am advocating coincides exactly with the method, which I believe most intelligent teachers of arithmetic are adopting, of reckoning the place of any digit of a number by its distance, not from the decimal point, but from the unit's digit. This distance it has been proposed to call the *order* of the digit, so that the order of the unit's digit is 0; those of the tens, hundreds, &c., 1, 2, &c.; and those of tenths, hundredths, &c., −1, −2, &c. Then, if the *order* of a number be regarded as that of its highest digit, its *order* is the *characteristic* of its logarithm. I forbear to dilate on the advantages of this reform in arithmetical language, but it is obvious that the proposed system naturally arises out of it. If the British Association or the Physical Society should, after discussion, accept the principle of the proposed nomenclature, and give it the stamp of their authority, I believe they would add to the benefits they have already conferred on science by the introduction of the C.G.S. system of units. My proposal would not extend to attempting to replace the words in ordinary use—kilometre, millimetre, kilogramme, &c.—unless they, in the course of time, died out, replaced by the synonyms here proposed on the principle of the "survival of the fittest."

Harrow, February 27. ROBT. B. HAYWARD.

IN NATURE of February 23 (p. 388) there appears an interesting letter from Prof. A. W. Rücker with reference to the equivalent value of the "micromillimetre." It is therein mentioned that the micromillimetre is commonly employed by biologists as equivalent to one-thousandth of a millimetre; but that the proper name for the thousandth of a millimetre (μ) is "micromētre," and not "micromillimetre."

Permit me, however, to suggest that even the denomination "micromētre," may be hardly acceptable to scientific workers. The denomination for the measure of the one-thousandth of a millimetre (μ), or 0·000001 metre, is "micron," and not "micromētre."

For the "micron" we have the authority of the "Comité International des Poids et Mesures." One shudders at the thought of the confusion likely to arise when computers are required to deal with both micromētre-units and micrometer-divisions.

The Comité International have also recommended the use of the following metric denominations for minute measurements:—

Denomination.	Symbol.	Equivalent.
Micron	μ ...	0·001 millimetre.
Microgramme ...	γ ...	0·001 milligrame.
Millilitre	*ml* ...	0·001 litre.
Microlitre	λ ...	0·000001 litre.

For the millionth of a millimetre we have at present the (C.G.S.) denomination "micromillimetre" ($\mu\mu$), as pointed out by Prof. Rücker. H. J. CHANEY.

7 Old Palace Yard, Westminster, February 27.

ALLOW me to add a few remarks to Prof. A. W. Rücker's letter, published in your issue of February 23 (p. 388).

Mr. O. J. Broch, Correspondent of our Institute in its Section of Mechanics, and Director of the International Board of Weights and Measures, having kindly undertaken to ascertain by actual measurement my pendulum's coefficient of expansion by heat, began by asking how old it was. On my expressing surprise at such a question, he told me that, having carefully measured the length of a brass rod recently made and 1 metre long, he found that it became shorter by 8 *microns* in the first year, and 3 more in the second one. *Micron* is currently used here to express 1/1000 of a millimetre. French botanists call it μ, and seldom use its first decimal because they cannot see such a small space.

The only objection against *micron* is that, unlike other subdivisions of the metre, it does not define its length by its name. But the word *metre* has itself the same fault. It is the ten-millionth part of a quarter meridian, and is, according to Clarke's computations, too short by 0·2 millimetre, or, more exactly, 187·7 microns. Improvements in geodesy will probably alter in either sense that fraction which is too small to disturb terrestrial requirements.

The quarter meridian being the true basis of our metrical system, it ought to have a name of its own, and might be called *megist*, as being the greatest space accurately measured. It should be the *metre* used in astronomy. Thus the velocity of light would be 30 megists, the motion of the star Aldebaran in the line of sight would be 18 megists per hour, and the sun's distance 15,000 megists ± 50. To give the latter in kilometres or miles is tantamount to describing the height of St. Paul's in London as being 1,100,000,000 microns. It is useless to express a distance in units so small that one of them may be added or subtracted without altering our useful notion of the whole sum. Moreover, those who can grasp at once a practical idea of such huge numbers are few and far between.

February 29. ANTOINE D'ABBADIE.

Coral Formations.

MR. G. C. BOURNE'S observations, as far as described in last week's NATURE (p. 414), appear to corroborate fully the view that corals grow more rapidly and luxuriantly on those parts of a reef or bank where there is an abundant supply of food, and only in scattered patches where the food supply is limited or where there is a quantity of sand or other inorganic materials in the currents. He states his belief that "the favourable conditions are due to the action of currents on coral growth." If it be not the food in these great oceanic currents, then Mr. Bourne should tell us what it is in "the action of currents" bathing the outer slopes of a reef that renders them favourable to growth; does he hold to the old view of more oxygen in the water?

It is to be hoped that Mr. Bourne has observed some of the corals feeding on the outer slope or in the lagoon, and can tell us of what their food consists. It will be interesting to know if he has worked his tow-nets in the outer currents, in the "strong currents," and in the still water, and has made a comparison of the results. If he has done so, his paper will doubtless be one of great interest and value.

There would appear to be a slip of the pen in the passage where Mr. Bourne refers to a current impinging directly on a slope. JOHN MURRAY.

An Incorrect Footnote and its Consequences.

THANKS to the wide circulation of NATURE, my original note with the above heading has attracted attention in quite a number of the proper quarters. Several letters have reached me on the subject, and more than one of the writers, after reporting that the *Demonstratio eliminationis Cramerianæ* had been found properly catalogued under De Prasse, proceed in consequence to express their surprise at Baltzer's mistake. Mr. Copeland's letter in yesterday's NATURE (p. 343) adds another instance of this correctness of cataloguing. The additional fact, which he mentions, that there are two copies of the *original edition* of the *Demonstratio* in the Dun Echt library is very interesting, and is a fresh proof of the existence there of valuable rarities.

When, however, Mr. Copeland diverges into the fruitless path of "the might have been" he is much less pleasantly instructive. Having read my letter on the search for a work by Mollweide, and on the discovery that the work meant was not by Mollweide at all but by De Prasse, Mr. Copeland turns to his catalogue under Mollweide, finds a cross-reference to De Prasse, looks up De Prasse, picks out the desired plum, and is pleased accordingly. In all this there is nothing singularly lucky or otherwise: it is exactly what ought to have happened. Mr. Copeland apparently thinks that the cross-reference in the Dun Echt catalogue was the missing link; but if he had occasion to look up other catalogues besides his own he would have found the same cross-reference or a more complete one, and might then have given my helpers in the search a little more credit. The fact is that the booklet of mathematical tables which was the cause of the cross-reference (and whose title Mr. Copeland carefully transcribes) is a comparatively common book, having gone in its time through several editions. Its name is thus of not infrequent occurrence in catalogues, being placed under De Prasse with a reference to Mollweide, or *vice versâ*; and, so catalogued,

it was repeatedly met with by us. There were, however, two points of difference between Mr. Copeland's position and ours. We unfortunately knew from the first what the cross-reference was for, and Mr. Copeland did not: we did not know that Mollweide was not the author, and Mr. Copeland, having read NATURE, did.

The hint is further let slip that something in Poggendorf "might have given a clue to the authorship." All I can answer is that at least two librarians looked up Mollweide in Poggendorf, and were not erratic enough to think of the clue. Indeed, the main part of my original letter has been written in vain if I have failed to make clear that at first we did not seek for "a clue to the authorship," Baltzer having so cruelly misled everybody by asserting in the usual matter-of-fact way that the author was Mollweide. And what I wanted to insist upon was the following simple canon—*Never, so long as books are catalogued as at present, insert without comment an author's name in a title where no author's name exists.* THOMAS MUIR.

Bothwell, Glasgow, February 11.

Cause of September Typhoons in Hong Kong.

AN investigation of the average distribution of atmospheric pressure in South-Eastern China and neighbouring regions has proved the existence of a trough of relatively low pressure in the channel between Formosa and Luzon, and in the northern part of the China Sea during September. This appears to be the reason why typhoons so frequently enter the China Sea during that month of the year, and cause north-east veering to south-east gales to be felt in Hong Kong. Like storms that visit the British Isles, they move along between two areas with higher pressures, and are sometimes developed under the influence of those areas. This remark would be of considerable value in forecasting typhoons in Hong Kong if the district round the China Sea were better furnished with telegraphic reporting stations than it is at the present time. W. DOBERCK.

Hong Kong Observatory, January 11.

The Composition of Water by Volume.

IN my paper "On the Composition of Water by Volume" (Proc. R.S. 1887, 398) the ratio 1·994 volumes of hydrogen to 1 volume of oxygen was given as the most probable value, as I assumed that both gases were of an equal degree of purity. The ratio 1·9967 : 1 was given from the best six experiments if the impurity be supposed to be altogether in the oxygen. At the last meeting of the British Association (B.A. Trans., 1887, 668) I pointed out that this was the most probable ratio, as I had found the impurity to be chiefly oxides of carbon arising from the combustion of traces of the vaseline used in lubricating the stop-cocks finding their way into the eudiometer. Dr. Sydney Young's interesting and ingenious letter (p. 390) is a most valuable corroboration of the hypothesis that the impurity is almost entirely due to the oxygen. A new and larger apparatus, enabling me to use twice the volume of gas, and to measure with much greater accuracy the residue, as well as make a complete analysis of it, still gives a ratio of less than 2 : 1, as the four last experiments made with it show..

	Measured volumes.		Residue.				Combining volumes.		Ratio.
	H	O	H	CO_2	CO	N	H	O	
I.	6909·4	3451·2	... 38·9.	7·2	7·9	0·7	... 6870·5	3440·1	... 1·9972 : 1
II.	6882·2	3414·2	... 74·9	2·6	5·3	3·6	... 6807·3	3409·0	... 1·9968 : 1
III.	7657·2	3798·7	... 63·3	—	—	0·8	... 7593·9	3798·7	... 1·9980 : 1
IV.	7561·4	3777·9	... 19·9	—	—	0·7	... 7541·5	3777·9	... 1·9962 : 1

In Experiments I. and II. vaseline was used as the lubricant, and in III. and IV. syrupy phosphoric acid, by using which all traces of the oxides of carbon are eliminated, and the gases shown to be of a high degree of purity. If we allow that half the amount of oxygen which was used to burn the carbon would be required in addition to burn the hydrogen combined with it in the vaseline, then the ratio becomes 2 : 1.

To use phosphoric acid as a lubricant with security, I find it is necessary to use safety-taps, and am having them in place of the ordinary ones now on my apparatus, and hope very shortly to settle beyond all doubt the true ratio in which hydrogen and oxygen combine to form water.

The ratios of the CO and CO_2 in Experiments I. and II. recall Bunsen's experiments. ALEXANDER SCOTT.

Durham, February 27.

Water Supplies and Reservoirs.

HAVING observed in NATURE (p. 375) an article on the drought of past years, and the probability of one this year also, from deficient rainfall, I take the occasion of suggesting that the old reservoirs might still be made more available for an additional storage of water to counteract its effects. As there is always abundance of rainfall, 40 inches, in Lancashire, and on its surrounding hills, from the cities of which district come complaints of want of water supplies, no fear need be entertained of lack of water if the rain could be all impounded without loss.

It has appeared to me surprising that our hill reservoirs have not been excavated deeper into the valleys and ravines they are made out of, after the manner of the water tanks in India.

In this country a reservoir seems to be simply formed by making a rampart across a ravine, and letting the upper part fill itself, as it stands naturally, with rain in course of time.

The ravine still lies encumbered with sodden grass, stumps of trees, rotting herbage, old walls, and fences, with organic remains, and submerged under the impounded lake, so that an emptied reservoir looks like a long mud ditch, through which a flood or a sea tide had lately passed.

Now if the sides were cut down perpendicularly, and the bottoms levelled horizontally, such valley reservoirs would be able to contain twice as much water as they now do, on the principle that the area of a rectangle is twice as great as that of a triangle between the same parallels.

The whole area of the reservoir might possibly be excavated cleanly out, so as to have its sides and bottom as good as any wet dock in a seaport, and our water supplies would then be considered quite sufficient, and of better quality, for the great towns. If this were done, say, for Liverpool and Manchester, there might be found less need for constructing new and distant waterworks, as the present reservoirs when thus enlarged would hold nearly double the amount they now do.

Besides the lessened rainfall, all reservoirs must suffer serious loss by evaporation, especially in dry seasons, and this is not occasioned so much by the sun's heat as by the action of drying winds, which may carry off as much as 0·20 inch per diem, or 6 inches in a month, or more than an average monthly rainfall. To counteract this tendency, belts of trees planted round the margins of reservoirs are found very useful in sheltering the surface of the waters from the winds, and they act beneficially besides in attracting rain itself to the pools. Further, on the same idea it might be found advisable to cover over entirely the head tanks for city supply, with sheds or roofs, so as to keep off the sun's rays from the water; or else to erect a high screen on the windward side to keep the prevailing winds off the surface, and counteract unnecessary evaporation.

Edinburgh. W. G. BLACK.

A Photographic Objective.

MY attention was called some time since to a letter from Prof. Pickering in your issue of October 13, 1887 (vol. xxxvi. p. 562), describing a form of objective adaptable either to photographic or visual work, by reversion of crown lens and alteration of its distance from the flint.

The form described is exactly similar to that which had been suggested to me by the President of the Royal Society, and which I reported on at the June meeting, 1887, of the Royal Astronomical Society, as having been actually constructed and found to give good results (see *Observatory*, No. 125, pp. 253 and 254).

I should perhaps have mentioned this matter before, but thought that Prof. Pickering would certainly have seen the published account of my previous communication. I have, however, lately seen a newspaper report that a patent has actually been granted for this form of objective.

It therefore appears necessary to point out that this form had been previously suggested by Prof. Stokes, and put in actual practice here.

I may mention that long previous to Prof. Pickering's communication, I had arranged with the Astronomer-Royal to construct the new 28-inch objective for Greenwich Observatory on this principle on certain conditions, and also that this particular form of photographic objective has a distinct place in the last edition of my catalogue. HOWARD GRUBB.

Rathmines, Dublin, February 27.

A Green Sun.

I WAS looking, a few days ago, at three o'clock in the afternoon, towards the sun, which was shining in a clear sky. Exhaust-steam from an engine employed in the new Thames Tunnel works, and situated just below my window, was passing intermittently over his face. Many puffs had already crossed it, some partially, others completely obscuring the luminous disk, when presently, three puffs, following each other quickly, successively covered the sun, which then shone brightly through the steam with a vivid light-green colour. The effect was strikingly noticeable, and the green colour intense. I watched for twenty minutes, but in vain, for another "green sun," and at 3.30 clouds came up.

I have since tried to reproduce the same effect by observing the arc lights in Cannon Street Station through steam rushing upwards from the safety-valve of a locomotive. Seen through the thickest part of such a column of vapour, the electric light exhibits a deep red colour, and I *think* there is a green transmission near the edge of the column; but the latter was unsteady, while the point is evidently critical, and it is impossible to say positively that it was so. D. PIDGEON.

Holmwood, Putney Hill, February 11.

RABIES AMONG DEER.

THAT all domesticated or semi-domesticated mammals succumb to inoculation with the virus of rabies has long been asserted, and examples of its occurrence have been duly recorded. The possibility, however, of the disease affecting half-wild animals seems to have been lost sight of, and it was therefore with much surprise on the part of the public that the announcement was received last year of the deer in Richmond Park being attacked by the malady.

Apart from the general interest attaching to the welfare of the public using the parks in which these animals are kept, and beyond the special interest felt by the veterinary profession in the clearing up of the diagnosis of this strange and novel condition, the outbreak was of importance as affording a fresh opportunity of investigating the character of the malady under, as it were, new circumstances, and hence we find in the reports of this epizooty recently furnished to the Privy Council by Mr. Cope and Prof. Horsley, many points which fill up certain blanks in our scientific information on the subject.

The prevention of rabies in all animals we have shown before to be the simplest task imaginable for the health authorities of this country to undertake, and nothing illustrates this more clearly than the history of the recent epidemic, which attracted so much notice on account of its excessive mortality, and which terminated by causing the local mischief which forms the ground of this article.

It will be remembered that in 1884 rabies began to increase in the London and home counties districts. No notice being taken of its spread, it soon produced a severe effect, when in 1885 the numerous deaths (twenty-seven) among human beings caused a popular panic, and led the authorities to institute measures for its repression. The authorities in the London district having provided for the merciful extirpation of stray dogs, the familiar vehicle of the disease, secured the non-transmission of the virus by enforcing the use of muzzles. The result of their work during 1886 has been seen during 1887, in the practically total immunity of the population of this great city from this the most justly dreaded of all diseases. Let us not forget to add in passing that as was pointed out at the time of the expiration of the local regulations by those acquainted with the malady, that the measures being but local could only produce a temporary relief from the evil, since the metropolis was continually being infected from districts beyond the reach of the regulations, and that though it could be kept free for a time, yet reintroduction of the virus would certainly occur, and the work would have to be done all over again. This is actually now happening, though not yet officially declared. The disease has reappeared (as it has usually done) in the southern suburbs, and is gradually making its way into the metropolis.

But to return. The epidemic of 1885 terminated in the London district with the infection of the roe deer in Richmond Park, resulting in the extermination of several hundreds of these valuable and pretty animals. From Mr. Cope's interesting report it appears that the first to be seized was a doe which had a suckling fawn, and as we learn from the very valuable evidence of Mr. Sawyer, the head-keeper of the Park, it seems that under these circumstances a doe will attack a dog attempting to worry the herd, as a rabid dog passing through the Park would do. Fortunately in the Richmond case no instance occurred of the transmission of the disease from the deer to man through the dog as in an outbreak recorded in 1856 at Stainborough. Had this happened, the deaths of the deer would not have been attributed to various causes, poisoning, &c., as they now were until the remarkable aggressiveness of the affected animals led to a thorough investigation by the veterinary advisers of the Government. Rabid deer were sent for observation to the Veterinary College, and the symptoms noted. The exact determination yet remained to be made, and, thanks to the recent researches of M. Pasteur, this was now possible. Portions of the central nervous system from these animals were sent to the Brown Institution, and there inoculated by Prof. Horsley into rabbits by the subdural method. These animals died after exhibiting the characteristic symptoms of rabies, and after death the usual *post-mortem* appearances were duly discovered. More infected deer were sent also to the Brown Institution, and the extraordinary changes effected by the disease more closely studied. This kind of deer, naturally gentle and timid, was transformed into a fierce and savage animal, rivalling the rabid horse almost in its attempts to do mischief. The early symptoms, as in all animals, appear to have been indicative of mental hallucination, for the animals would stop feeding, hold up their heads, sniff the air, and then, without the slightest reason, burst into a gallop. When placed in confinement the least noise attracted their attention, and later—*i.e.* on the second and third day—caused them to charge in the direction of the sound. The mental perversion which leads a rabid dog one moment to lick with almost frantic energy a healthy dog placed with it, and then the next moment to violently bite it, finds its parallel in the deer similarly affected, for these animals in a like manner licked their companions, and then ferociously attacked them, seizing them with their jaws (usually about the shoulders) and tearing off hair and pieces of skin. The points thus inoculated with the virus after cicatrization became, as is almost invariably the case, the seat of intense irritation when the disease actively showed itself; hence one of the most prominent signs presented by the animals was that of their rubbing themselves with such force as to make these parts raw. In connection with the differences which are now known to be characteristic of the same disease in different classes of animals, it is interesting to note that in all large animals, whatever be the previous temperament, the course of the malady is closely identical; thus in the horse, the ox, the sheep, the pig, the deer, &c., the illness is rapid, there is great aggressiveness, and yet early paralysis. It is of common knowledge that in the dog these two latter features are sometimes widely separated. The paralysis may set in so soon as to obliterate aggressiveness, and thus a distinct form (dumb) of rabies be produced, though of course the aggressive form of the disease always ends in paralysis if not suddenly arrested by syncope. In the deer the combination of the two symptoms seems to have been very equal. For even when the animal had fallen down from paresis (of the hind-limbs more especially) it would nevertheless spring up and attempt to seize and worry with its teeth every person or object

coming within its reach. The complete metamorphosis of the usual temper of the animal is of course only to be explained by profound mental disturbance, exactly as seen in the human being. We have alluded to the mode of transmission of the disease—viz. through the saliva. This mode was put to direct experiment by an infected animal being placed with a healthy one which had been isolated for some time, and the incubation period was determined in this instance to be nineteen days, the comparative shortness of the period being no doubt due to the very numerous points of inoculation. An interesting and confirmatory circumstance of the reality of this method of transmission was afforded by the fact that so long as the bucks retained their horns they were able to literally stave off infection, but as soon as these natural means of defence fell off at the usual periods, both sexes suffered alike.

The mode of death seems in all cases to have been ultimately cardiac failure, which supervened frequently before the customary coma, the final stage of paralysis, was developed. Relatively, syncope occurred much more frequently than it does in the human subject, and *a fortiori* than it does in the dog, a circumstance explicable by the necessarily extremely fatiguing nature of the fits of excitement to which deer are evidently specially liable in the early development of the disease. According to Prof. Horsley's pathological report, both macroscopic and microscopic appearances of the affected tissues revealed the usual lesions which are symptomatic of rabies. This last fact is a healthy sign of scientific progress, for any layman who has sought to obtain from books or verbal statements made by those justly recognized as being qualified to speak with authority on this subject must have been disappointed with the uncertainty of knowledge which has prevailed respecting the morbid anatomy of rabies up to the present time. The obscurity which existed on this point was aggravated no doubt by the absurd popular superstitions connected with the disease, and by the failure to recognize that it was simply a very severe kind of one of the acute specific maladies. From the latter cause especially has confusion arisen, since it will be found that previous records of the *post-mortem* appearances fallaciously comprehend the examination of animals dying at all possible stages of the malady. But now we know these points accurately; and as in this particular case the subject has been so thoroughly worked up, there will be scarcely any excuse for the disease escaping immediate recognition and adequate treatment.

Here we cannot help pointing out what a very grave injury is inflicted on the public by the vexatious operation of the so-called Vivisection Act, which prevents the veterinary inspector from at once resorting to M. Pasteur's admirably simple and conclusive method of testing the real condition of any animal killed under the suspicion of rabies. Under the present *régime* valuable time is lost, and risk incurred of the inoculative material becoming useless from decomposition, &c., by reason of his being compelled to forward it to some such institution as the Brown for examination. The very valuable observation recently published by M. Pasteur's assistant Dr. Roux, that the immersion of the tissue in a mixture of glycerine and water prevents septic change, but does not mitigate the influence of the virus, to a slight extent obviates part of the difficulties and inconvenience just noted, but the anomaly still remains that, while the immense value of the experimental test has received the full recognition of the recent Committee of the House of Lords, the law does not permit it to be used except in one, or at the outside two places in Great Britain, which have with the usual difficulties and obstruction succeeded in obtaining the necessary permission. No one perhaps supposes that the benefits which science offers to the public will ever be received with anything like adequate acknowledgment of the difficulties, and it may be dangers, which have attended this or that particular discovery. But we think that it cannot be recognized by the mass of the people who actually or theoretically direct the Legislature by their votes, that, while they eagerly reap the benefits of the harvest of science, at the same time they permit that harvest to be choked by the tares of legislative obstruction, and thus very greatly diminish the profits which would otherwise be theirs.

Just as we are much behind other nations in the foundation of technical instruction, so we are being fast outstripped in the provision for means for the scientific investigation of matters which, like the one we are now considering, greatly concern the public welfare. We believe it to be a fact that at the present moment neither of the two great Government Departments which are concerned in the scientific arrest of national disease, viz. the Privy Council and the Local Government Board, have any laboratory whatever at their disposal, and consequently are obliged to seek the necessary accommodation in private institutions; or, to put it in plain language, the Government is not ashamed to get its public work done by the favour of private means. The Berlin Laboratory and the Pasteur Institute should serve as the kind of example which a statesman whose desire for the improvement of the country and the people is not a question of votes but of genuine interest might study with advantage.

Those gentlemen, unfortunately few in number, who represent science at the present moment in Parliament, would have a large field of good work open to them if they attempted to reform this state of affairs by adjusting the advantages and assistance offered by science to the real needs of the nation. At present the actual opinion of the scientific world on any subject of special interest is usually only extracted with difficulty by evidence before a Select Committee. It would be very easy for the scientific members of the House to concentrate their force by previous meeting and organization, and so to give weight to that side in a debate which was truly working for the best solution of any national problem involving health and disease. In former years, the opinion of unscientific persons has been sought on the subject of rabies as being of equal weight with the assured observations of scientific experts. This lamentable state of things has led to the present condition of our legislation against this disease, under which the malady is but temporarily, if readily, stamped out in one district alone; this same district becoming infected again from neighbouring parts of the country as soon as the regulations are withdrawn. There is no doubt from the minutes of the Lords Committee on Rabies, that the Report of that Committee was drafted in this unfortunate manner owing to the influence of Lords Mount-Temple and Onslow, who, in their speeches and writings, have afforded numerous evidences of their complete want of scientific knowledge of the nature of the disease, and who, consequently, have failed to grasp the most obvious way in which it can be extirpated—namely, the universal application of preventive legislation. Mistakes of this kind, it seems to us, would be utterly prevented by combined action of the scientific members of either House, and if, as is sometimes our unfortunate duty, we have to chronicle ill-advised measures of suppositiously scientific officialism, let us hope they will not have passed out into law without a strenuous protest from the *united* voice of "our representatives."

THE COMING OF AGE OF THE "JOURNAL OF ANATOMY AND PHYSIOLOGY."

DURING the past summer there was established (as our readers have been informed), under the title of the "Anatomical Society of Great Britain and Ireland," a new brotherhood of anatomists; and the adoption by

it of the above-named journal as a medium for publication, taken in conjunction with the fact that the same has entered on its twenty-second year, affords a fitting opportunity for briefly reviewing its progress and prospects.

Of the work of the Society to which we have alluded it would be premature to judge. It has been founded in the cause of "those interested in the science of anatomy," and on glancing through its roll of members we see that the comparative and human anatomist are, at last, at work in a common cause. All modern experience shows it to be a truism that the study of human anatomy, if it is to bear good fruit, must be based upon the comparative method. It is well known that essays have long since been made, by certain leaders at home, and by Coues more especially in America, towards the realization of this dream: we will not pause to comment upon the somewhat tardy manner in which these have been received by human anatomists at large. The new Anatomical Society, as its constitution shows, is alive to the truth we have asserted, and, this being so, we shall follow with extreme interest the progress of the new brotherhood, which, if properly ordered, cannot fail to exercise a most beneficial influence upon the healing art.

The journal which the founders of the new Society have selected as their mouth-piece has had, thus far, a successful run. To its pages most of our leading anatomists and physiologists have contributed, and within its covers lie papers which have revolutionized the particular departments of knowledge with which they deal. Fifteen months ago it entered, under a change of publishers, upon a "new series"; and more recently it has, under its extended auspices, as might be expected, shown signs of increase in bulk. Its editors have been ever indefatigable, and most willing to oblige all who have applied to them; but the limit of their generosity has most certainly been reached, and, unless we are sadly mistaken, they will before long find it necessary to reconsider their scheme. In anticipation of this, and in the interests of workers in general, we would advise a more judicious selection and revision of matter tendered for publication than is at present adopted. In the current number we find thirteen papers presented in all—some of great merit, others of a more questionable character. In one of them we read at the outset the remarkable statement that "the minute anatomy of the skin of the horse has never before been described," and at the conclusion the erroneous assertion that "having got perfectly free from the old hair the (hair-) papilla now commences secreting again." On reading this and certain other papers which have been published of late, we cannot close the volumes without being struck with the general looseness and absence of all regard for authority which pervade them. This should not be. To papers such as these the worker turns for originality, or, failing that, for at least a *résumé* of work done up to the time of writing: their multiplication, in the unsatisfactory form to which we now reluctantly call attention, is regrettable, and, in the interests of a literature already overburdened, greatly to be deplored.

By way of further insuring the restriction of the publication within reasonable limits, we would urge the exclusion from the body of each issue of pure compilations and papers wholly controversial—such, for example, as one a short time ago devoted to a consideration of the relations of the Mammalia to the lower Vertebrata, and others which could be named. Productions such as these, containing nothing original, and occasionally but a portion of that which is known on the subjects under review, should be dealt with as supplementary matter. We hear a great deal nowadays, on all hands, about the scant recognition of work done by our countrymen. The retention of papers such as those to which we have alluded, in an authoritative journal like the one before us, cannot fail to call forth the unwelcome "*enthält nichts neues*"; and if it be persisted in, is it not likely that we may yet have to thank ourselves, in a measure, for the supposed want of respect?

Far be it from us to discourage the efforts of individual workers. In calling attention to these defects we merely desire to guard against reproach. If the journal whose interests we are seeking is to continue its useful work done in the past, and to do justice to the best interests of its new supporters in the future, some such deliberate modifications as those to which we have pointed are called for. Far-reaching interests will not excuse inauthoritativeness, and, if the new leaven is to work its best, the rising generation of anatomists will not tolerate inefficiency.

NOTES.

BARON VON SCHWERIN, the Swedish explorer, has presented his whole collection of ethnographical objects, gathered during the last two years' journeys in Africa, to the National Ethnographical Museum at Stockholm. The collection is the largest and most valuable ever presented to this institution by any private person.

ADMIRAL SIR ASTLEY COOPER KEY died suddenly last Saturday. He was in his sixty-seventh year. He had seen much active service, and had held some high appointments, including that of Principal Naval Lord of the Admiralty, and Director of the Royal Naval College, which owed much to his endeavours to apply science to the wants of the Navy.

THE Swedish Government has decided to expend £3000 on a new botanical museum at the Lund University.

THE eighth German Geographentag will be held at Berlin on April 4, 5, and 6.

FROM July 25 to 31 there will be held in Paris, in the rooms of the Medical School, a meeting of the Society for the study of Human and Animal Tuberculosis, under the presidency of Profs. Chauveau and Villemin. Interesting communications and papers are expected.

THE Chair of Psychology to which M. Ribot has been appointed has long existed in the Collège de France, and was not, as has been stated, established by the Paris Municipal Council. This Chair must not be confounded with that of *Philosophie Biologique*, which the Council is creating for Prof. Giard.

A SHORT course of lectures on "The Protection of Buildings from Lightning," by Prof. Oliver J. Lodge, F.R.S., to be delivered under the title of the "Dr. Mann Lectures," as a memorial of the late Dr. Mann, will be begun on Saturday afternoon next, March 10, at the Society of Arts. The course will consist of two lectures, the first of which will take the form of a slight historical sketch, and will call attention to the outstanding questions, difficulties, and points of controversy in connection with lightning-conductors. At the second lecture experimental answers will be given to some of the questions raised, and an endeavour will be made to supply a more complete account of the liability of conductors to side-flash than has yet been attempted. The chair will be taken at 3 o'clock.

LAST Saturday, Sir James Paget delivered an interesting address to the students attending University Extension Lectures in London. His subject was "Scientific Study," and he showed in a remarkably clear and striking way how the study of science develops the power of observation, fosters accuracy of thought, gives men a vivid conception of the difficulty of attaining to a real knowledge of the truth, and makes them familiar with the methods by which they may pass from that which is proved to

the thinking of what is probable. He also offered illustrations of the power of science to minister to the needs of ordinary life, and to satisfy man's "insatiable appetite for the knowledge of wonders." Such addresses as this, delivered by acknowledged masters in their own departments of study, do excellent service to science by bringing prominently before the public the solid advantages which are to be gained by scientific training. They are also made the occasion of some good writing in the daily newspapers. The *Daily News*, for example, had an excellent article on Sir James Paget's address, enforcing the principle that "the study of science goes further than other studies to teach us the simple love of truth for truth's sake."

THE *Japan Weekly Mail* states in connection with the recent publication of the "Life and Letters of Charles Darwin" that the *Beagle*, in which Darwin made his memorable voyage, is now used as a Japanese training-ship. It is stationed at Yokosuka, a naval station in the Bay of Yedo, not far from Yokohama.

THE Directors of the Crystal Palace have arranged that the Photographic Exhibition shall remain open until March 17, a fortnight later than was at first intended. The public interest in this Exhibition is said to have exceeded the most sanguine expectations.

THE Council has reported to the Senate of the University of Cambridge against the admission of women to degrees.

AT the University of Zürich there are at present forty-five female students, twenty-nine of whom study medicine, fourteen philosophy, and two political economy. In 1887 there were 108 female medical students in Paris.

A NEW and most valuable method of determining the molecular weights of non-volatile as well as volatile substances has just been brought into prominence by Prof. Victor Meyer (*Berichte*, 1888, No. 3). The method itself was discovered by M. Raoult, and finally perfected by him in 1886, but up to the present has been but little utilized by chemists. It will be remembered that Prof. Meyer has recently discovered two isomeric series of derivatives of benzil, differing only in the position of the various groups in space. If each couple of isomers possess the same molecular weight, a certain modification of the new Van't Hoff-Wislicenus theory as to the position of atoms in space is rendered necessary; but if the two are polymers, one having a molecular weight n times that of the other, then the theory in its present form will still hold. Hence it was imperative to determine without doubt the molecular weight of some two typical isomers. But the compounds in question are not volatile, so that vapour density determinations were out of the question. In this difficulty Prof. Meyer has tested the discovery of M. Raoult upon a number of compounds of known molecular weights, and found it perfectly reliable and easy of application. The method depends upon the lowering of the solidifying point of a solvent, such as water, benzene, or glacial acetic acid, by the introduction of a given weight of the substance whose molecular weight is to be determined. The amount by which the solidifying point is lowered is connected with the molecular weight M by the following extremely simple formula: $M = T \times \frac{P}{C}$; where C represents the amount by which the point of congelation is lowered, P the weight of anhydrous substance dissolved in 100 grammes of the solvent, and T a constant for the same solvent readily determined from volatile substances whose molecular weights are well known. On applying this law to the case of two isomeric benzil derivatives the molecular weights were found, as expected, to be identical, and not multiples; hence Prof. Meyer is perfectly justified in introducing the necessary modification in the "position in space" theory. Now that this generalization of Raoult is placed upon a secure basis, it takes its well-merited rank along with that of Dulong and Petit as a most valuable means of checking molecular weights, especially in determining which of two or more possible values expresses the truth.

A REPORT on Indian fibres and fibrous substances exhibited at the Colonial Exhibition, 1886, has been published by authority of the Secretary of State for India. It contains the results of a laboratory investigation conducted by C. F. Cross, E. J. Bevan, and C. M. King, in association with E. Joynson; and Dr. George Watt contributes notes of methods of treatment and uses prevalent in India. In issuing the volume, the authors say that perhaps the utmost they can hope to do is to indicate the scope of a more adequate treatment of the subject. They are convinced that when the vegetable fibres come to be recognized as constituting a special field for research, and worthy the attention of those who have command of the necessary resources, there will be a considerable gain to science in the results of the systematic and sustained investigations which will follow.

MESSRS. LONGMANS AND CO. are preparing for publication "The Testing of Materials of Construction," embracing the description of testing machinery and apparatus auxiliary to mechanical testing, and an account of the most important researches on the strength of materials, by William Cawthorne Unwin, F.R.S.; "A Text-book of Elementary Biology," by R. J. Harvey Gibson, Lecturer on Botany in University College, Liverpool; "Dissolution and Evolution and the Science of Medicine," by C. Pitfield Mitchell; and "The Fundamental Principles of Chemistry practically taught, by a New Method," by Robert Galloway, Honorary Member of the Chemical Society of the Lehigh University, U.S.

MESSRS. SWAN SONNENSCHEIN, LOWREY, AND CO. are issuing, in parts, what promises to be a most useful publication—"The Cyclopædia of Education."

A FIFTH edition of Munro and Jamieson's "Pocket-book of Electrical Rules and Tables" has been issued. The first part of this excellent little volume deals with the fundamental principles and measurements of the science; the second part with their applications, including telegraphy, telephony, electric lighting, and the transmission of power by means of electricity. In the new edition many important additions have been made.

WE have received Part I. of "The Characeæ of America," by Dr. T. F. Allen. The author has postponed the publication of the work from time to time in order to accumulate material for a more complete account of the species growing in America. The demand in America for information concerning these plants is, however, so pressing that Dr. Allen has thought it best to issue the first part, which contains introduction, morphology, and classification. The second part will appear in a year or two, and will give descriptions of the species now known to inhabit American waters. The work is illustrated.

THE new number of the Proceedings of the American Philosophical Society (July to December, 1887), contains, among other important papers, a valuable "Contribution to the History of the Vertebrata of the Trias of North America," by E. D. Cope. There are also interesting papers on the question, "Were the Toltecs an Historic Nationality?", and on the ethnology of British Columbia, the former by D. G. Brinton, the latter by F. Boas.

MM. BEAUREGARD AND GALIPPE, of Paris, have issued a second edition of their practical guide to micrographical work. It has been much enlarged.

M. REINWALD, of Paris, has just brought out the first volume of MM. C. Vogt and Yung's "Anatomie comparé pratique."

THE Report on the Administration of the Meteorological Department of the Government of India for the financial year 1886–87 gives interesting details of the work carried on in the various provinces, and of the inspection of the stations. The observatories now number 135; three have been established in the new territory of Upper Burmah, where scarcely anything is yet known about the meteorology. Rainfall is registered at 486 stations, and bright sunshine at six observatories. Ground temperature is recorded at five selected stations, and some of the results are of great interest, showing that the average temperature of the ground in India is about 5° above that of the air; and also that there is a small oscillation of many years' duration, amounting to about 4°, affecting the air temperature and the intensity of solar radiation. Considerable attention is paid to the laws of drought, and the hope is expressed that by degrees they may be established on a sound physical basis. The influence of forests on rainfall has been fully discussed, and the evidence afforded is favourable to the assumption that forests increase the rainfall. The work of marine meteorology also is actively prosecuted; the weather charts of the Bay of Bengal have been lately mentioned (NATURE, December 8, 1887, p. 137). A work on the storms of that district is in course of preparation, and it is proposed to draw up a hand-book on the subject, for the use of seamen.

UNDER the title of "Deutsche ueberseeische meteorologische Beobachtungen," the Hamburg Meteorological Office has commenced a new publication containing observations made under its auspices abroad. The first part contains observations made at six stations in Labrador from September 1883 to December 1884. These stations were equipped in August 1882 as supplementary to the International Polar Expeditions, and, as the missionary observers were willing to continue the observations, and the stations are important owing to the passage of many barometric depressions over Labrador, it has been decided to retain them. The other stations for which observations are published are Hatzfeldhafen (New Guinea) and Walfish Bay (West Coast of Africa). Future parts are to be published as soon as another year's observations are received from Labrador, and will include observations received from other stations in the meantime.

THE Pilot Chart for the North Atlantic Ocean for the month of February draws attention to the great danger to Transatlantic navigation from icebergs and field ice, from the present time and until the end of August. The ice is liable to be encountered off the Grand Banks as far south as 42° N., and between the 42nd and 52nd meridians. It is pointed out that too much reliance should not be placed on the use of the thermometer, and that warning may often be obtained by means of the echo thrown back from the surface of an iceberg when a whistle is sounded, or any sharp noise is made.

THE Chief Signal Officer of the United States has issued a new edition of "Instructions to Observers of the Signal Service" (Washington, 1887, 142 pp. large 8vo). The "Instructions" are most complete, and contain information which will be very useful to observers in all countries, and many points that will be novel to English readers. On the establishment of a station, a local committee of management is formed, the chairman of which corresponds directly with the Signal Office, and a detailed report on the working of the station is furnished each year. All barometrical observations are to be reduced for gravity at lat. 45°, and complete directions are given for removing air from both barometers and thermometers. Instead of the usual drawings of the instruments, detailed plans of all their separate parts are given; by this means observers obtain an accurate knowledge of their construction. The observation of clouds is referred to seven types only. Full directions are given for drawing weather maps from telegraphic reports, and, finally, a good list of works recommended for study, and the necessary tables for reduction, complete the volume.

A REMARKABLE achievement in transportation of live fish a great distance is described by M. Jousset de Bellesme in a recent number of the *Revue Scientifique*. The aquatic fauna of Chili being very poor, a selection of fish, comprising 100 Californian salmon, 40 carp, 20 tench, 20 gudgeon, with a number of eels, barbs, minnows, lotes, &c., were despatched from Paris in September last to stock the waters. The voyage, of about a month, was, of course, a very trying one in this relation, especially as regards variation of temperature. In treatment of the fish care was taken to lessen the activity of their functions by refrigeration and starvation (a carp will live fifty days without food), and a continuous air circulation was kept up in the water (which was not renewed). There was some loss among the salmon, but thirty-nine were successfully installed at Santiago; and the other groups were mostly intact. Only the gudgeons, lotes, and barbs, suffered serious loss. The experiment seems to prove the possibility of carrying alive the most delicate fish from any point of the globe to any other point. It was also ascertained that a temperature of 23° C. is not hurtful to the health of alevins of *Salmo quinat*, as might have been feared. The expense of the transport was considerable, but was willingly borne by the Chilian Government, in view of future advantage to the country.

THE *Zoologist* for March reprints an extraordinary pamphlet, entitled, "An Account of Wolves nurturing Children in their Dens." This pamphlet was printed at Plymouth in 1852, and has long been out of print. On the wrapper of a copy in the Zoological Library of the Natural History Museum at South Kensington there is the following memorandum in the handwriting of the late Colonel Hamilton Smith:—"This account, I am informed by friends, is written by Colonel Sleeman, of the Indian army, the well-known officer who had charge of the Thugg inquiries, and who resided long in the forests of India." The writer records a number of cases of children who are said to have been nurtured by wolves in India. In one instance a large female wolf was seen to leave her den followed by three whelps and a little boy. This happened near Chandour, ten miles from Sultanpoor, in the year 1847. The boy went on all fours, and ran as fast as the whelps could. He was caught with difficulty, and had to be tied, as he was very restive, and struggled hard to rush into holes and dens. When a grown-up person came near him he became alarmed, and tried to steal away. But when a child came near him he rushed at it with a fierce snarl, like that of a dog, and tried to bite it. When cooked meat was put near him he rejected it with disgust; but when raw meat was offered he seized it with avidity, put it on the ground under his hands, like a dog, and ate it with evident pleasure. He would not let anyone come near him while he was eating, but he made no objection to a dog coming and sharing his food with him. The trooper who captured the boy left him in charge of the Rajah of Hasunpoor, who sent him to Captain Nichollets, commanding the first regiment of the Oude Local Infantry at Sultanpoor; and some interesting notes as to the boy's habits are given on this officer's authority. He died in August 1850; and after his death it was remembered that he had never been known to laugh or smile. He used signs when he wanted anything, and very few of them except when hungry, and he then pointed to his mouth. When his food was placed at some distance from him, he would run to it on all fours, like any four-footed animal, but at other times he would walk uprightly occasionally. He shunned human beings, and seemed to care for nothing but eating. If the pamphlet can be proved to be perfectly trustworthy, it certainly deserves to be carefully studied by anthropologists.

THE last issue (Heft 37) of the German Asiatic Society of Japan contains a lengthy paper, with numerous tables of analyses, on the food of the Japanese, the authors being Dr. Kellner and M. Mori. They refer at the outset to the extraordinary differences of opinion amongst various writers as to the exact nature of the staple diet of the Japanese people. One writer says it is almost wholly boiled rice flavoured with small quantities of fish or pickled vegetables; another says that, as far as means allow, it is a mixed, and not a purely vegetable diet, and therefore physiologically ample; a third that it is almost wholly vegetarian; a fourth, that as much animal food is consumed in Japan as in Germany, Austria, France, and the Danubian Principalities; and so on. All the writers here quoted are modern men of science who have resided in Japan, and have therefore had ample opportunities for forming an accurate opinion. As to beef, however (there is no mutton in Japan), there can be no question that its consumption is very small. In 1882 only 36,288 beasts were slaughtered, or about 1 kilogramme of meat per head of the population, and it must be borne in mind that a large consumption takes place at the open ports amongst Europeans, and in the proximity of vessels. The conclusions to which the present writers—one of them, it will be noticed, being a native investigator—come is that the food of the Japanese people varies so considerably that, from a physiological point of view, no single proposition can be laid down respecting it. There are two main groups to be distinguished: in one, the people from poverty are compelled to be vegetarians, and use a diet which leaves much to be desired in its effect in strengthening the body; those in the second group are able to obtain animal food from the sea with some ease, and therefore use a mixed diet, which in kind and quantity appears ample. Between these two extremes we find all kinds of diet. The authors have not only made analyses of the various food-stuffs of Japan, but have investigated in various public institutions, from prisons to schools for army officers, the effect of various classes of food on the labour and weight of different persons.

ON February 10, at 12.40 a.m., a brilliant meteor was seen at Venersborg, in Sweden. It went in a direction from south to north, and was surrounded by an intense blue light. It was seen to fall to the earth some considerable distance off, but no sound could be heard.

DR. ROBERT FRIES, a Swedish botanist, has completed a memoir on the fungus-flora of the south-west coast of Sweden on which he has been engaged for a number of years. It embraces 865 varieties.

PROF. SVEN LOVÉN, the "Nestor of Swedish science," recently completed his seventy-ninth year, when he received numerous congratulations from friends at home and abroad. He is at present engaged in publishing a catalogue by Linnæus of the Lovisa Ulrika Museum in Sweden, which will be accompanied by numerous illustrations and explanatory notes from a modern scientific point of view by Prof. Lovén.

THE report of the Norwegian Association for the Preservation of Archæological Remains for last year shows that thirty-one barrows were opened in 1887 by the Association at Tvetene, in the parish of Brunlanæs, all of which were found to date from the early Iron Age. Some 146 objects of various kinds were found. These objects were added to the Museum of the Christiania University.

THE well-known Norwegian naturalists, M. Michelet and Dr. Bahrt, have introduced a Bill into the Storthing, prohibiting the killing of any birds (except birds of prey, ravens, rooks, and magpies) in the whole of Norway during the period April 1 to August 15, also the taking of eggs or young birds. The chief object of this Bill is to put a stop to the present wanton destruction of birds by foreign "sportsmen."

MR. F. S. WELLS, of Southgate, has sent us four photographs of the lunar eclipse of January 21 last. Considering the small size of the photographs, they are very interesting, and Mr. Wells tells us that they were taken without costly apparatus. In the original negatives the images were merely seven-sixteenths of an inch. Mr. Wells enlarged them five diameters.

MR. R. COPELAND writes to us:—"I have just learnt from Leipzig that Prof. Krehl is the University Librarian at that place, and not Virchl as printed in Dr. Muir's letter on p. 246, and repeated by me on p. 344 of NATURE." Mr. Copeland also mentions that the "Demonstratio eliminationis Cramerianæ" was duly entered under De Prasse by Mr. R. Tucker, Hon. Sec. Mathematical Society, when drawing up the catalogue of the "Mathematical and Scientific Library of the late Charles Babbage" in 1872. This library forms the nucleus of Lord Crawford's collection of scientific books.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Captain R. F. Hibbert; a Common Raccoon (*Procyon lotor*) from North America, presented by Mr. C. J. Urquhart; a —— Civet (*Viverricula* ——) from China, presented by Mr. Percy Montgomery; two Laughing Kingfishers (*Dacelo gigantea*) from Australia, presented by Mrs. Mars Buckley; twelve Black-headed Gulls (*Larus ridibundus*), a Common Gull (*Larus canus*), British, presented by Mr. J. G. Barker; five Prince of Wales's Pheasants (*Phasianus principalis* ♂ ♂ ♀ ♀ ♀) from Afghan Turkistan, presented by Major Peacock, R.E.; a Cape Eagle-Owl (*Bubo capensis*), five Angulated Tortoises (*Chersina angulata*), three Areolated Tortoises (*Homopus areolatus*), a Natal Sternothere (*Sternothærus castaneus*), a Smooth Snake (*Homolosoma lutrix*), an Infernal Snake (*Boodon infernalis*), a Rufescent Snake (*Leptodira rufescens*), a Spotted Slowworn (*Acontias meleagris*), five Round-throated Frogs (*Rana fuscigula*), a Narrow-headed Toad (*Bufo angusticeps*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Natal Sternothere (*Sternothærus castaneus*) from South Africa, presented by Colonel J. H. Bowker, F.Z.S.; two Cirl Buntings (*Emberiza cirlus*), British, purchased; a Hog Deer (*Cervus porcinus*), an Eland (*Oreas canna*), a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

TEMPEL'S COMET, 1867 II.—M. Raoul Gautier has published in the Memoirs of the Société de Physique et d'Histoire Naturelle de Genève, vol. xxix. No. 12, a discussion of the orbit of the comet discovered by Herr W. Tempel, at Marseilles, on April 3, 1867, with especial reference to its appearances in 1873 and 1879. There are several points of especial interest about this comet: not only was it an addition to the number of known comets of short period, but it possesses the peculiarity of an elliptic orbit of but slight inclination, and of less eccentricity than that of any other member of the same class. Its spectrum, too, would seem to be unusual, for the imperfect view of it obtained by Dr. Huggins, May 4 and 8, 1867, led him to conclude that the bright bands, which it gave together with a continuous spectrum, were not those of carbon. Its orbit, and especially its period, is also subject to great perturbations from the action of Jupiter, and its perihelion distance was considerably increased between 1873 and 1867 without its aphelion distance being much altered. It had also been identified by M. Winnecke with the comet observed by Goldschmidt at Paris, May 16, 1855, in a search for De Vico's comet, but von Asten's inquiries have shown that the identification was an erroneous one.

M. Gautier—though the perturbations due to Jupiter during the period 1873–79, with which he was principally engaged, have been but small, the two bodies being always distant from each other—has calculated the perturbations after the method of variation of the elements, since this method was most suitable

for the periods 1867–73 and 1879–85, and he wished to connect his calculation with those for the two other periods, which it is his intention to compute, and which he hopes to carry forward so as to furnish positions for the comet for its next return in 1892.

The following are the final results obtained by M. Gautier for the two appearances :—

	Second appearance, 1873. Mean equinox 1873·0.		Third appearance, 1879. Mean equinox 1879·0.		Mean errors common to both systems.
M_0	1873 April 15·0 = −4° 5′ 24″·177	M	1879 April 24·0 = −2° 10′ 2″·454	±	29″·35
μ =	592″·9765465	=	593″·1200165	±	0·000140
ϕ =	27° 33′ 22″·79	=	27° 33′ 6″·69	±	5·24
	° ′ ″		° ′ ″		′ ″
π' =	240 2 52·71	=	240 15 31·77	±	1 31·75
☊′ =	21 29 0·30	=	21 29 34·33	±	6·14
ι' =	27 0 58·62	=	27 0 39·50	±	2·00
π =	238 2 52·98	=	238 15 30·65	±	1 31·75
☊ =	78 43 48·42	=	78 45 55·66	±	13·18
ι =	9 45 58·59	=	9 46 2 64	±	2·61
T =	1873 May 9·83096	=	1879 May 7·15493	±	0^d·0495
log a =	0·5179794	=	0·5179093		
log q =	0·2482605	=	0·2482463		
e =	0·4626205	=	0·4625512		

The time of perihelion passage is given in Berlin mean time.

The comet was not seen in 1885, and there seems distinct evidence, from the greater difficulty of observation in 1873 and more especially in 1879, that it has diminished in brightness at each succeeding return.

COMET 1888 *a* (SAWERTHAL).—The following elements have been computed for this comet by Mr. W. H. Finlay, Royal Observatory, Cape of Good Hope :—

T = 1888 March 17·18 G.M.T.

$$\left.\begin{aligned} \pi - ☊ &= 4^\circ\ 29' \\ ☊ &= 244\ \ 6 \\ \iota &= 43\ 57 \end{aligned}\right\}\ \text{Mean equinox 1888·0.}$$

$$\log q = 9·8354$$

Error of middle observation—

$$\Delta\lambda \cos\beta = -5'' \quad \ldots \quad \Delta\beta = -2''.$$

$$\begin{aligned} x &= [9·8927]\ r \sin(330^\circ\ 30' + v) \\ y &= [0·0000]\ r \sin(240\ \ 7 + v) \\ z &= [9·7954]\ r \sin(329\ 30 + v). \end{aligned}$$

The following ephemeris for Greenwich midnight has been computed by Dr. L. Becker, the perihelion passage having been increased by one day, as suggested by Prof. Krueger :

1888.	R.A. h. m.	Decl. ° ′	Log r.	Log Δ.	Brightness.
March 5	20 33·9	33 9 S.	9·865	9·956	1·5
13	21 3·4	20 29 S.	9·840	9·975	1·6
21	21 30·8	8 31 S.	9·838	0·008	1·4
29	21 57·5	1 58 N.	9·859	0·047	1·0
April 6	22 23·5	10 43 N.	9·898	0·088	0·7

The brightness on February 18 has been taken as unity.

THE TOTAL ECLIPSE OF THE MOON, JANUARY 28.—The following list has been received from the Pulkowa Observatory of the number of occultations observed at those observatories from which reports had been received up to February 17, in addition to those given in NATURE for February 2 (p. 333) :

Pulkowa	50	Padua	4
Tashkent	21	San Fernando	10
Turin	32	Strasburg	10
Belgrade	3	Bordeaux	21
Bothkamp	30	Kiel	36
Geneva	23	Collegio Romano	5
Neuchâtel	6	Wilhelmshaven	2
Kis Kartal	2	Marseilles	39
Paris	12	Liverpool	11
St. Petersburg	2	Bilk	8

At Helsingfors and Algiers they had also been successful.

The weather was cloudy at the following stations : Besançon, Breslau, Charkow, Dorpat, Dresden, Gotha, Gottingen, Hamburg, Jena, Kalocsa, Kasan, Kremzmunster, Leipzig, Munich, Nikolajen, Pola, Prague, Riga, and Upsala. Seventy-five observatories had not reported at the above-mentioned date.

VARIATIONS OF LUNAR HEAT DURING THE ECLIPSE OF THE MOON.—Dr. Boedicker succeeded in making a series of interesting experiments under favourable circumstances of the variations in the amount of heat radiated to us from the moon during the progress of the total eclipse of January 28. The observations were made with a Thompson's galvanometer used in connection with Lord Rosse's 3-foot reflector at Parsonstown, and commenced at 7h. 19m., or 1h. 10m. before the first contact with the earth's penumbra, and continued until 15h. 45m., or 1h. 34m. after the last contact. 638 readings were made in all. The principal deductions drawn from the observations were :—

(1) The heat radiated by the moon commenced to decrease long before the first contact with the penumbra.

(2) Twenty-two minutes before the commencement of totality the heat was reduced to less than 5 per cent. of that which it had been twenty minutes before the first contact with the penumbra.

(3) In spite of this rapid cooling at the approach of totality, the heat after the last contact with the penumbra did not remount immediately to the point where it had been before the first contact.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *MARCH* 11–17.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 11

Sun rises, 6h. 24m. ; souths, 12h. 10m. 1·3s. ; sets, 17h. 57m. : right asc. on meridian, 23h. 28·4m. ; decl. 3° 25′ S. Sidereal Time at Sunset, 5h. 16m.

Moon (New on March 12, 16h.) rises, 6h. 16m. ; souths, 11h. 23m. ; sets, 16h. 39m. : right asc. on meridian, 22h. 41·3m. ; decl. 10° 59′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	5 43	11 13	16 43	22 30·9	6 42 S.
Venus ...	5 29	10 12	14 55	21 30·1	15 24 S.
Mars ...	21 20*	2 38	7 56	13 55·2	8 56 S.
Jupiter ...	0 48	5 1	9 14	16 17·9	20 24 S.
Saturn ...	12 51	20 49	4 47*	8 8·9	20 44 N.
Uranus ...	20 10*	1 44	7 18	13 0·9	5 45 S.
Neptune..	8 44	16 24	0 4*	3 42·9	18 1 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

March.	h.	
11	6	Mercury in conjunction with and 5° 8′ north of the Moon.
16	4	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′			h. m.	
R Ceti	2 20·3	0 41 S.	Mar.	13,		*M*
λ Tauri	3 54·5	12 10 N.	,,	14,	22 5	*m*
ζ Geminorum	6 57·5	20 44 N.	,,	15,	2 0	*M*
R Canis Majoris	7 14·5	16 12 S.	,,	11,	0 41	*m*
			,,	17,	20 17	*m*
U Monocerotis	7 25·5	9 33 S.	,,	16,		*M*
δ Libræ	14 55·0	8 4 S.	,,	14,	0 40	*m*
U Coronæ	15 13·6	32 3 N.	,,	17,	1 50	*m*
S Libræ	15 15·0	19 59 S.	,,	13,		*M*
R Herculis	16 1·2	18 40 N.	,,	14,		*M*
R Ursæ Minoris	16 31·5	72 30 N.	,,	12,		*M*
U Ophiuchi	17 10·9	1 20 N.	,,	16,	3 0	*m*
X Sagittarii	17 40·5	27 47 S.	,,	11,	3 0	*M*
U Sagittarii	18 25·3	19 12 S.	,,	14,	3 0	*m*
			,,	17,	2 0	*M*
β Lyræ	18 46·0	33 14 N.	,,	11,	4 0	m_2
R Sagittæ	20 9·0	16 23 N.	,,	14,		*m*
T Vulpeculæ	20 46·7	27 50 N.	,,	13,	2 0	*M*
			,,	14,	4 0	*m*
δ Cephei	22 25·0	57 51 N.	,,	17,	22 0	*M*

M signifies maximum ; *m* minimum ; m_2 secondary minimum.

Meteor-Showers.

	R.A.	Decl.	
Near Capella	50°	48° N.	March 4–12.
,, ξ Virginis	175	10 N.	Bright ; slow.
,, κ Cephei	300	80 N.	Bright ; slow.

GEOGRAPHICAL NOTES.

MOUNT RORAIMA, in British Guiana, was ascended for the third time on October 14 last, by Mr. F. Dressel, an English orchid collector. The first ascent, it will be remembered, was by Mr. Im Thurn, in December 1884. The second was in November 1886, by Mr. Cremer, also an orchid collector. Mr. Im Thurn's ascent took place in the beginning of the wet season, when everything was saturated with moisture. Mr. Dressel ascended after continuous dry weather, and found the upper surface comparatively dry, the elevated portions most markedly so; while large areas of the sward-like levels were perfectly desiccated. The water in the various channels was very shallow, and the deep basins or depressions contained but very small quantities, though in no case was any found to be quite dry. Frequently the surface of the water in these shallow basins was more or less covered with a green, apparently a Confervoid, layer. In the pools at the bottom of these wide basins, Mr. Dressel found a considerable quantity of quartz, in the form both of separate crystals, and of aggregated masses, of various and often of large sizes. The presence of such quartz in such positions and under such conditions, *Timehri* points out, is an extremely interesting fact, though our want of knowledge of the petrographic character of the formation of the top of Roraima, beyond the fact of its being sandstone, renders it barren, and one hardly justifying speculation. It will be remembered that on the first ascent no animal life was noticed during the short time spent on the top; and this necessarily denoted the likelihood of the absence or great rarity of birds and insects. During the two or three hours spent on the top by Mr. Dressel, no birds were seen; but a few specimens of butterflies, all of one kind, of a dark brown and nearly black colour, were observed, and two of them were caught, though one alone was sufficiently preserved to show much of its structure. In the shallow basins a few forms of a small black toad with a yellow spot on the throat was also seen, and one was caught, but was accidentally left on the top. A third animal form was found in the moist earth attached to some plants which had been pulled up; from Mr. Dressel's description it is conjectured by *Timehri* to be a Millipede, allied to *Julus*. It is probable enough that a stay of a day or two on the top would well repay the naturalist; and Mr. Dressel thinks it would not be difficult to arrange for such a stay. The fantastic shape into which the sandstone has been fashioned, and the weirdness of the scenes which have been so graphically described by Mr. Im Thurn, affected Mr. Dressel in a similar manner. He mentions that the surface of the rocks present very closely the appearance of granite, owing to weathering; and at first he thought some mistake had been made in describing the formation as sandstone, until he moved away a small rock from its setting, when its real nature was revealed.

Timehri for December last contains a very interesting account by Mr. E. A. Wallace of a visit he paid to the Guahivos, an isolated tribe of Indians, living near the head of the River Meta, a tributary of the Orinoco, in the Republic of Columbia.

AT the last meeting of the Royal Geographical Society Mr. Robert Gordon read a paper on the Ruby Mines near Mogok, Burma. These ruby mines lie about 100 miles N.N.E. of Mandalay. The ruby-bearing region, so far as known, lies within an area ten miles long by five wide, and consists of groups of small valleys nestling beneath the Toung-Meh range, and to the south of it. The Enjouk valley to the north is said to yield rubies and sapphires, but they have not been regularly worked. The valleys arrange themselves into three groups of nearly equal area by the distribution of the watercourses. To the east a few streams unite to form the Yay-Nee, or red water, so called from the washings of red earth from the mines. The most remarkable thing, Mr. Gordon stated, in the Mogok and neighbouring districts, is the distinctness and diversity of races among the peoples in the different communities, who evidently have kept themselves from intermarriage with their neighbours for centuries, and a brief notice of the tribes whose types are found here may not be out of place. In Kathey, as the name implies, the villagers are Katheys, whose ancestors were brought as prisoners from Munnipore very long ago, as they have lost both the Hindoo religion and their own language. In Mandalay, Prome, and Henzadah, where bodies of the same people have been long transplanted, they keep their race and religion pure still. The ethnologist would find matter of intense interest in the interactions of some of these races upon each other, and perhaps the history of these transplanted Katheys would yield the most curious results. When surveying for the railway in the district south of Mandalay, Mr. Gordon found them extensively distributed throughout the country, always living separately in their own villages, and retaining many of their peculiar characteristics, even when they had become thoroughly Burmanized in their speech, religion, and general habits. They are colonies of pure Aryan race, retaining the features and colour and physique of their Indian ancestors, although surrounded for centuries by Turanians of great assimilating power, whose cordial hospitality and tolerance tend to modify and absorb most of the races coming into close contact with them. None of the yellow races of Burma, or Siam, or China, milk their cattle, and it is difficult when travelling in those regions to get a supply of this very useful article. Near Mandalay, and to the south of it, however, the Katheys have accustomed many of the Burmese to the use of milk, and it is perhaps the only part of Burma where it could be got in the country places. In Bama and other villages the people are Paloungs, who keep up intercourse with the tea-growing Paloungs on the hills to the east, and preserve their language, although, like the Katheys, they have become Buddhists. Less is known of the Paloungs than of most of the great tribes bordering on Burma. They differ in speech, and claim to differ in origin, from all their neighbours. They occupy a wedge-shaped territory of mountains and plateau between the Ruby Mines of Burma, the Shans, and China; their principal State being called Toung-baing, which has nominally been subject to Burma, but which, from its inaccessibility, has been practically independent. The region is known to the Burmese as the La-pet Toung, or Tea Mountains, as it is the part from which great supplies of tea in a dry or in a pickled state are brought. The Paloungs who cultivate it appear to be a quiet, unaggressive people; and they do not themselves bring their produce to the Burmese markets, but sell it to trading caravans of Shans and Panthays. In Kyatpyen the people claim to be of pure Burmese stock. They dress, however, in Shan costume of blue or white trousers and jackets, which is very unusual for the Burmese, whose ordinary costume resembles the Scotch kilt. In Mogok the permanent residents are Shans, but Burmanized. Separate communities of pure Chinese and of Mohammedan Chinese are found as permanent or as temporary residents. Beyond these principal peoples, we find in this small locality, attracted by its wealth and its markets, bodies of Mainthas and of Leesaws as temporary visitors. Although the Kachyens are near neighbours to the north, the powerful Shan State of Momeit prevents their irruption to the Ruby Mines. The Mainthas are either Chinese Shans of a different type from the main body, or are hill Chinese from the North-eastern Chinese Shan States. The Leesaws are hill-men of weaker physique, who occupy the mountain regions of Western Yunnan, and are found in isolated communities in the higher parts of the Northern Shan States. They are supposed to be of the same tribal origin as the Burmese; but to have been driven and kept in the more inhospitable hill tracts to the north.

ACCORDING to *Allen's Indian Mail*, Colonel Sartorius, of the 1st Beloochee Regiment, has made an interesting report on his recent journey through the Southern Shan and Red Karen country. At Saga iron ore is found in abundance. Tin is plentiful in Lower Kerennie, and coal at the Lowelon Mountain. Besides these, silver, sulphur, and saltpetre were also found. He describes Rosambhe Lake as being quite as beautiful as the lakes of Cashmere, and the Falls of Kazor, which are 130 feet in height, are perhaps the finest in the East.

OUR ELECTRICAL COLUMN.

LORDS CRAWFORD AND WANTAGE, Sir Coutts Lindsay, and others, have boldly thrown down the gauntlet to the gas people. They have taken ground at Deptford for a central station, and are going to supply electricity to London. They start with 200,000 lamps, and charge at the same rate as gas at 4*s*. 2*d*. per 1000 cubic feet.

THE Meteorological Society are promised a fine display of atmospheric electrical apparatus for exhibition at their meeting on March 20. Lightning protectors of all kinds will be shown.

THE introduction of the terms "magnetic resistance," and "magneto-motive force," as the analogues of electric resistance and electromotive force, with their ratios, magnetic flux and

electric current, is exercising the minds of electricians just now. Mr. Bosanquet has put it very clearly that when there is any opposition to a physical change of such a nature that it is the greater the greater the measure of the cause, and the less the measure of the effect, it is clearly a resistance; and in this sense the quotient of magneto-motive force (ampere-turns among practical men) by magnetic flux per unit area (magnetic induction) is clearly resistance. It must, however, not be forgotten, that magnetic permeability is the analogue of electrostatic capacity, and if we regard iron as the analogue of a dielectric or an insulator, the use of the term is wrong.

THE following relative figures of the cost of the production of 1000 watt-hours, the unit of electrical energy introduced by the Board of Trade, are given by Peukert in the *Centralblatt fur Electrotechnic.*

	s.	d.
Thermo-electric battery (gas)	33	4
Bunsen battery	3	2
Daniell ,, ...	2	2½
Dynamo (gas)	0	6¼
,, (steam)	0	2½

MENGARINI is continuing the work originated by Blaserna, by which the maturing of wine is considerably expedited by the passage of powerful currents through it.

HEIM (Hanover) has recently made some interesting measurements of the intensity of light emitted by various artificial sources of light in daily use:—

Lamps.	Candle-power.	Consumption per Candle per Hour.
Ordinary petroleum	15	3·65 grammes
Argand (gas)	21·9	10·9 litres
Welsbach (gas)	14·4	6·6 ,,
Wenham (gas)	28·4	8·77 ,,
Flat burner (gas)	16·9	14·8 ,,
Pieper arc, 6 mm.	377	·405 watts
Pilsen arc, 10 mm.	1420	·291 ,,
Siemens arc, 14 mm.	3830	·236 ,,
Siemens glow	16	3·25 ,,

VON LANG has measured the counter-electromotive force of an arc lamp, using 5 mm. carbons, and finds it 37 volts, or for Edlund's formula—

$$E = a + blC,$$

where a and b are constants, l the length of the arc, and C the current—

$$a = 35{\cdot}07,\ b = 1{\cdot}32,\ l = 2{\cdot}5 \text{ mm.},\ C = 5 \text{ amperes.}$$

He has found these constants for various other materials. Cross and Shepherd (Boston) had found this back electromotive force to be 39 volts. What is this so-called counter-electromotive force? Surely it is an abuse of terms.

MR. SHELFORD BIDWELL (Royal Society, March 1) is continuing his admirable researches on the changes produced by magnetism in the lineal dimensions of the different magnetic metals. He finds that iron, which first expands with the magnetizing force, soon reaches a maximum point, whence it retracts until it attains its original length; but, on still further increasing the magnetizing force, it contracts until it apparently reaches a minimum point, beyond which his means have not enabled him to proceed. Bismuth appears to continually expand; nickel to continually contract; whilst cobalt contracts, reaches a minimum point, and then expands, approaching its original length. Manganese steel was unaffected. His apparatus was so perfect and sensitive that he could read a variation of one hundred-thousandth of a millimetre.

PROFS. AYRTON AND PERRY have satisfactorily disposed of the question as to whether there is any difference in the light emitted by a glow-lamp when incandesced by alternate or direct currents. They find no difference. The same power (3·39 watts) applied gives the same light (one candle) in each case.

THE PRESIDENT'S ANNUAL ADDRESS TO THE ROYAL MICROSCOPICAL SOCIETY.[1]

RETROSPECT may involve regret, but can scarcely involve anxiety. To one who fully appreciates the actual, and above all the potential, importance of this Society in its bearing upon the general progress of scientific research in every field of physical inquiry, the responsibilities of President will not be lightly, whilst they may certainly be proudly, undertaken.

I think it may be now fairly taken for granted that, as this Society has, from the outset, promoted and pointed to the higher scientific perfection of the microscope, so now, more than ever, it is its special function to place this in the forefront as its *raison d'être*. The microscope has been long enough in the hands of amateur and expert alike to establish itself as an instrument having an application to every actual and conceivable department of human research: and whilst in the earlier days of this Society it was possible for a zealous Fellow to have seen, and been more or less familiar with, all the applications to which it then had been put, it is different to-day. Specialists in the most diverse areas of research are assiduously applying the instrument to their various subjects, and with results that, if we would estimate aright, we must survey with instructed vision the whole ground which advancing science covers.

From this it is manifest that this Society cannot hope to enfold, or at least to organically bind to itself, men whose objects of research are so diverse.

But these are all none the less linked by one inseverable bond; it is the microscope: and whilst, amidst the inconceivable diversity of its applications, it remains manifest that this Society has for its primary object the constant progress of the instrument—whether in its mechanical construction or its optical appliances; whether the improvements shall bear upon the use of high powers or low powers; whether it shall be improvement that shall apply to its commercial employment, its easier professional application, or its most exalted scientific use; so long as this shall be the undoubted aim of the Royal Microscopical Society, its existence may well be the pride of Englishmen, and will commend itself more and more to men of all countries.

This, and this only, can lift such a Society out of what I believe has ceased to be its danger, that of forgetting that in proportion as the optical principles of the microscope are understood, and the theory of microscopical vision is made plain, the value of the instrument over every region to which it can be applied, and in all the varied hands that use it, is increased without definable limit. It is therefore by such means that the true interests of science are promoted.

It is one of the most admirable features of this Society that it has become cosmopolitan in its character in relation to the instrument, and all the ever-improving methods of research employed with it. From meeting to meeting it is not one country, or one continent even, that is represented on our tables. Nay, more, not only are we made familiar with improvements brought from every civilized part of the world, referring alike to the microscope itself and every instrument devised by specialists for its employment in every department of research; but also, by the admirable persistence of Mr. Crisp and Mr. Jno. Mayall, Jun., we are familiarized with every discovery of the old forms of the instrument wherever found or originally employed.

The value all of this cannot be over-estimated, for it will, even where prejudices as to our judgment may exist, gradually make it more and more clear that this Society exists to promote and acknowledge improvements in every constituent of the microscope, come from whatever source they may; and, in connection with this, to promote by demonstrations, exhibitions, and monographs the finest applications of the finest instruments for their respective purposes.

To give all this its highest value, of course, the theoretical side of our instrument must occupy the attention of the most accomplished experts. We may not despair that our somewhat too practical past in this respect may right itself in our own country; but meantime the splendid work of German students and experts is placed by the wise editors of our Journal within the reach of all.

I know of no higher hope for this important Society than that it may continue in ever-increasing strength to promote, criticise, and welcome from every quarter of the world whatever will improve the microscope in itself and in any of its applications, from the most simple to the most complex and important in which its employment is possible.

There are two points of some practical interest to which I desire for a few moments to call your attention. The former has reference to the group of organisms to which I have for so many years directed your attention, viz. the "Monads," which throughout I have called "putrefactive organisms."

There can be no longer any doubt that the destructive process of putrefaction is essentially a process of fermentation.

[1] Delivered by the Rev. Dr. Dallinger, F.R.S., at the annual meeting of the Royal Microscopical Society, February 8, 1888.

The fermentative saprophyte is as absolutely essential to the setting up of destructive rotting or putrescence in a putrescible fluid as the torula is to the setting up of alcoholic fermentation in a saccharine fluid. Make the presence of torulæ impossible, and you exclude with certainty fermentive action.

In precisely the same way, provide a proteinaceous solution, capable of the highest putrescence, but absolutely sterilized, and placed in an optically pure, or absolutely calcined air; and while these conditions are maintained, no matter what length of time may be suffered to elapse, the putrescible fluid will remain absolutely without trace of decay.

But suffer the slightest infection of the protected and pure air to take place, or, from some putrescent source, inoculate your sterilized fluid with the minutest atom, and shortly turbidity, offensive scent, and destructive putrescence ensue.

As in the alcoholic, lactic, or butyric ferments, the process set up is shown to be dependent upon and concurrent with the vegetative processes of the demonstrated organisms characterizing these ferments; so it can be shown with equal clearness and certainty that the entire process of what is known as putrescence is equally and as absolutely dependent on the vital processes of a given and discoverable series of organisms.

Now it is quite customary to treat the fermentive agency in putrefaction as if it were wholly Bacterial, and, indeed, the putrefactive group of Bacteria are now known as Saprophytes, or saprophytic Bacteria, as distinct from morphologically similar, but physiologically dissimilar, forms known as parasitic or pathogenic Bacteria.

It is indeed usually and justly admitted that *B. termo* is the exciting cause of fermentive putrefaction. Cohn has in fact contended that it is the distinctive ferment of all putrefactions, and that it is to decomposing proteinaceous solutions what *Torula cerevisiæ* is to the fermenting fluids containing sugar.

In a sense, this is no doubt strictly true: it is impossible to find a decomposing proteinaceous solution, at any stage, without finding this form in vast abundance.

But it is well to remember that in Nature putrefactive ferments must go on to an extent rarely imitated or followed in the laboratory. As a rule the pabulum in which the saprophytic organisms are provided and "cultured," is infusions, or extracts of meat carefully filtered, and, if vegetable matter is used, extracts of fruit, treated with equal care, and if needful neutralized, are used in a similar way. To these may be added all the forms of gelatine, employed in films, masses, and so forth.

But in following the process of destructive fermentation as it takes place in large masses of tissue, animal or vegetable, but far preferably the former, as they lie in water at a constant temperature of from 60° to 65° F., it will be seen that the fermentive process is the work, not of one organism, nor, judging by the standard of our present knowledge, of one specified class of vegetative forms, but by organisms, which, though related to each other, are in many respects greatly dissimilar, not only morphologically, but also embryologically, and even physiologically.

Moreover, although this is a matter that will want most thorough and efficient inquiry and research to understand properly its conditions, yet it is sufficiently manifest that these organisms succeed each other in a curious and even remarkable manner. Each does a part in the work of fermentive destruction; each aids in splitting up into lower and lower compounds; the elements of which the masses of degrading tissue are composed; while apparently, each set in turn, does by vital action, coupled with excretion, (1) take up the substances necessary for its own growth and multiplication; (2) carry on the fermentive process; and (3) so change the immediate pabulum as to give rise to conditions suitable for its immediate successor. Now the point of special interest is that there is an apparent adaptation in the form, functions, mode of multiplication, and order of succession in these fermentive organisms, deserving of study and fraught with instruction.

Let it be remembered that the aim of Nature in this fermentive action is not the partial splitting of certain organic compounds, and their reconstruction in simpler conditions, but the ultimate setting free, by saprophytic action, of the elements locked up in great masses of organic tissue: the sending back into Nature of the only material of which future organic structures are to be composed.

I have said that there can be no question whatever that *Bacterium termo* is the pioneer of Saprophytes. Exclude *B. termo* (and therefore with it all its congeners) and you can obtain no putrefaction. But wherever, in ordinary circumstances, a decomposable organic mass, say the body of a fish, or a considerable mass of the flesh of a terrestrial animal, is exposed in water at a temperature of 60° to 65° F., *B. termo* rapidly appears, and increases with a simply astounding rapidity. It clothes the tissues like a skin, and diffuses itself throughout the fluid.

The exact chemical changes it thus effects are not at present clearly known; but the fermentive action is manifestly concurrent with its multiplication. It finds its pabulum in the mass it ferments by its vegetative processes. But it also produces a visible change in the enveloping fluid, and noxious gases continuously are thrown off.

In the course of a week or more, dependent on the period of the year, there is, not inevitably, but as a rule, a rapid accession of spiral forms, such as *Spirillum volutans*, *S. undula*, and similar forms, often accompanied by *Bacterium lineola:* and the whole interspersed still with inconceivable multitudes of *B. termo.*

These invest the rotting tissues like an elastic garment, but are always in a state of movement. These, again, manifestly further the destructive ferment, and bring about a softness and flaccidity in the decomposing tissues, while they without doubt, at the same time, have, by their vital activity and possible secretions, affected the condition of the changing organic mass. There can be, so far as my observations go, no certainty as to when, after this, another form of organism will present itself; nor, when it does, which of a limited series it will be. But, in a majority of observed cases, a loosening of the living investment of Bacterial forms takes place, and simultaneously with this, the access of one or two forms of my putrefactive monads. They were amongst the first we worked at; and have been, by means of recent lenses, amongst the last revised. Mr. S. Kent named them *Cercomonas typica*, and *Monas dallingeri* respectively. They are both simple oval forms, but the former has a flagellum at both ends of the longer axis of the body, while the latter has a single flagellum in front.

The principal difference is in their mode of multiplication by fission. The former is in every way like a Bacterium in its mode of self-division. It divides, acquiring for each half a flagellum in division, and then, in its highest vigour, in about four minutes, each half divides again.

The second form does not divide into two, but into many, and thus, although the whole process is slower, develops with greater rapidity. But both ultimately multiply—that is, commence new generations—by the equivalent of a sexual process.

These would average about four times the size of *Bacterium termo:* and when once they gain a place on, and about, the putrefying tissues, their relatively powerful and incessant action, their enormous multitude, and the manner in which they glide over, under, and beside each other, as they invest the fermenting mass, is worthy of close study. It has been the life-history of these organisms, and not their relations as ferments, that has specially occupied my fullest attention; but it would be in a high degree interesting if we could discover, or determine, what beside the vegetative or organic processes of nutrition are being effected by one, or both, of these organisms on the fast-yielding mass. Still more would it be of interest to discover what, if any, changes were wrought in the pabulum, or fluid generally; for after some extended observations I have found that it is only after one or other, or both, of these organisms, have performed their part in the destructive ferment, that subsequent and extremely interesting changes arise.

It is true that in some three or four instances of this saprophytic destruction of organic tissues, I have observed that, after the strong Bacterial investment, there has arisen, not the two forms just named, nor either of them; but one or other of the striking forms now called *Tetramitus rostratus*, and *Polytoma uvella;* but this has been in relatively few instances. The rule is that *Cercomonas typica*, or its congener, precedes other forms, that not only succeed them in promoting, and carrying to a still further point the putrescence of the fermenting substance, but appear to be aided in the accomplishment of this by mechanical means.

By this time the mass of tissue has ceased to cohere. The mass has largely disintegrated, and there appears amongst the countless Bacterial and monad forms, some one, and sometimes even three forms, that whilst they at first swim and gyrate, and glide about the decomposing matter, which is now, much less closely invested by *Cercomonas typica*, or those organisms that may have acted in its place, they also resort to an entirely new mode of movement.

One of these forms is *Heteromita rostrata*, which it will be remembered, in addition to a front flagellum, has also a long fibre, or flagellum-like appendage that gracefully trails as it swims. At certain periods of its life they anchor themselves in countless billions all over the fermenting tissues, and as I have described in the life-history of this form, they coil their anchored fibre, as does a Vorticellan, bringing the body to the level of the point of anchorage, then shoot out the body with lightning-like rapidity, and bring it down like a hammer on some point of the decomposition. It rests here for a second or two, and repeats the process; and this is taking place, by what seems almost like rhythmic movement all over the rotting tissue. The results are scarcely visible in the mass; but if a group of these organisms be watched, attached to a small particle of the fermenting tissue, it will be seen to gradually diminish, and at length to disappear.

Now, there are at least two other similar forms, one of which, *Heteromita uncinata*, is similar in action, and the other of which, *Dallingeria drysdali*, is much more powerful, being possessed of a double anchor, and springing down upon the decadent mass with, relatively, far greater power.

Now, it is under the action of these last forms, that in a period, varying from one month to two or three, the entire substance of the organic tissues disappears, and the decomposition has been designated by me "exhausted"; nothing being left in the vessel but slightly noxious, and pale gray water, charged with carbonic acid; and a fine, buff-coloured impalpable sediment at the bottom.

My purpose is not, by this brief notice, to give an exhaustive, or even a sufficient account, of the progress of fermentive action, by means of saprophytic organisms, on great masses of tissue: my observations have been incidental, but they lead me to the conclusion that the fermentive process is not only not carried through by what are called saprophytic Bacteria, but that a *series* of fermentive organisms arise, which succeed each other, the earlier ones preparing the pabulum or altering the surrounding medium, so as to render it highly favourable to a succeeding form. On the other hand, the succeeding form has a special adaptation for carrying on the fermentive destruction more efficiently from the period at which it arises, and thus ultimately of setting free the chemical elements locked up in dead organic compounds.

That these later organisms are saprophytic, although not Bacterial, there can be no doubt. A set of experiments recorded by me in the Proceedings of this Society some years since would go far to establish this (*Monthly Microscopical Journal*, 1876, p. 288). But it may be readily shown, by extremely simple experiments, that these forms will set up fermentive decomposition rapidly, if introduced in either a desiccated or living condition, or in the spore state, into suitable but sterilized pabulum.

Thus while we have specific ferments which bring about definite and specific results; and while even infusions of proteid substances may be exhaustively fermented by saprophytic Becteria; the most important of all ferments, that by which Natura's dead organic masses are removed, is one which there is evidence to show is brought about by the successive vital activities of a series of adapted organisms, which are for ever at work in every region of the earth.

There is one other matter of some interest and moment, on which I would say a few words. To thoroughly instructed biologists, such words will be quite needless; but, in a Society of this kind, the possibilities that lie in the use of the instrument are associated with the contingency of large error, especially in the biology of the minuter forms of life, unless a well-grounded biological knowledge form the basis of all specific inference, to say nothing of deduction.

I am the more encouraged to speak of the difficulty to which I refer, because I have reason to know that it presents itself again and again in the provincial Societies of the country, and is often adhered to with a tenacity worthy of a better cause. I refer to the danger that always exists, that young or occasional observers are exposed to, amidst the complexities of minute animal and vegetable life, of concluding that they have come upon absolute evidences of the transformation of one minute form into another; that in fact they have demonstrated cases of heterogenesis.

This difficulty is not diminished by the fact that on the shelves of most Microscopical Societies there is to be found some sort of literature written in support of this strange doctrine.

You will pardon me for allusion again to the field of inquiry in which I have spent so many happy hours. It is, as you know, a region of life in which we touch, as it were, the very margin of living things. If Nature were capricious anywhere, we might expect to find her so here. If her methods were in a slovenly or only half-determined condition, we might expect to find it here. But it is not so. Know accurately what you are doing, use the precautions absolutely essential, and through years of the closest observation, it will be seen that the vegetative and vital processes generally, of the very simplest and lowliest life-forms, are as much directed and controlled by immutable laws, as the most complex and elevated.

The life-cycles, accurately known, of monads, repeat themselves as accurately as those of Rotifers or Planarians.

And of course, on the very surface of the matter the question presents itself to the biologist why it should not be so. The irrefragable philosophy of modern biology is that the most complex forms of living creatures have derived their splendid complexity and adaptations from the slow and majestically progressive variation and survival from the simpler and the simplest forms. If, then, the simplest forms of the present and the past were not governed by accurate and unchanging laws of life, how did the rigid certainties that manifestly and admittedly govern the more complex and the most complex come into play?

If our modern philosophy of biology be, as we know it is, true, then it must be very strong evidence indeed that would lead us to conclude that the laws seen to be universal break down and cease accurately to operate, where the objects become microscopic, and our knowledge of them is by no means full, exhaustive, and clear.

Moreover, looked at in the abstract, it is a little difficult to conceive why there should be more uncertainty about the life-processes of a group of lowly living things, than there should be about the behaviour, in reaction, of a given group of molecules.

The triumph of modern knowledge is the certainty which nothing can shake, that Nature's laws are immutable. The stability of her processes, the precision of her action, and the universality of her laws, is the basis of all science; to which biology forms no exception. Once establish, by clear and unmistakable demonstration, the life-history of an organism, and truly some change must have come over Nature as a whole, if that life-history be not the same to-morrow as to-day; and the same to one observer, in the same conditions, as to another.

No amount of paradox would induce us to believe that the combining proportions of hydrogen and oxygen had altered, in a specified experimenter's hands, in synthetically producing water.

We believe that the melting-point of platinum and the freezing-point of mercury are the same as they were a hundred years ago, and as they will be a hundred years hence.

Now, carefully remember that so far as we can see at all, it must be so with life. Life inheres in protoplasm; but just as you cannot get *abstract matter*—that is, matter with no properties or modes of motion—so you cannot get *abstract* protoplasm. Every piece of living protoplasm we see has a history: it is the inheritor of countless millions of years. Its properties have been determined by its history. It is the protoplasm of some definite form of life which has inherited its specific history. It can be no more false to that inheritance than an atom of oxygen can be false to its properties.

All this, of course, within the lines of the great secular processes of the Darwinian laws; which, by the way, could not operate at all if caprice formed any part of the activities of Nature.

But let me give a practical instance of how, what appears like fact, may over-ride philosophy, if an incident, or even a group of incidents, *per se* are to control our judgment.

Eighteen years ago I was paying much attention to Vorticellæ. I was observing with some pertinacity *Vorticella convallaria*; for one of the calices in a group under observation, was in a strange and semi-encysted state, while the remainder were in full normal activity.

I watched with great interest and care, and have in my folio still the drawings made at the time. The stalk carrying this individual calyx fell upon the branch of vegetable matter to which the Vorticellan was attached, and the calyx became perfectly globular; and at length there emerged from it a small form with which, in this condition, I was quite unfamiliar: it was small, tortoise-like in form, and crept over the branch on setae or hair-like pedicels; but, carefully followed, I found it soon swam, and at length got the long neck-like appendage of *Amphileptus anser!*

Here then was the cup or calyx of a definite Vorticellan form, changing into (?) an absolutely different Infusorian, viz. *Amphileptus anser!*

Now I simply reported the *fact* to the Liverpool Microscopical Society, with no attempt at inference; but two years after I was able to explain the mystery, for, finding in the same pond both *V. convallaria* and *A. anser*, I carefully watched their movements, and saw the *Amphileptus* seize and struggle with a calyx of *convallaria*, and absolutely become encysted upon it, with the results that I had reported two years before.

And there can be no doubt but this is the key to the cases that come to us again and again of minute forms suddenly changing into forms wholly unlike. It is happily amongst the virtues of the man of science to "rejoice in the truth," even though it be found at his expense; and true workers, earnest seekers for Nature's methods, in the obscurest fields of her action, will not murmur that this source of danger to younger microscopists has been pointed out, or recalled to them.

And now I bid you as your President farewell. It has been all pleasure to me to serve you. It has enlarged my friendships and my interests; and although my work has linked me with the Society for many years, I have derived much profit from this more organic union with it; and it is a source of encouragement to me, and will, I am sure, be to you, that, after having done with simple pleasure what I could, I am to be succeeded in this place of honour by so distinguished a student of the phenomena of minute life as Dr. Hudson. I can but wish him as happy a tenure of office as mine has been.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. x. No. 2 (Baltimore, January 1888).—In the opening paper (pp. 99–130), entitled "Soluble Quintic Equations with Commensurable Coefficients," G. P. Young develops at some length the application of his general method, described in vol. vi., to the solution of twenty quintic equations, such as $x^5 - 10x^3 - 20x^2 - 1505x - 7412 = 0$.—Mr. D. Barcroft discusses (pp. 131–40) forms of non-singular quintic curves. The subject is profusely illustrated by drawings of 47 curves on twelve large pages (interpolated between pp. 140 and 141).—F. Morley (pp. 141–48) writes on critic centres in cubics.—The expression of syzygies among perpetuants by means of partitions, by Captain P. A. MacMahon, R.A. (pp. 149–68), is a very interesting addition to the author's previous papers on the subject.—The number concludes with three short papers: "Démonstration directe de la formule Jacobienne de la transformation cubique," by the Abbé Faà de Bruno; note on geometric inferences from algebraic symmetry, by F. Morley; and "Surfaces telles que l'origine se projette sur chaque normale au milieu des centres de courbure principaux" (pp. 175–86), by P. Appell.

Rivista Scientifico-Industriale, January 31.—On chemical valency, by Prof. Fr. Mangini. The probable cause of valency, that is, the varying proportions with which the atoms of the simple bodies combine with hydrogen, or its equivalent chlorine, to form molecules, is here attributed to the varying degrees of motion assumed to be pre-existent and inherent in the atoms themselves. A numerical coincidence is pointed out between the acoustic, luminous, and chemical phenomena, seven being the number of the chief musical notes, of the chief colours in the spectrum, and, as is now generally admitted, of the chemical valencies. It is further to be noted that the temperature required to produce the spectral lines varies with the valencies of the different elements. Thus, a much higher temperature is required for the polyvalent than for the monovalent alkalines, and in all these phenomena a connection is seen to exist between the heat required to show the spectral lines and the quantivalence of the atoms. Another nexus is found between the allotropic state and the number of vibrations needed to produce the spectroscopic phenomena. This highly suggestive paper will be continued in a future number of the *Rivista*.

Bulletins de la Société d'Anthropologie de Paris, tome x. fasc. 3 (Paris, 1887).—On the various methods of measuring the thorax, by Dr. E. Maurel. The writer, in enumerating the various instruments in use for this purpose, gives the preference to those designed by MM. Woillez, Niely, and Fourmentin, by which a graphic representation of the dimensions of the chest is obtained; although he claims to have improved upon their design in an instrument to which he has given the name stethograph.—On a Breton amulet, called "Kistin Spagn," by M. Bonnemère. Under this name the people of Locmariaque treasure a seed, probably a cashew nut, or, according to some, the seed of the mahogany-tree, which is brought home by Breton sailors. The nut is carefully scraped and boiled in new milk, when it is supposed to be a sovereign remedy against intestinal disorders. By some of the peasant women, however, the nut is pierced and worn on a chain, with their keys, scissors, &c., as an amulet. Singularly enough, it is found that even in Paris these nuts are believed to be specifics against various diseases, more especially the gout, three or four when carried in the trousers pocket being regarded as capable of warding off this malady.—On calves born with so-called bull-dog heads, by M. Dareste. Animals of this description were at one time characterized in South America as constituting a distinct race, but the gradual diminution in their numbers since the cattle of the pampas have acquired a marketable value leads to the inference that they are being killed when first dropped, in order to eliminate deformed animals from the herds, and this opinion of the deformity of the so-called "natos-calves" is confirmed by the presence of other abnormalities in all the animals of this description which have been examined in Europe.—On the colour of the hair and eyes in Limagne, near the Monts-de-Dôme, by Dr. Pommerol. These observations refer to 200 individuals, and appear to indicate that, taken generally, one-fourth of the population have light hair, and three-fourths dark hair, while light and dark eyes are equally frequent.—On the worship of Taranis in popular traditions of Auvergne, by Dr. Pommerol. The writer believes that under this name we have the Gallic representative of the supreme god of the heavens, and wielder of thunder and storms; and that the custom still prevalent in France of building an uncut stone into the gable or roof-top of a house, or hammering into the newly finished walls an irregularly formed metal, wooden, or stone cross, or mallet, to keep bad luck from the building, is a survival of the ancient usage of averting evil by the help of emblems connected with the worship of the supreme gods, as Baal's stone, Jupiter's thunderbolt, or Thor's hammer.—Circumcision in its social and religious significance, by M. Lafargue. The fact that this rite was practised among the Egyptians long before its adoption by the Hebrews has led to the inference that its practice was due to hygienic considerations only. But the author believes that we have here merely one of the numerous forms of mutilations submitted to by primæval men with a view of propitiating their deities, and of which we have such varied and striking evidence among different peoples, as the Assyrians and Aztecs, as well as among the black races; while survivals of similar faith in the efficacy of voluntarily inflicted suffering and mutilation are to be traced in the mythology of the Greeks and Romans.—On the influence of their surrounding medium on the peoples of Central Asia, by M. de Ujfalvy. Referring to the services recently rendered to science by Richthofen in unravelling the tissue of misconceptions in regard to the geognosy of Central Asia, due to the theories of Humboldt, Klaproth, and others, the writer considers the influence which the soil and their surroundings have had on the inhabitants of the four distinct zones into which the first-named of these *savants* has subdivided the Asiatic continent. Thus, while the central zone, by the general levelling of the surface through the chemical disintegration of the rocks, and the absence of streams to enrich the soil, compels men to follow a nomadic, or pastoral, rather than a settled life, the peripheral zone abounds in rich and fertile lands, yielding abundant opportunities for the exercise of human industry, and a corresponding advance in mental and social development. The intermediate zones correspond ethnographically with the transitional character of their geognostic features. Next to the extraordinary influence of the varied configurations of Asia on the destinies of its inhabitants, M. de Ujfalvy points out the importance of loess formations as factors in determining the spread and establishment of civilization. This part of the subject is treated at great length, and deserves the careful attention of the palæologist no less than the student of ethnography, seeing that the loess constitutes an important agent in the preservation of the animal and industrial remains of prehistoric ages.—On the nervous system, considered from a physico-chemical point of view, by Dr. Fauvelle. Here the nervous system of man is regarded as a physical apparatus, presenting certain analogies with an electric pile.—Anthropology, and philology, with reference to the

Philippines, by M. O. Beauregard. This is a lengthy treatise on the products, language, sociology, and history of the islands, based chiefly on Spanish authorities.—Report, by M. Topinard, of the excavation of the Neolithic grotto of Feigneux (Oise), in which was found a skull that had been trepanned both before and after death. These finds were specially rich, including four skulls which bore traces of having been compressed; and, considered generally, this deposit may be regarded as a pendant to that of Orrouy.—(1) On a burial ground of the Stone Age at Crécy-en-Brie; (2) on cut flints in the alluvial sand below Paris; and (3) on a prehistoric work-place at Fontenay-aux-Roses, by M. Thieullen. The writer draws attention to the frequency with which the larger debris of cut flints are found near water, and always in localities favourable to the existence of prehistoric man, while from the character of the great ossuaries, in which, as at Crécy-en-Brie, the remains of men and women of all ages, and children, are found, he believes we may assume that the men of the period lived in family rather than in tribal association.—A study of the brain of Bertillon, by MM. Chudzinski and Manouvrier. A *résumé* of the results of this carefully conducted cerebral analysis, which are here given in detail, shows generally, *inter alia*, a large development of the anterior portion of the brain in all directions; a relatively inferior development in point of size in the temporal lobes, and in the cerebellum; and great ramification in the fossæ.

THE *Izvestia* of the Russian Geographical Society (xxiii. part 5) contains, besides Dr. Bunge's preliminary report about his expedition to the New Siberia Islands, a lecture on the problems of scientific geography by Dr. Petri, who was appointed in October last Professor of Geography and Anthropology at the St. Petersburg University; a paper by M. Rovinsky on the beliefs of the Montenegrins; M. Nikolsky's sketch of fishing on Lake Aral, a valuable contribution to the knowledge of the fishes inhabiting Lake Aral, and especially the lower Amu-daria, their habits, and the modes of fishing; and notes by General Stebnitsky on recent pendulum observations, on M. Boguslavsky's work on the Volga, and on W. J. Havenga's map of Sumatra.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 2.—"On Tidal Currents in the Open Ocean." By J. Y. Buchanan, F.R.S.

This paper gives details of some current observations which I made in the open ocean north of the Canary Islands in October 1883 in the course of the surveying expedition preliminary to the laying of a telegraph cable between these islands and the mainland of Spain. This expedition consisted of two steamers, the *Dacia* and the *International*, belonging to the India-rubber, Gutta-percha, and Telegraph Works Company (Limited), of Silvertown. The chief scientific results gained during it were the confirmation of the view—which was suggested by the density and temperature of the bottom water observed in this part of the Atlantic during the cruise of the *Challenger*—that the overflow of warm concentrated sea-water from the Mediterranean at the bottom of the Straits of Gibraltar was the cause of the abnormally high density and temperature of the bottom water in this part of the ocean, and the preparation of a complete survey of the bed of the ocean in this district. During the progress of the work several very remarkable "oceanic shoals" were discovered and surveyed, notably the "Coral Patch" in lat. 34° 57′ N., long. 11° 57′ W., with a depth of 400 to 500 fathoms, and the "Dacia Bank," in lat. 31° 9′ N., long. 13° 34′ W., with a minimum depth of 49 fathoms. In sounding over both of these banks conclusive evidence was obtained of the existence of actual vertical precipices in some positions on their flanks; and from the very great average steepness all round, it is rendered in every way probable that, if they were laid dry, they would form mountain peaks as precipitous and inaccessible as any to be found on land. The dredging on the Coral Patch showed it to consist of deep-sea corals, principally *Lophohelia prolifera*, growing with the utmost luxuriance and attached to dead stems of the same species, already getting coated with peroxide of manganese.

For the purposes of the survey of the "Dacia Bank" a buoy was anchored on its edge, and on the afternoon of October 21 I spent some hours in a boat made fast to it, and observed the current in strength and direction. The following is a summary of the results:—

Hour p.m.	2.15	2.40	3.30	4.6
Direction (true) ...	N. 11° E.	N. 41° E.	N. 56° E.	N. 101° E.
Rate (knots per hour)	0·47	0·30	0·26	0·30

It will be seen from these observations that in two hours the current had shifted its direction through 90°, and had passed through a minimum velocity of 0·26 per hour without there having been any period of "slack water." The observations are too few in number to make it worth while submitting them to analysis; but a little study of them will show that they indicate a current which is the resultant of a continuous current and a periodic one. A constant current running south-east by east, combined with a tidal current running north-north-west and south-south-east, the maximum velocity of which, in either direction, is twice that of the permanent current, would give a resultant agreeing fairly with that observed.

No measurements were made of the under current, but, by sinking a tow-net made fast to a sounding-line, it was seen to be running at a depth of 75 fathoms in the same direction as the surface current and apparently with much the same velocity. In the channels between the Canary Islands, where even on the shallowest ridges there is over 1000 fathoms of water, the tidal current reaches to the very bottom, and its scouring action is shown by the nature of the bottom. To seaward, in 1800 or 2000 fathoms, the bottom is a fine Globigerina ooze, which gets coarser and sandier as the water shoals in the channels, till on the summit ridge there is generally no loose deposit at all, and the bottom is rock or coral coated with black oxide of manganese. Round the western end of Teneriffe the tide runs violently, causing rips and overfalls. Much rocky ground is met with in the North Atlantic in depths of 1300 and 1400 fathoms, especially on the ridge which appears to extend through the whole length of that ocean. It is not unlikely that the summit edge of this ridge may be swept clean through the greater part of its length, and it must be remembered that the removal of sediment from one part of the ocean bottom means its deposit in greater abundance in others, especially in hollows in the neighbourhood of the ridge. Hence a sounding in "ooze" or "mud" in one position furnishes no argument against the trustworthiness of another sounding in the vicinity and in equally deep water on "rock" or "hard ground."

It is evident, then, that the power of shoals to transform the tidal wave into tidal currents furnishes a natural agency which tends to limit the indefinite shoaling of the water by the continual deposition of loose sediment. On the other hand, these currents, in sweeping clean the rocky eminences at the bottom of the ocean, prepare a lodging-place for deep-sea corals, and assist in bringing food to them when settled, thus enabling them to build up their pillar-like banks, of which a very fine example is furnished by the "Coral Patch" above referred to. There can be little doubt that it is reducing more or less rapidly the depth of the water above it. The "Dacia Bank" and the "Seine Bank" are examples where limiting conditions, probably of temperature, appear to have been reached. The water may be too warm for the deep-sea species; and not warm enough for the tropical, *par excellence*, reef-building species.

A remarkable cluster of banks resembling those above described occurs off the Brazilian coast, between the Agulhas reef and the islands of Trinidad and Martin Vaz Some of them are named, as the Jaseur, the Montague, and the Victoria banks; with from 25 to 30 fathoms, and completely surrounded by deep water. Further north is the dangerous Rocas, lying close to the route of steamers from North America and Europe to South American ports. Further south, again, are two suggestive soundings, one of 19 fathoms, in lat. 32° 40′ S., long. 47° 0′ W., marked "Nelson, 1859," and the other of 72 fathoms, in lat. 37° 50′ S., long. 49° 50′ W., marked "Sutlej, 1863," in the chart. Seamen are not usually mistaken as to whether they have or have not found bottom in depths such as 19 or 72 fathoms, and there is little doubt that careful search would reveal the existence of shoals in these localities. But the search must be diligent and methodical, always following the lead of the soundings as they shoal. The careful and detailed study of these oceanic shoals or embryo islands is of great importance for oceanographical science, and it would not be easy to find more interesting work for the marine surveyor.

March 1.—"On the Changes produced by Magnetization in the Dimensions of Rings and Rods of Iron and of some other Metals." By Shelford Bidwell, F.R.S.

Linnean Society, February 16.—W. Carruthers, F.R.S., President, in the chair.—Mr. Spencer Moore exhibited, and made some remarks upon, specimens illustrative of the *Palmella* state *Draparnaldia glomerata*.—Mr. D. Morris (Royal Gardens, Kew) exhibited a specimen of wood of *Hieronyma alchornioides* received from Trinidad, showing in its fissures mineral deposits, which on chemical analysis proved to be calcic carbonate. For comparison, Mr. Morris also exhibited and made some observations upon some deposits of calcic phosphate in teak. Some of these (described by Sir Fred. Abel, Quart. Journ. Chem. Soc. xv. 91), are 6 feet in length, 6 inches in breadth, and from $\frac{1}{8}$ inch to $\frac{3}{8}$ inch in thickness. Deposits in bamboo known as *tabasheer* (silicate) were shown, as also pearls (carbonate of lime) from cocoanuts, received from Dr. Sydney T. Hickson (see NATURE, vol. xxxvi. p. 157). All these specimens were from the Museum of Economic Botany of Kew.—Dr. Burn Murdock exhibited and offered remarks upon the intra-marginal (so-called) veins in the section *Areolata* of the genus *Erythroxylon*, of which *E. coca* is the most familiar species. These lines are due to a thickening of the parenchymatous tissue which takes place in the bud stage, and are in no way connected with the venation of the leaf.—Mr. G. F. Sherwood exhibited a collection of photographs taken in Samoa, illustrating the scenery and people, together with a number of necklets formed with strings of various bright-coloured seeds.—The first paper of the evening was read by Mr. H. N. Ridley, on self-fertilization and cleistogamy in orchids. Three common methods of self-fertilization were explained: (1) by the breaking up of the pollen mass, and falling of the dust either directly upon the stigma, or into the lips whence it comes into contact with the stigma; (2) by the falling of the pollen masses as a whole from the clinandrum into the stigma; and (3) by the falling forward of the pollinia from the clinandrum, or the anther cap, the caudicle and gland remaining attached to the column. An interesting discussion followed, in which Prof. Marshall Ward, the Rev. G. Henslow, and Mr. A. W. Bennett took part.—A paper was then read by Dr. John Rae, F.R.S., entitled "Notes on some of the Birds and Mammals of Hudson's Bay Territory." Dr. Rae, whose long residence in Northern and Arctic America enabled him to speak authoritatively from personal observation, gave an interesting account of the migration of the Canada goose, snow goose, and blue-winged goose, and of the habits of the American hare and lemming. He particularly referred to the belief entertained by some of the Indian tribes he had met with, and to which he himself gave credence, that certain species of small birds are assisted on their migrations by being carried on the backs of the Canada geese. Mr. J. E. Harting, in criticising this paper, gave an exposition of the views held by leading ornithologists on the subject of the American Canada and snow geese, their relationship and nomenclature, and pointed out that the story of small birds being carried by larger ones is not confined to North America, but is current in South-Eastern Europe, Palestine, and Arabia, where trustworthy evidence has been obtained that wagtails and other small birds travel on the backs of cranes. He added that one instance was known to him of such an occurrence in England, a short-eared owl having been seen to arrive on the north coast of Yorkshire carrying on its back a golden-crested wren, which was secured by the observer.

Chemical Society, February 16.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—Chemical investigation of Wackenroder's solution, and explanation of the formation of its constituents, by Prof. Debus, F.R.S. Wackenroder's solution is obtained by passing hydrogen sulphide into an aqueous solution of sulphur dioxide until the latter is decomposed. It has been considered to contain sulphur in suspension and pentathionic acid in solution, although neither the acid nor its salts have been prepared pure, and, in consequence, Spring has denied the existence of the acid, regarding it as a solution of sulphur in tetrathionic acid. The author finds that Wackenroder's solution contains: (1) sulphur in suspension in very minute drops, (2) a new allotropic modification of sulphur, (δ), in simple solution, and in the colloidal condition, (3) traces of trithionic acid, (4) tetrathionic acid, (5) pentathionic acid, and (6) a polythionic acid containing more sulphur than the penta- acid, probably hexathionic acid. Pure potassium and copper pentathionates were prepared, and the reactions of the polythionates studied, among the most interesting of which are the spontaneous changes in aqueous solution shown by the equations—

$$(a)\ K_2S_5O_6 = K_2S_4O_6 + S\,;\ (b)\ 2K_2S_4O_6 = K_2S_3O_6 + K_2S_5O_6\,;$$
$$(c)\ 2K_2S_3O_6 = K_2S_4O_6 + K_2SO_4 + SO_2\,;\ (d)\ 3K_2S_3O_6 = K_2S_5O_6 + 2K_2SO_4 + 2SO_2\,;$$

the reactions (*a*) and (*b*) occurring in either direction with equal facility. The final products of the action of hydrogen sulphide on tetra- and penta-thionic acids are water and sulphur. The polythionic acids can also be obtained by the action of sulphur dioxide on potassium thiosulphate or on the chlorides of sulphur. The concluding portion of the paper was devoted to a discussion of the formulæ of the polythionates.—Potilizin's law of the mutual displacement of chlorine and bromine, by Prof. Thorpe, F.R.S., and Mr. J. W. Rodger. On heating bromine with an equivalent quantity of an anhydrous metallic chloride in a sealed glass tube, free from air, to the temperature of the melting-point of zinc, Potilizin found that the amount of chlorine displaced by bromine was greater the higher the atomic weight of the metal in the chloride; and further, that, if A be the atomic weight of the metal, p the percentage of chlorine displaced from its chloride when treated as above, and E its valency, the formula $\frac{A}{pE^2}$ = a constant held good in the case of fourteen chlorides. To test the validity of this law, the authors heated the chlorides of sodium, potassium, silver, strontium, barium and lead with bromine at 350°–450°, and found that, with the exception of silver chloride, in which the deviation was not so marked, the amount of chlorine displaced was considerably less than that required by Potilizin's law, and in all cases stood in no definite relation either to the duration of heating or to the atomic weight of the metal of the chloride used, although most chlorine was displaced from the chloride of highest molecular weight when several were heated simultaneously. These experiments therefore disprove the validity of Potilizin's law.—A gasometric method of determining nitrous acid, by Dr. P. F. Frankland. Based on the interaction of urea and nitrous acid.—The action of some specific micro-organisms on nitric acid, by the same. The author has investigated the behaviour, when grown in nutritive solutions containing nitrates, of a number of micro-organisms obtained from air and water, and cultivated in a state of purity. Of thirty-two different forms so examined, sixteen or seventeen, and particularly *Bacillus ramosus* and *B. pestifer*, were found to reduce the nitrate to nitrite more or less completely, whilst the remainder were quite destitute of this power. The behaviour of the organisms was not altered in this respect by excluding air from the solutions in which they were cultivated.—The action of phosphorus pentachloride on salicylaldehyde, by Mr. C. M. Stuart.—Some interactions of nitrogen chlorophosphuret, by Mr. W. Couldridge.—Action of alcohols on ethereal salts in presence of small quantities of sodic alkylate, by Prof. Purdie and Mr. W. Marshall.—Note on the densities of cerium sulphate solutions, by Dr. B. Brauner. The values of the densities of solutions of the anhydrous and of the hydrated salt are identical for solutions of equal concentration.

ERRATUM.—P. 406, second column, line 9 (from top), *for* $v = (n^2 - 1)(n^2 + 2)$ *read* $v = (n^2 - 1)/(n^2 + 2)$.

Physical Society, February 25.—Prof. Reinold, F.R.S., President, in the chair.—The following papers were read:—Note on the efficiency of incandescent lamps with direct and alternating currents, by Prof. W. E. Ayrton, F.R.S., and Prof. J. Perry, F.R.S. This relates to the question whether the "efficiency" (candles per watt) is greater or less for alternating than for direct currents. Experiments made by Messrs. Shepherd and Wheatley, two of the students at the Central Institution (to whom the authors express their thanks for the valuable assistance rendered) show that no appreciable difference can be detected when the lamp is at the same candle-power. In performing the experiments, three-way switches in connection with Gramme and Ferranti machines were arranged so that the current through the lamp could be quickly changed from direct to alternating, or *vice versâ*, adjustable resistances having been previously placed in the two circuits to give equal readings on a Cardew voltmeter placed as a shunt to the lamp. The currents were measured by a reflecting dynamometer wound with fine wire in order to make the error, due to unequal current density over the section, negligible. The problem has also been investigated from

theoretical considerations, but the results as yet deduced would not lead the authors to anticipate the equal efficiency found experimentally. An interesting discussion followed, in which Mr. Swinburne, Prof. S. P. Thompson, Mr. Boys, and the authors took part.—Observations of the height, length, and velocity of ocean waves, by the Hon. Ralph Abercromby. Several sets of observations were made by the author in the South Pacific in 1885. The heights were measured by a sensitive ameroid, and the length and velocity by a chronograph, assuming the length and speed of the vessel to be known. The largest waves observed in a heavy sea gave a height of 46 feet, length 765 feet, velocity 47 miles per hour, and time period of 16·5 secs. Great discrepancies exist between the results of different observers, which the author believes to be chiefly due to the comparative rarity of well-defined simple waves. Replying to a question from Mr. Baily, the author said the effect on the barometer of the difference of wind pressure on the two sides of a wave was negligible.—On the temperature at which nickel begins suddenly to lose its magnetic properties, by Mr. Herbert Tomlinson. Different authorities give different values, ranging from about 300° to 400° C. In investigating the subject the author found that the said temperature depends on the magnetizing force used; *e.g.* with magnetizing forces of 5, 99, and 182 units, the temperatures at which the permeability attained its maxima were 287° C., 248° C., and 242° C., and those corresponding to permeability = 0 were 333°, 392°, and 412° respectively. From the above results it will be seen that for small magnetizing forces the change of permeability from maximum to 0 is much more sudden than for the greater forces. As in iron, the permeability decreases as the magnetizing force increases. An experiment was shown in which a nickel plated brass wire was heated to dull redness whilst suspended between the poles of an electro-magnet, and allowed to cool. When the critical temperature was attained, the wire was suddenly attracted to one or other of the poles. In reply to Mr. Shelford Bidwell, the author stated that the changes in permeability due to ordinary atmospheric changes of temperature were considerable, when small magnetizing forces were used.—Experiments on electrolysis, by Mr. W. W. Haldane Gee, Mr. H. Holden, and Mr. C. H. Lees. Whilst studying some electrolytic polarization phenomena with palladium electrodes in dilute sulphuric acid (pure), a dense liquid was seen *after reversing the current* to flow downwards in streaks from the anode. The paper is devoted to the investigation of the character of the liquid streaks, and the authors conclude that the streaks are of concentrated sulphuric acid, formed by the union of the hydrogen (occluded by the electrode whilst serving as cathode) with the SO_4 liberated at the same electrode when the current is reversed. Similar streaks were found with phosphoric acid, &c. In their next paper the authors hope to describe some experiments in which these and similar effects become of great importance in changing the resistances of electrolytes.

Zoological Society, February 21.—Prof. W. H. Flower, F.R.S., President, in the chair.—Mr. A. Thomson exhibited a series of insects reared in the insect-house in the Society's Gardens during the past year, and read a report on the subject.—Prof. G. B. Howes read a note on the azygos veins of the Anurous Amphibia. The author described an individual specimen of *Rana temporaria*, in which the azygos vein (prerenal portion of the posterior cardinal) had been retained on one side, its relations differing in important details from that observed by Hochstetter in *Bombinator*. By way of supplementing that author's work, he had examined examples of a few genera not dealt with by Hochstetter. He recorded the presence of these veins in the only specimen of *Discoglossus* dissected, and in one of five individuals of *Alytes obstetricans*—facts which lent additional support to the views of Cope and Boulenger of the lowly affinities of the Discoglossidæ. He had failed to detect these vessels in the *Aglossa*; while he regarded their total absence in *Pelobates* and *Pelodytes* as fresh evidence of the Pelobatoid rather than the Discoglossid affinities of the last-named genus.—Mr. A. Smith-Woodward read the second part of his palæontological contributions to Selachian morphology.—Mr. Oldfield Thomas gave an account of the mammals obtained by Mr. G. F. Gaumer on Cozumel and Ruatan Islands, Gulf of Honduras.—A second paper by Mr. Thomas contained the description of a new and interesting annectent genus of Muridæ, based on a specimen which had been in the Paris Museum for some years. This was supplemented with remarks on the relation of the Old and New World members of the family.—Dr. G. H. Fowler exhibited and made some remarks on a new *Pennatula* from the Bahamas, the most interesting feature of which was the presence of immature antozooids at the dorsal end of the leaves, devoid of tentacles, but possessing a well-marked syphonoglyphe on the stomatidæum which disappears with the increasing age of the polyp. The species was proposed to be named *Pennatula bellissima*.

Royal Meteorological Society, February 15.—Dr. W. Marcet, F.R.S., President, in the chair.—The following papers were read:—Electrical and meteorological observations on the Peak of Teneriffe, by the Hon. Ralph Abercromby. The author made a trip to the Island of Teneriffe in October 1887, for the purpose of making some electrical and meteorological observations, and now gives some of the results which he obtained, which may be summarized as follows:—The electrical condition of the Peak of Teneriffe was found to be the same as in every other part of the world. The potential was moderately positive, from 100 to 150 volts, at 5 feet 5 inches from the ground, even at considerable altitudes; but the tension rose to 549 volts on the summit of the Peak, 12,200 feet, and to 247 volts on the top of the rock of Gayga, 7100 feet. A large number of halos were seen associated with local showers and cloud masses. The necessary ice-dust appeared to be formed by rising currents. The shadow of the Peak was seen projected against the sky at sunset. The idea of a south-west current flowing directly over the north-east Trade was found to be erroneous. There was always a regular vertical succession of air currents in intermediate directions at different levels from the surface upwards, so that the air was always circulating on a complicated screw system.—Rainfall of South Africa, 1842–1886, by Mr. W. B. Tripp. The author gives the rainfall statistics from all those stations situated in South Africa which possess records of ten complete years and upwards. He remarks upon the chronological succession of wet and dry years, and the consecutive years above and below the mean; and also describes the seasonal distribution of monthly maxima, and the extent over which monthly rains prevail. He concludes by comparing the curves of rainfall with those of sunspot energy.—Some methods of cloud measurements, by Mr. Nils Ekholm. As exact cloud measurements afford almost the only easily available means of determining motions in the upper regions of the atmosphere, the author describes some methods which seem to him likely to give the best results. He also details the plans adopted at the Swedish Polar Station, Cap Thorsden, in Spitzbergen, and at the Upsala Observatory, for determining the direction and angular velocity of the clouds, and for making direct measurements of the height and absolute motions of the clouds.

EDINBURGH.

Royal Society, January 30.—The Rev. Prof. Flint, D.D., Vice-President, in the chair.—Prof. Nicholson read a paper on the causes of movements in general prices.—Prof. J. B. Haycraft and Dr. E. W. Carlier gave a demonstration of a method by which human blood may be withdrawn from the body and its fluidity preserved. Castor-oil is the medium in which the blood is suspended. The finger from which the blood is obtained is greased and plunged in the oil before the puncture is made, every precaution being taken to prevent contact of the blood with the air or with solid matter. In this way the blood may be preserved in a fluid state for a considerable time. As the drops of blood settle slowly in the oil, the corpuscles are seen to fall to the lower part of the drops, while the clear plasma remains above. Prof. Haycraft and Dr. Carlier believe that the human blood plasma has never before been demonstrated in an unaltered condition except in microscopic quantity. Coagulation eventually occurs, because the blood necessarily comes in contact with the sides of the wound made in the finger.—Mr. D. B. Dott read a paper, written by himself in conjunction with Dr. Ralph Stockman, describing experiments which show that the ordinarily accepted formula of morphine is the correct one.—Mr. Robert Kidston read the first part of a paper on the fossil flora of the Staffordshire coal-fields, and also read a note on *Neuropteris plicata*, Sternb., and *Neuropteris rectinervis*, Kidston.—Mr. John Aitken communicated a note on a monochromatic rainbow seen at sunset.—Prof. Haycraft read a note on a "scratching centre" found in the spinal chord of some vertebrates.—Prof. Tait communicated an answer to Prof. Boltzman's strictures, which appeared in the *Sitzungsberichte* of the Vienna Academy, on his investigations on the kinetic theory of gases. This has been sent to the *Philosophical Magazine*.

PARIS.

Academy of Sciences, February 27.—M. Janssen in the chair.—On the doctrine of the probability of error; the law of Gauss, by M. J. Bertrand. It is shown that the law of Gauss, based on the postulate, "The mean of the results of any number of measurements is the most probable value deducible from those measurements," is incapable of rigorous demonstration.—Artificial production of rhombohedric crystals of rubies, by MM. E. Fremy and A. Verneuil. Specimens were shown of these crystals produced by the method described at the meeting of March 14, 1887. These are very different from the rubies obtained by the authors in 1877, which were produced in a vitreous vein from which they were detached with great difficulty. The present gems are on the contrary produced in a porous and friable vein, where they occur in clusters of crystals in a state of great purity, and from which they may be easily removed. To effect this it suffices to throw the product of calcination into a flask of water and shake it violently. Then the vein being light remains in suspension in the water, while the heavier rubies are at once precipitated to the bottom. The gems are always rhombohedric, and in every respect comparable to the natural stones. They have the same colour and hardness, easily scratch topaz, become black when heated, regaining their beautiful pink tint when cooled, have a diamond-like brilliance, and perfectly regular crystalline form. The paper was followed by some remarks by M. Des Cloiseaux, to whom the specimens had been submitted for a thorough crystallographic examination. —On some general conditions under which nitrogen is fixed by vegetable soil, by M. Berthelot. The author had already established by a long series of experiments that certain argillaceous earths and certain sands have the property of fixing atmospheric nitrogen and enriching themselves by a slow and progressive process with organic nitrous substances obtained directly or indirectly from living organisms. Since then he has prosecuted the study of this interesting phenomenon, and here resumes the results of his further researches. Some experiments are also described on the transformation of the nitrates in the soil into nitrous combinations of organic character. His observations tend to the general conclusion that the earth should not be regarded as an inert mineral body, stable and invariable in its composition until disturbed by the process of vegetation, but as a body filled with living beings, and whose chemical composition and abundance of nitrogen vary and oscillate with the conditions determining the vitality of those beings.—On a method of quantitative analysis of chloroform, and on the solubility of this body in water, by MM. G. Chancel and F. Parmentier. Priority of discovery is claimed by the authors for this process, which, in a recent communication to the Academy, M. L. de Saint-Martin describes as new.—The Neolithic epoch at Champigny, by M. Emile Rivière. The results are described of the researches that have been carried on since 1867, by MM. Le Roy des Closages, Carbonnier, and the author, at the Neolithic station near the village of Champigny in the Department of the Seine. Here have been found numerous flint implements, scrapers, arrow-heads, polished hatchets, knives, besides four grind-stones and much coarse pottery curiously ornamented, all in association with the bones of the horse, pig, deer, roebuck, and ox. The material of some of the implements points at long migrations, or else a widespread intercourse with more or less remote tribes, the rocks used in their fabrication occurring in the region stretching from Belgium to Chiavenna in the Italian Alps.—Elements and ephemeris of the planet 272, by M. Charlois. These elements are the result of three observations made at the Observatory of Nice on February 4, 11, and 18.—Permanent deformation and thermodynamics (continued), by M. Marcel Brillouin. The chief feature of the present study is the determination of the consequences of the axiom of Clausius.—Experimental researches on the variations produced by a shock in the magnetic condition of a steel bar, by M. G. Berson. It is shown generally that the shocks or impacts given to steel bars have the effect of facilitating the disposition of the molecules in a given direction under the action of the stimulating forces, by diminishing for a very brief interval the molecular friction known as coercing force.—On the laws of chemical equilibrium, by M. H. Le Chatelier. This is a reply to a recent communication from M. Duhem claiming priority in connection with a law of thermo-chemistry lately enounced by the author.—Action of aniline on epichlorhydrine, by M. Ad. Fauconnier. Continuing the researches of M. Hörmann, the author has succeeded in obtaining one of that chemist's anticipated bases, which results from the combination of two molecules of aniline with one of epichlorhydrine. The mode of preparation and properties of this body are described.—On the respiration of corn yeast at various temperatures, by MM. Gréhant and Quinquaud. Continuing the classical studies of MM. Pasteur and Schützenberger, the authors have carried out a series of experiments to measure the volume of oxygen absorbed and of carbonic acid produced by yeast living at first in distilled water in the absence of sugar and in contact with a determined volume of air. They find that the relation $\frac{CO_2}{O}$ is variable with the temperature, so that the isolated yeast-cells would appear to behave differently from the fungi and tissues lacking chlorophyll, which give a constant relation $\frac{CO_2}{O}$ under all temperatures for the same individuals of the same species.

Astronomical Society, February 1.—M. Flammarion, President, in the chair.—M. Flammarion expressed his admiration of what he had seen at the Nice Observatory on a recent visit. In the great equatorial (30 inches aperture), the Orion nebula is splendid, stars of the sixteenth magnitude seem bright, and double stars from 0"·1 to 0"·3 apart are discovered.—M. Flammarion observed the lunar eclipse on January 28 at Nice. The moon remained easily visible during totality, and of a bright copper hue. The Nice Observatory is 375 metres above the level of the Mediterranean Sea. In the finder of the great equatorial the shadow was fringed with a transparent border about 2′ in breadth. MM. Henry Brothers and M. Trouvelot remarked the contrast this eclipse presented with that of October 1884, in which the moon nearly disappeared. M. Detaille said that he had been struck by the very fine colour of the moon; the earth's shadow, though ill-defined on the edge, was quite circular.—MM. Henry showed a photograph of the Pleiades taken with their 34-centimetre object-glass, and an exposure of four hours. The negative included stars down to the seventeenth magnitude. Much new nebulous matter is discovered in this photograph. One of the bright stars is enveloped in a dense nebula hitherto unseen. Several singular long thin streaks of nebulous matter extend in some cases from star to star to a considerable length.—M. Berteaux, geographical editor, presented the Society with a new map of the moon by M. C. Gaudibert, the well-known selenographer. This map has been made from M. Gaudibert's observations and revisals; it has been drawn by M. Tenet, and reproduced by heliography. The diameter of the disk is 64 centimetres.

BERLIN.

Physical Society, February 3.—Prof. von Helmholtz, President, in the chair.—Prof. Paul du Bois Reymond spoke on the difficulty of forming any conception of force acting across an intervening space. From among the various instances of such forces the speaker selected gravity for a thorough discussion. He explained the six properties characteristic of this force, pointing out that only two of them—viz. the proportionality to the mass, and the law of inverse squares of the distances—can be proved experimentally, while some of its other properties, as, for instance, the independence of gravity from the condition of motion of the mass, are much doubted by many observers. Prof. du Bois Reymond then discussed the ever-recurring endeavours in past times to arrive at some mechanical construction for gravity, endeavours which were in all cases unsatisfactory, since they were always dependent either on the fundamental properties of matter, which are themselves incomprehensible, or upon physical phenomena whose basis was still undetermined. Just as in the case of many problems the experiments for whose solution have been repeated until their inaccuracy was clearly proved, so also in the case of gravity has a mechanical conception been repeatedly sought for: hence it becomes necessary to show that gravity is beyond our comprehension, and the speaker proceeded to do this by showing that Lesage's theory of the impact action of the atoms of ether, which has been so long and persistently believed, while it explains the law of inverse squares does not explain the proportionality to the mass, and in certain special cases leads to perfectly impossible results. Gravity is therefore incomprehensible, and Newton's view that it is something inherently present in all matter is correct, since it is by means of this force alone that matter is made evident to us; indeed, as far as the matter itself is concerned, it may be entirely left out of account.—Prof. Helmholtz then explained how he is in the habit of treating the subject of gravity in his lectures. He represents it as being that

law of Nature, established by experience, that every body when in the neighbourhood of another body is subject to an acceleration which is proportional to its mass, and diminishes in the ratio of the inverse square of the distance between them. Such a law of Nature as this, established as it is on the basis of experience, is on the whole not unsatisfactory.—The same speaker then briefly communicated the results of two researches which he had brought before the Academy of Sciences on the previous day. Of these one is due to Prof Kundt, and has reference to the refractive power of metals. He has succeeded in constructing transparent prisms of metals, and thus determining their refractive index. The other, due to Prof. Hertz, has for its subject the rate of propagation of electro-dynamic action. By an extremely ingenious method, which the speaker explained, and which has been used by Prof. Hertz, in many of his previous researches, for the measurement of electrical vibrations, he has succeeded in proving that electricity is propagated along a metallic wire at the rate of 200,000 kilometres per second, and that electro-dynamic action passes through dielectrics with the velocity of light. These experiments thus provide the experimental confirmation of the Faraday-Maxwell theory of electro-dynamic action.

Meteorological Society, February 7.—Dr. Vettin, President, in the chair.—Lieut. Gross gave an account of a balloon voyage which he made on January 21, and described, while presenting the curves he had obtained, his meteorological observations made during this voyage with wet and dry bulb thermometers. One point of great interest which he described was that the balloon remained constantly at the upper surface of the layer of clouds which it was traversing, so that while the body of the balloon was above the clouds the car was completely immersed in the latter, notwithstanding that ballast was frequently thrown out.—Dr. Hellmann produced the curves of temperature for Northern Italy for the month of January, which showed that the cold in this region had been much more intense than in Berlin: the minimum temperature at Alessandria was – 16·5° C.—Prof. Schwalbe spoke on the subject of earthquakes in their relationship to meteorological and cosmic phenomena. He proved, on the basis of a study of the literature of this subject extending over many years, that all sorts of meteorological phenomena, such as temperature, atmospheric pressure, wind, moisture, rain, dryness, atmospheric electricity, clouds, and even optical phenomena, have been referred to earthquakes, either as accompaniments or the outcome or the cause of the same. If the statistics of earthquakes are alone considered, or more especially if microseismic observations are taken into account, the above relationship admits of being readily established; but it breaks down completely if it is worked out in a really scientific way throughout the whole of any one or a series of years. The same remark holds good with respect of those cosmic relationships which have been supposed to exist by various writers, such as that the attraction of the moon and the sun is a cause of earthquakes; this view has recently been held by Falb, and although it is in complete antagonism to the results of careful scientific investigation it has nevertheless been largely accepted by laymen. Just as the whole of Falb's views admit readily of being disproved, so also do his prognostications of earthquakes. According to Falb, each lunar quarter-day may be considered to be essentially connected with the occurrence of an earthquake which may take place either five days sooner or three days later than this time; but, notwithstanding the concession of these wide limits as to time, it has not been found that these periods are always accompanied by an earthquake.

Stockholm.

Royal Academy of Sciences, February 8.—Baron A. E. Nordenskiöld gave an account of a work he is now editing, entitled "Atlas, containing maps (copies) printed during the fifteenth and sixteenth centuries."—On the Aralo-Caspian Sea and the glaciation of the North of Europe, by Dr. H. Sjögren.—On the compression of the crust of the earth under the atmospheric pressure, by the same.—On the method used in computations concerning a certain Life Assurance Company, by Prof. Mittag-Leffler.—On the probability of divergence occurring in employing the hitherto usual methods to represent planetary pertubations analytically, by Prof. Gyldén.—On the Bacteria of the swine-plague, by Dr. E. Selander.—On the structure of Champia and Lomentaria, by Prof. Agardh.—On a series, by Dr. Lindman.—Contributions to the knowledge of the reactions of the plato-oxalate, by Dr. Söderbaum.—On the action of chloron on α- and β-naphthol, by Prof. Cleve.—On two β-amido-naphthalin-sulphon acids, by G. Forsling.—On the action of the metaphosphoric acid on di- and tri-oxides, by K. J. Johansson.—Contributions to the knowledge of carbo-hydrates; No. 2, on graminine, by Drs. Ekstrand and Johansson.—Contributions to the theory of the undulatory movement in a gaseous medium (continuation), by Prof. Bäcklund.—On the rhombic porphyry from the valley of Brumun in Norway, by H. Bäckström.—The form of the crystals, and the optical constants of hydro-carbostyrile, by the same.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Hand-book of Perspective: H. A. James (Chapman and Hall).—Elementary Hydrostatics: S. B. Mukerjee (Thacker).—Chambers's Encyclopædia, New Edition, vol. i. (Chambers).—The Flora of West Yorkshire: F. A. Lees (Reeve).—The Fisheries and Fishery Industries of the United States; Section 2, Geographical Review: G. B. Goode (Washington).—The Religious Sentiments of the Human Mind: D. G. Thompson (Longmans).—Incwadi Yami: J. W. Matthews (Low).—History of Portugal; E. McMurdo (Low).—Geometry in Space: edited by R. C. J. Nixon (Clarendon Press).—The World to Come: J. W. Reynolds (K. Paul).—Flora of the Hawaiian Islands: W. Hildebrand (Williams and Norgate).—Facts about Ireland: A. B. MacDowall (Stanford).—Everybody's Pocket Cyclopædia (Saxon).—On Cold as a Cause of Disease: W. H. Ransom (Williams and Norgate).—Bulletin de l'Académie Royale des Sciences de Belgique, No. 12 (Bruxelles).—Geological Magazine, March (Trübner).—Catalog der Conchylien-Sammlung, Sechste Lieferung (Paetel, Berlin).—Memoirs of the Boston Society of Natural History, vol. iv. Nos. 1 to 4 (Boston).—La Première Comète périodique de Tempel, 1867, ii. (Genève).

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THURSDAY, MARCH 15, 1888.

LIFE CONTINGENCIES.

Institute of Actuaries' Text-book of the Principles of Interest, Life Annuities and Assurances, and their Practical Application. Part II. Life Contingencies (including Life Annuities and Assurances). By George King. (London: C. and E. Layton, 1887).

SOME years ago the Council of the Institute of Actuaries came to the conclusion that the students of actuarial science were subjected to great inconvenience and loss of time in consequence of the number of different books and scientific papers to be consulted in acquiring a knowledge of the subject. Persons actively engaged in the work, and wishing to refresh their memory as to the best methods of solving some special question, frequently felt the same sort of inconvenience. The Council, with that consideration for the students which has always been characteristic of them, resolved to provide what was wanted. They accordingly authorized the compilation and publication—the cost to be borne by the Institute—of a "Text-book of the Principles of Interest, Life Annuities and Assurances, and their Practical Application." The first volume, entitled Part I., and treating of the principles of interest (including annuities-certain), has been before the public since 1882. The second part, which is concerned with "Life Contingencies," has now been issued. The editing or authorship of this portion of the text-book was intrusted to Mr. George King, the Actuary of the Atlas Insurance Company, and formerly of the Alliance, whose practical acquaintance with assurance calculations, well-known devotion to his work, and experience as a lecturer at the Institute, qualified him, in a high degree, for undertaking the task.

In the opening chapters of the present volume, the author deals with the ordinary mortality table, its construction from different kinds of data, and its varied application by the actuary and the statist, including the determination of the probable numbers dying or surviving in a community, or in an annuity or other society. Such a table, showing out of a certain number of persons born how many attain to each year of age, may obviously be formed from records of the duration of life in a great number of individual cases; always provided the cases constitute a fair selection. Here, however, arises great practical difficulty, and mortality tables are, in consequence, usually constructed from observations yielding the probability of living one year at each year of age. This is so important a fact, at least to students commencing the study, that we should have been glad if the "elementary illustrations" given by the author had included a numerical illustration in brief detail reproducing the process underlying one or other of the standard tables. The author has proceeded wisely, we think, in first collecting the elementary formulæ of the doctrine of chances, and then showing how these may be applied to the numbers of the mortality table in order to solve the many and important questions arising in connection with single or joint lives. He points out two fallacies which it is desirable the public should recognize as such. This is one:—

"It will be found . . . that the higher the age from which we count, the greater will be the average age at death. Thus, at age 10, the average age at death is 60·257 years; at age 20, it is 62·101; at age 30, it is 64·726; and at age 60, it is 73·808. . . . It is frequently stated by shallow reasoners that some professions, such as that of the lawyer, must be conducive to longevity . . . because the average age at death of the members of that profession is much higher than that of the general population. But the general population starts from age 0; and starting from age 0 the average age at death, if the mortality were to follow the table, would be only 47·785 years, whereas . . . a lawyer does not enter the profession until he reaches manhood; and usually it is not until many years later that he attains sufficient eminence for his death to be commented upon. Therefore, even if the rate of mortality among lawyers be not more favourable than among the general population, the average age at death of those whose deaths attract notice must be greater."

Much attention is given in this portion of the book to attempts which have been made to embody the *law of mortality* in a mathematical formula which should readily lend itself to the purposes of calculation. Two such attempts are introduced to our notice: the hypothesis of De Moivre, and the hypothesis of Gompertz. De Moivre, in his treatise on "Annuities on Lives," published in 1725, made the assumption, now well known, that, out of eighty-six births, one person dies every year until they are all extinct. Gompertz, in a paper contributed to the Royal Society in 1825, just a century later, observed: "It is possible that death may be the consequence of two generally co-existing causes: the one, chance, witbout previous disposition to death or deterioration; the other, a deterioration, or increased inability to withstand destruction." It would appear, however, that he did not pursue this twofold notion to its conclusion, but contented himself with investigating the effect of supposing "the average exhaustion of a man's power to avoid death to be such that at the end of equal infinitely small intervals of time he lost equal portions of his remaining power to oppose destruction which he had at the commencement of these intervals." The words now quoted, taken alone, perhaps do not give a very precise idea of what was intended, but they really cover the assumption that the force of mortality increases in geometrical progression, and may be represented, as Mr. King says, by Bc^x, where B and c are constants, and x the age. From this, the equivalent of the differential coefficient of the log of the number living, we find the number living at age x may be expressed in the form $k(g)^{c^x}$. By judiciously choosing values for the constants k, g, and c, the results approximate more or less closely to fact for a greater or smaller extent of life, but it was left to Mr. Makeham, the present Actuary to the Church of England Assurance Company, to perfect the formula, and render it an exponent of the effect of the two co-existing causes of death originally contemplated by Gompertz. The final shape of the formula then became $ks^x(g)^{c^x}$, wherein a fourth constant, s, is introduced. In this shape, although there still remains a difficulty with the youngest ages of life, the formula has been used for adjusting crude observations

and simplifying calculations involving contingencies depending on several lives. The hypothesis of Gompertz, as formulated by Makeham, is, no doubt, useful for graduating certain tables, and for dealing with some of the more complex problems of life contingencies, but we doubt whether a disproportionate consideration is not given to it and to its application. In so far as it presents itself to us as the most successful effort yet made to fasten down the law of mortality, it has, no doubt, a charm and a fascination for everyone, and especially the mathematician; but, keeping in view the limited use made of it for the ordinary purposes of assurance work, and that even for graduating it is only one of several methods in vogue, we are inclined to think a less elaborate treatment would have been more commensurate with the proper scope of a text-book and book for general reference.

The next, and of course the main, portion of the volume is concerned with the great class of questions involving the consideration of interest when combined with life contingencies; that is to say, with annuities and assurances, whether on single or joint lives, and whether absolute or contingent; with advowsons, next presentations, fines for the renewal of leases on lives; also with life interests and reversions, and the values of life policies. Explanations and demonstrations are given at length, and some of them are exceptionally good. We may note that, in the chapter on annuities and assurances, the author says: "It has been common, in treatises on life annuities, to deal with annuities and assurances separately, but the two classes of benefits are so intimately connected that they ought always to be taken together." We are not quite sure that we have caught exactly the nature of the objection entertained by the author to the common method of dealing with the two kinds of benefit. We take it the intimate connection alluded to implies that both things are built up of the elementary forms of which $v^n l_{x+n}$ is the type, and proceed on parallel lines, and not that the results for the one should be obtained by giving an algebraic twist to the results deduced by a direct process for the other. We do not infer from his words, or gather from his book, that he would not exhibit the present value of an assurance by direct reference to the present value of £1 to be received by each of the persons alive at age $x + n$, rather than obtain it by an indirect process of reasoning, such, for instance, as this:—"If here be an annuity on (x) payable at the end of each year on which he enters, and another annuity payable at the end of each year which he completes, it is evident that the difference between the two is the value of £1 payable at the end of that year on which (x) enters, but which he does not complete; that is, the value of £1 payable at the end of the year of the death of (x), or, in other words, the value of an assurance on (x). Now $v(1 + a_x)$ is evidently the value of the first-named annuity, and, deducting from this the value of the ordinary annuity, a_x, we have the value of the assurance, $v(1 + a_x) - a_x$." The building up of a formula by premising its verbal interpretation is often an admirable example of ingenuity, but this process can never be allowed to displace the established course of mathematical reasoning.

In this, the staple portion of his work, Mr. King manifests his extensive acquaintance with the subject, or, rather, subjects. With a great quantity of matter at his command, he has used the pruning-knife very sparingly, possibly too sparingly. All the usual formulæ are given for precise calculation, and a number of approximative processes are developed where an exact calculation would be too cumbrous for actual use. It is worth suggesting for consideration whether a collection of questions to be worked out by students might not with advantage be inserted in a future edition of the book. There are many precedents for such a course in connection with text-books, and a goodly supply of questions is already at hand in the examination-papers set at the Institute in past years.

There is a third portion of the work, occupying some seventy pages, in which finite differences, interpolation, and summation are treated with more fullness than branches of pure mathematics would seem to be entitled to in a volume professedly assigned to life contingencies. Indeed, the author admits in his preface that these subjects were not within the scope of the text-book as originally planned. No doubt we have placed before us propositions which are specially applicable to actuarial needs, arranged and demonstrated with Mr. King's usual ability; but it seems to us they would have been more conveniently published in some other connection than the present. A knowledge of these things in a duly regulated course of study would naturally precede the consideration of their application.

The text of the work is supplemented by a collection of interesting tables, commencing with a table of mortality based on a combination of data for young and mature lives, and intended to show the mortality of healthy male life from birth to extreme old age. We must not fail to mention that the collection embraces complete tables for finding the value of joint-life annuities up to four lives inclusive.

Looking at the work as a whole, we find the various subjects are cleverly handled, the propositions appear one after the other in well-ordered succession, the demonstrations are well chosen, and the wording is clear and effective. Altogether Mr. King has done his work diligently and with good judgment, and has placed all future students of the Institute under a debt of obligation to himself and to the Council.

ROSENBUSCH'S "PETROGRAPHY."

II.

Mikroskopische Physiographie der massigen Gesteine. Von H. Rosenbusch. II. Abtheilung. Zweite gänzlich umgearbeitete Auflage. (Stuttgart, 1887.)

IN a notice (NATURE, vol. xxxv. p. 482) of the first part of the present work, we showed that the author, adopting a natural system of classification which gives the first place to field-evidence, divides the eruptive rocks into three great groups, viz. (1) the *Plutonic* rocks; (2) the *Dyke* rocks (*Ganggesteine*); and (3) the *Volcanic* or *Effusive* rocks. Unable to free himself entirely from the idea that geological age ought to be an essential factor in rock-classification, he subdivides the third group into a *palæo-volcanic* and a *neo-volcanic* series. It is the treatment of the neo-volcanic series which constitutes the bulk of this, the second and final part of the book.

The neo-volcanic rocks, which are stated to be essentially confined to Tertiary or post-Tertiary times, occur, for the most part, as lava-streams and sheets, and are often accompanied by tuffs. They are classified by Prof. Rosenbusch as follows :—

(*a*) Family of the Liparites and Pantellerites (equivalents, on the one hand, of the palæo-volcanic quartz-porphyries, on the other, of the granitic plutonic rocks).

(*b*) Family of the Trachytes and basic Pantellerites (equivalents of the palæo-volcanic quartzless porphyries, and of the plutonic syenites).

(*c*) Family of the Phonolites and Leucitophyres (equivalents of the plutonic elæolite-syenites).

(*d* and *e*) Family of the Dacites and Andesites (equivalents of the porphyrites and diorites).

(*f*) Family of the Basalts (equivalents of the melaphyres and certain augite-porphyrites in the palæo-volcanic series; and of the gabbros and diabases among the plutonic rocks).

(*g*) Family of the Tephrites (equivalents of the theralites, *i.e.* plagioclase-nepheline rocks of the plutonic series).

From this synopsis the merits of the new classification may be appreciated. No classification that taxonomic ingenuity may devise will wholly satisfy the desires of the sanguine petrologist. Rocks, however much they may be characterized by a certain amount of geological uniformity persistent over large areas (which have aptly been termed "petrographical provinces"), are still, it must be remembered, mere mineral aggregates; and the amount of possible variation, dependent on differences in chemical constitution, and varying conditions of consolidation, is enormous. Rock-types, which may be clearly defined and sharply separated on paper, will, in the field, often be found passing over into one another by gradations so imperceptible that the petrographer must regard as hopeless any attempt to draw a hard and fast line between them.

A weak point in Prof. Rosenbusch's classification seems to us his fundamental separation of the "dyke-rocks" (*Ganggesteine*) from the plutonic and volcanic series (*Tiefen- und Ergussgesteine.*) Both plutonic bosses and volcanic sheets must necessarily be accompanied by dykes or pipes through which the eruption took place, and into the rocks composing which they pass by imperceptible gradations. The author, indeed, calls attention himself to this fact (on pp. 6 and 522), and proposes to include under the head of "*Ganggesteine*" only those rocks which occur *solely* in the form of dykes and are unaccompanied by tuffs. Still, rocks so nearly allied as these must necessarily be to the dykes and volcanic pipes and necks in immediate connection with the centre of eruption, should not, we think, be so widely separated from them. On the other hand, we find placed in this group rocks, such as granite-porphyry, which are known to occur in bosses, as, for instance, at Shap and at Dartmoor.

As to the question of age, it is so far satisfactory that the author has gone a step in what is surely the right direction, in eliminating this factor from the consideration of the plutonic rocks. With regard to the advisability of retaining the separation into an older and a younger series of the volcanic rocks, Prof. Rosenbusch refrains from expressing an opinion (p. xi. of preface.) In connection with this question, we must draw attention to one point. The structure characteristic of the dolerites (diabases of the Germans) in which allotriomorphic masses of augite are penetrated by idiomorphic crystals and microlites of felspar, and which is known as *ophitic structure*, occurs nowhere in more typical development than in the dolerites of the Western Isles of Scotland (described and figured by Judd) and of Iceland (Bréon), a statement that anybody who has seen rock-sections from these localities will support. Yet these rocks, apparently because they are of Tertiary age, are placed by Prof. Rosenbusch (pp. 725 and 733) with the basalts, and are described as possessing "*intersertal structure*," a structure characterized, according to the definition given on p. 504, by the presence of a hypocrystalline interstitial substance (mesostasis) wedged in between the felspars. That some of the rocks in question contain small wedge-shaped portions and films of glassy interstitial substance nobody will deny; but that many of them are perfectly holocrystalline and truly ophitic is equally beyond question.

Besides "intersertal structure" we notice two other structural terms used now for the first time, viz. "*pilotaxitic*" and "*hyalopilitic*." The former is applied to a holocrystalline structure, especially characteristic of certain porphyrites and basalts, in which the ground mass consists essentially of slender laths and microlites of felspar in felted aggregation, and often exhibits fluxion-phenomena. The addition of films of glass produces "*hyalopilitic*" structure.

New rock-names are *Tholeiite* (p. 504) and *Alnöite* (p. 805). The former is given to a variety of augite-porphyrite with typical "intersertal structure." Certain North of England dykes (the Hett dyke, Tynemouth dyke, and Hebburn dyke) described by Teall, are referred to this group. Several of the English, Scotch, and Irish traps, described by Allport and Hull, are, according to the author, olivine-tholeiites (p. 515). The word "*Alnöite*" is applied by Prof. Rosenbusch to a subdivision of the melilite-rocks, hitherto classed with the melilite-basalts, but differing from the latter by their occurrence in the form of dykes and their near relation to the elæolite-syenites.

Interesting to English readers are the remarks contained on pp. 417, 418. In referring to the Cambrian quartz-felsites and felsites of Wales, which have been described by Messrs. Bonney, Cole, and Rutley, Prof. Rosenbusch compliments these authors on not having overlooked the influence of dynamic metamorphism in developing their present character. He then goes on to say that he has been led, partly by Prof. Bonney's descriptions, partly by the examination of sections, to the belief that two distinct classes of rocks are here associated, viz. metamorphosed eruptive rocks (schistose porphyries), and metamorphosed slates and tuffs (porphyroïdes). A comparative study of these rocks in connection with the "*Lenne-porphyren*" and the porphyroïdes of the Thüringer Wald would, the author thinks, be productive of interesting results. Many of these rocks (*e.g.* from between Llanberis and Cwm-y-Glo, north-west of Cwm-y-Glo, Llyn Padarn, near Llanberis; also the nodular felsites from Conway Falls, and the rock from Digoed) ought, judging from the frequent occurrence of striated and microperthitic felspars,

rather to be referred to the quartz-keratophyres than to the quartz-porphyries (p. 418).

We are glad to see that olivine is no longer regarded by the author as an essential constituent of basalt. This rock-name is thus made to gain considerably in significance, since it now embraces all (neo-)volcanic rocks of *basic* composition which essentially contain plagioclase and augite, whether they occur as lava-sheet or dyke. The *acid* plagioclase-augite rocks, on the other hand, whether with or without olivine, are referred to the andesites.

In connection with the basalts, it may be of interest to point out how considerable an alteration in the minor subdivisions of a rock-group has been produced by modern microscopic research. The old familiar grouping of the basalts, according to their granular texture, as dolerite, anamesite, and basalt, has been superseded. The modern petrographer distinguishes, with Prof. Rosenbusch, between the following structural varieties, which may coexist with any granular dimension: (1) "hypidiomorphic granular," (2) "intersertal," (3) "holocrystalline-porphyritic," (4) "hypocrystalline-porphyritic," and (5) "vitrophyric."

Welcome additions to the book are an appendix to the invaluable literature-index of Vol. I., bringing it up to the present date; and a useful index of localities, compiled by Dr. H. B. Patton. The book is well got up, well printed, and remarkably free from typographical errors.

F. H. HATCH.

A TREATISE ON CHEMISTRY.

A Treatise on Chemistry. By Sir H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S. Vol. III. The Chemistry of the Hydrocarbons and their Derivatives; or, Organic Chemistry. Part IV. (London: Macmillan and Co., 1888.)

THE present instalment of this well-known work deals with those benzenoid compounds containing respectively seven and eight atoms of carbon.

The excellent features referred to in our notices of the previous parts are preserved in this new section. The historical portions are especially valuable. Most text-books of organic chemistry restrict themselves to giving an account of the existing state of the science; but in the present work the description of every important compound, or group of compounds, is prefaced by an historical review of the various investigations which have led up, step by step, to the views now held. To students of organic chemistry, who, in ninety-nine cases out of a hundred, never see the older memoirs (and, if they did, would probably only be bewildered by the obsolete nomenclature and formulæ), these historical introductions are a great boon. As instances of this interesting mode of treatment, we may cite the historical introductions to the subjects of *toluene*, of the *nitrotoluenes*, and of *creosote*—with the account, in the latter case, of the confusion between creosote and phenol, and of the way in which this confusion was eventually cleared up. In this connection we may call the attention of our spelling reformers among English chemists to the passage (p. 33) quoted from Reichenbach's original memoir in which he first coins the word "creosote." The etymological knowledge of the average English chemist (when it exists at all) is little—and dangerous. He has learned that there is such a word as κρέας, and rashly opining that he is at liberty to derive an English word from a Greek nominative, he changes Reichenbach's spelling to "cre*a*sote"—a corrupt form which, as "creasotum," has passed into the Pharmacopœia, embalmed in the choicest apothecaries' Latin. One regrets that the zeal of the reformer was not tempered by the knowledge that Reichenbach derives the word from the contracted genitive, κρέως.[1]

The descriptive portion of the work is full and accurate. The only case that we have noticed in which the information is not up to date is in the account of the *benzaldehydines* (pp. 141 and 142), which are represented as ordinary condensation-compounds of ortho-diamines with benzaldehyde; whereas Hinsberg showed, about a year and a half ago, that they are in reality benzylated anhydro-bases. The name "Nevile" is also throughout erroneously given as "Neville."

OUR BOOK SHELF.

A Text-book of Organic Materia Medica. By Robert Bentley, M.R.C.S., F.L.S. Cr. 8vo. pp. 415. (London: Longmans, Green, and Co., 1887.)

IT is a difficult matter to produce a text-book of materia medica which shall answer the requirements of the student in these days. No subject is less clearly defined either by teachers or by the authorities at Examining Boards. Prof. Bentley indicates this difficulty in his introduction, where he first defines "materia medica" and the allied words "pharmacology" and "therapeutics," and then confesses that our first English authority in this department of science, Dr. Lauder Brunton, has used some of the terms in a different sense. There is one advantage, however, in this difference of view—namely, a variety in the treatment of the subject; and we have to thank Prof. Bentley for having produced a work which departs in many directions from the somewhat stereotyped arrangement of English works on materia medica.

As might have been expected from the accomplished Professor of Botany in King's College, the work is mainly devoted to a careful description of the characters of medicinal plants and their products. The arrangement of the plants is founded, so far as the Phanerogamia are concerned, upon that adopted by Bentham and Hooker in their "Genera Plantarum." The descriptions are given very fully, so as to enable the student to recognize the drugs with facility and certainty, and thus at the same time readily to detect any adulteration. The author is right when he expresses his belief that in the latter respect the book will be especially valuable to the pharmacist. To the medical student and to the medical practitioner adulteration is no longer a subject of direct interest. The day has gone by when crude drugs came into the dispensary of the doctor, who now buys all the preparations ready made; and the Examining Bodies, aware of this, have relieved medical students of the laborious subject of drug adulteration, and now require of them the recognition of but a few of the most important specimens. No doubt the book will find its largest circle of readers amongst young men preparing for the examinations of the Pharmaceutical Society.

In our opinion it would have been better to give the strength as well as the dose of the more important preparations, such as those of opium.

The sections on the chemical composition of drugs have

[1] "Of course the reformer may write "creatosote" if he chooses; but "creasote" is inadmissible.

been carefully brought up to the level of recent researches. The methods of the separation of active principles, such as morphine and atropine, from the crude substances, and their reactions, are not given.

Prof. Bentley does not undertake to give more than the most general indication of the action of the remedies he has so fully described. All that is said of rhubarb, for instance, under the heading of medicinal action, is that "it possesses tonic and slightly astringent properties, and in large doses it acts as a purgative." This is a very good system for pharmaceutical students, and according to some authorities for medical students also at the commencement of their career. But it manifestly encourages learning by rote. What impression of definiteness or value does the word "tonic," for example, represent in the mind of the juvenile reader? Of course none.

Again, whilst we acknowledge that Prof. Bentley has on the whole confined himself to an account of the actions of the various drugs on the healthy organism, we must object to the heading "Medicinal Properties," which is put before the paragraphs descriptive of these. A drug has an action quite apart from the circumstance that it may be employed as a "medicine," *i.e.* in relation to the treatment of disease.

The book contains a number of beautiful illustrations of plants and drugs. It is remarkably free from typographical errors, and the style of its production reflects credit on the publishers.

Catalogue of the Fossil Mammalia in the British Museum (*Natural History*). Part V., containing the Group Tillodontia, the Orders Sirenia, Cetacea, Edentata, Marsupialia, Monotremata; and a Supplement. By Richard Lydekker, B.A., &c. (London: Printed by order of the Trustees, 1887.)

WITH this part Mr. Lydekker completes his laborious and very meritorious work of cataloguing the large collections of Mammalian fossil remains in the British Museum.

The named species are 719 in number, and are arranged under 301 generic and 100 family headings, 106 out of this total being regarded as not to be distinguished from existing forms.

Rich as is the collection in the British Museum, it is very far from including all the known existing fossil forms of Mammalia; but, failing any treatise on such, this work will be of the greatest assistance to all workers in this field. Though at the commencement of his Catalogue Mr. Lydekker did not give descriptions of all the forms detailed, yet, as it proceeded, he somewhat altered his method, giving some of the more important distinctive characters, and so the value of the work to the student has been increased.

A volume of this nature is not capable of being described in any detail, and it will suffice to add that it will be quite a necessary book of reference in the library of a biologist.

Lehrbuch der Histologie. Von Dr. Philipp Stohr, a. o. Professor der Anatomie zu Wurzburg. (Jena: Gustav Fischer, 1887.)

THIS is an excellent little treatise on the same lines as Ranvier's larger "Traité Technique d'Histologie" and Prof. Schäfer's smaller "Essentials of Histology." The various tissues are systematically described with clear and well selected wood-cut illustrations; and after each section of the systematic description a full and careful account is given of the best methods of preparation, which will enable the student to verify the descriptive account. The microscopic structure of the chief organs is treated in the same way. The directions as to *technique* are not merely those suitable for an elementary student, but such as will be useful to one who is advancing in the direction of original research. The figures are, with the exception of a few diagrams, actual representations of what the student should be able to obtain by the particular mode of preparation recommended. An introductory chapter treats of the arrangement of the laboratory, and the apparatus and reagents necessary.

E. R. L.

A Treatise on Photography. By Captain Abney, R.E., F.R.S. (London: Longmans, Green, and Co., 1888.)

THE appearance of a fifth edition of this well-known book is sufficient proof of its popularity, and no trouble seems to have been spared by the author to make this issue a success. The volume has been thoroughly revised, and much new matter added. The author gives the results of his researches, communicated to the Royal Society, on the "Effect of the Spectrum on the Haloid Salts of Silver;" concluding with a chapter on celestial photography, and photography with the microscope.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Coral Formations.

I HAVE read Captain Wharton's paper on "Coral Formations" (p. 393), and the letters of Mr. John Murray and Mr. G. C. Bourne in your issue of March 1.

There is, doubtless, room for difference of opinion as to this important and many-sided question, but I think the balance of evidence is in favour of Mr. Murray's view as to the formation of lagoons.

In this connection the fact that carbonate of lime is soluble in water had been practically overlooked, and its increased solubility in sea-water seems to have been unnoticed before Mr. Murray formulated his views as to their formation. The active life in coral reefs is practically outwards (assuming a shape similar to fairy rings on grass), leaving the central portion more or less dead, or with wide spaces of coral sand and only scattered patches of living animals. The organic matter in this dead coral, by its oxidation, produces carbonic acid, which dissolving in the sea-water exalts its solvent action on the carbonate of lime, now more or less in an amorphous condition.

Reducing such a question to figures has a great advantage, and is often the only way of arriving at a safe conclusion. With assistance derived from the Scottish Marine Station, I have lately been conducting some experiments on the solubility of carbonate of lime in sea-water, the results of which may interest the readers of NATURE at the present time.

The experiments were conducted with sea-water of specific gravity 1·0265 (obtained from the German Ocean 20 miles from land), and at temperatures of from 70° F. to 80° F., which reefs require. The corals used were several species of *Porites*.

Dead or rotten coral exposed to sea-water under these circumstances is soluble to the extent of 5 to 20 ounces per ton.

Take now a reef with a lagoon already formed, half a mile in diameter. This will give an area of about 600,000 square yards, and supposing the water to be 3 feet deep and only one-sixth part of this to be in actual contact with the dead coral, we have 100,000 tons exerting its solvent action. This would give, were the sixth part of the lagoon water to be expelled and replaced with fresh sea-water at each tide, and taking the solvent action at only 10 ounces to each ton, an amount of carbonate of lime removed equal to about 3000 tons each year.

I do not insist that such an amount of carbonate of lime *must* year by year be removed from the lagoon, but I think these experiments show that the carbonate of lime so removed may easily exceed any additions to the lagoon by secretions of

animals living in it, or by coral sand carried into it by wind and waves from the outer edge in the same space of time, and therefore I think the balance of evidence is in favour of Mr. Murray's explanation of lagoon formation.

Could the experiment be made, it would be a matter of extreme interest to know if, and in what proportion, carbonate of lime really exists in lagoon waters, as also the proportion in the waters outside the reef, where new coral formation is actively at work.

It is quite reasonable to suppose that the dead coral so dissolved in the formation of lagoons is carried out as material for fresh coral growths. ROBERT IRVINE.

Royston, Edinburgh, March 6.

IN reference to the interesting discussion on coral formations which has recently appeared in NATURE, a few words from the chemist's point of view may not be out of place.

For some time past I have been endeavouring to satisfy myself regarding the solubility of calcium carbonate in sea-water, and with this end in view I immersed weighed pieces of dead coral (dried at 212° F. till constant) in sea-water. These were protected by suspending them under glass bells floated in about 18 inches of water, and allowed to remain at rest for a known length of time. The following are the results obtained:—

First Experiment.—Oculina varicosa, from St. Thomas, West Indies, weight 16·3164 grammes, with a surface of, roughly, 8 square inches, lost by solution in twenty days, 0·0748 gramme.

Second Experiment.—Madrepora scabrosa, from Levuka, Fiji, weight 21·8540 grammes, surface of 16 square inches, lost 0·1497 gramme in thirty days.

Third Experiment.—Montipora foliosa, Amboyna, weight 15·3334 grammes, surface of 15 square inches, lost 0·1223 gramme in forty-six days.

Every care was taken that the corals should not be subjected to the action of other than convection currents. The temperature ranged between 30° and 40° F. Specific gravity of the water found less than 1·026.

Mr. W. G. Reid, in a paper communicated to the Royal Society of Edinburgh, showed that the solubility of carbonate of calcium increased with pressure; and when determining the percentage of $CaCO_3$ in certain soundings I found that the greater the depth the less carbonate derived from surface shells was present, while it is a well-known fact that in the red clay or other deep-sea deposits, $CaCO_3$ almost completely, if not entirely disappears, as has frequently been pointed out by Mr. Murray.

From the above considerations there can be little doubt that there is considerable action going on in the waters of the ocean.

Take, for instance, a circular lagoon four miles in diameter; this would give a superficial area of 12½ square miles. Taking the results obtained in Experiment 1, and applying them to this imaginary case, then in twenty days, in absolutely still water, there would be dissolved 464 tons of $CaCO_3$, equal to 8472 tons in a year. If the specific gravity of carbonate of calcium be taken at 2·65, this amount would give a thickness of half an inch covering the whole area of the lagoon. In other words, at the same rate it would require about a century to deepen the lagoon one fathom.

These results must be, however, very much under-stated, as the temperature in the coral regions is about twice what I could obtain; the sea-water is denser; there is the action of carbonic acid gas, CO_2, which is constantly being generated by decomposing organic matters, especially in these warm areas, and all which would increase materially the solubility. Moreover, there are the tides and currents continually replacing, or rather mixing with, the denser waters.

The coral animals in the lagoon are, however, constantly laying down new material in the shape of $CaCO_3$, assimilated either directly from the sea or through the medium of other organisms upon which they feed, or both combined. Now it depends upon the excess of the one process over the other whether there be an increase or decrease in the depth of the lagoon.

Growth is restricted mainly in reefs to the outer periphery, leaving large spaces of coral sand in the interior to be freely acted upon. In this way the coral formation increases outwards, while there is a deepening of the interior, albeit this deepening is very small. JAMES G. ROSS.

14 Argyle Place, Edinburgh, March 10.

CAPTAIN WHARTON in his interesting paper satisfactorily explains a condition of reef-growth previously little known and but imperfectly understood. I was pleased to learn that Mr. Bourne's long-expected account of Diego Garcia will soon be published. His remarks concerning the *directing* influence of currents agree closely with those of Semper ("Animal Life," vol. xxxi. Internat. Sci. Ser. p. 228). Of the importance of this agency there can, I think, be no doubt; but solution is also an important agency within the lagoon, and one more capable of actual demonstration than the directing force of the currents. But amongst the supporters of the *anti-subsidence* theory of Murray there is a difference of opinion as to the relative importance to be attached to solution; and we cannot accept the name of "theory of solution" for the new view if it is intended to exclude the other agencies that previously cause the death of the coral, such as the repressive influence of sand, the diminished food-supply, the tidal scour, &c. With this exclusive meaning, the name "theory of solution" would contradict itself, and we should be regarding the problem in much the same light as if we were solely to contemplate the mystery of our own existence from the point of view of the undertaker.

The development of the new theory should be borne in mind. Chamisso, seventy years ago, advanced the view that an atoll owes its form to the growth of the corals at the margin and to the repressive influence of the reef-debris in the interior; but this view gave no satisfactory explanation of the foundation of such a coral reef, and Darwin was driven to his theory of subsidence. The great defect in the view of Chamisso was, however, removed by Murray, who supplied the *foundation* of an atoll without employing subsidence; and investigations in the Florida Sea and in the Western Pacific have confirmed his conclusions. The *forms* of reefs he attributed to well-known physical causes; but Semper and Agassiz have dwelt upon the importance of other agencies, and in our present state of knowledge it will be wisest to combine in one view the several agencies enumerated by these three naturalists as producing the different forms of coral reefs. On the outer side of a reef we have the directing influence of the currents, the increased food-supply, the action of the breakers, &c. In the interior of a reef we have the repressive influence of sand and sediment, the boring of the numerous organisms that find a home on each coral block, the solvent agency of the carbonic acid in the sea-water, and the tidal scour. These are all real agencies, and we only differ as to the relative importance we attach to each. Future investigations will probably add others to the list, besides ascertaining the mode and degree of action of each cause.

March 10. H. B. GUPPY.

Reason and Language.

THE kindness of Prof. Max Müller's reply I recognize with pleasure but without surprise, since those who know him know him to be as remarkable for his courtesy as his great learning.

In answer to his first question, I must say that I made a point of attending his Royal Institution lecture on the day his "Science and Thought" was published, and was greatly disappointed that illness hindered my attending the others. But I immediately obtained his book, and applied myself to understand what seemed to me its essence, though I have not read it from cover to cover. Should I have to review it, of course I shall conscientiously peruse the whole of it.

Before replying further, it may be well to restate my position as follows.

Man is an intellectual being able to apprehend certain things directly and others indirectly. Normally, his conceptions clothe themselves in vocal sounds, and get so intimately connected therewith, that the "word" becomes practically a single thing composed of a mental and an oral element. But these elements are not *identical*, and the *verbum mentale* is anterior and superior to the *verbum oris* which it should govern and direct. Abnormally, conceptions do not clothe themselves in oral expressions at all, but only in manual or other bodily signs, and this shows that concepts may be expressed (however imperfectly), in the language of gesture without speech. One consequence of these relations is that neither the utterance of sounds (articulate or inarticulate) nor bodily movements could have generated the intellect and reason of man, and Noiré's hypothesis falls to the ground. On the other hand, beings essentially intellectual, but as yet without language, would immediately clothe their

nascent concepts in some forms of bodily expression by means of which they would quickly understand one another.

As to the expressions "reason" and "reckoning," I would observe that a study of an organism's embryonic development is a most valuable clue to its nature, and no doubt a similar utility attends historical investigations in Prof. Max Müller's science. Nevertheless, we cannot understand the nature of an animal or plant by a mere knowledge of an early stage of its existence; an acquaintance with the outcome of its development is even more important. Similarly, I venture to presume, the ultimate meaning of a word is at least as much its true meaning as is some archaic signification which may have grown obsolete. The word "spirit," if it once meant only the breath, means more now—as we see from the Professor's first letter. Similarly, if "reason," in its Latin form, once only meant "reckoning," that is no "reason" why it should only mean reckoning now. Here it would seem as if we had an instance of the *verbum mentale* having acted upon and modified the *verbum oris*. I cannot but regard the representation that affirmative and negative propositions are mere cases of addition and subtraction as an incorrect and misleading representation, save when they refer to mathematical conceptions. I am compelled also to object to another of the Professor's assertions. He says:—"There is a wide difference between our apprehending our own activity and apprehending that A is A. Apprehending our own activity is inevitable, apprehending that A is A is voluntary." It is true there is a great difference between these apprehensions, though they both agree in being instances of apprehensions which are not inferences, and as such I adduced them (NATURE, February 16, p. 364). Nevertheless in my judgment the difference between them is not the difference which the Professor states. Both are alike voluntary, regarded as deliberate reflex cognitions, and both are alike inevitable, regarded as indeliberate, direct perceptions. The labourer inevitably perceives that his spade is what it is, though the nature of that perception remains unnoticed, just as he inevitably perceives his own continuous being when he in no way adverts to that fact.

I must further protest against the assertion that the idea "therefore" is "present in the simplest acts of cognition"—that every perception of an object is an inference. This I regard as one of the fundamental errors which underlie all the madness of idealism. Akin thereto is the notion that a philosopher who desires to speak with the very strictest accuracy ought, instead of using "the big I," to say, "a succession of states of consciousness." To me it is certain that even one state of consciousness (to say nothing of "a series") is no more immediately intued by us than is the substantial ego; each being cognized only by a reflex act. What I intue is my "self action," in which intuition both the "ego" and the "states" are implicitly contained, and so can be explicitly recognized by reflection. I was myself long in bondage to these two errors, from which it cost me severe mental labour to escape by working my way through philosophical subjectivism. These questions I cannot here go any further into, and I only mention them in consequence of Prof. Max Müller's remarks. I will, however, in turn, refer him to my "Nature and Thought," as well as to a larger work which I trust may before long be published, and which, I venture to hope, he will do me the honour to look at.

My object in calling attention to the fact that one word may have several meanings, and several words one meaning, was to show that there could not be "identity" between thought and language. This point the Professor seems practically to concede, since he now only calls them "inseparable, and in one sense identical." I do not understand degrees of identity. No mere closeness of resemblance or connection can make two things absolutely identical. I did not, however, content myself with denying this "identity" on account of polyongeny and homonymy; I also referred to common experience (which shows us that men do not invent concepts for preformed words, but the reverse), and I appealed to certain facts of consciousness. To my assertions about consciousness the Professor replies: "The object of all scientific inquiry is the general and not the individual." But this is a quite inadequate reply, since our knowledge of general laws is based on our knowledge of individual facts, and if only one man could fly, that single fact would be enough to refute the assertion that flight is impossible to man.

With respect to evolution, I never said that Prof. Max Müller misunderstood "natural selection," but only that he misrepresented it—of course unintentionally. It is of the essence of natural selection not to affirm teleology as formerly understood, although, of course, it can say nothing (for the whole of physical science can say nothing) about a primordial teleology at the foundation of the entire cosmos. I, in common with the Professor, look forward to "the ultimate triumph of reason and right," but my confidence is not due to any "faith" I have in "Nature" or anything else. I profoundly distrust "faith" as an ultimate basis for any judgment; I regard my conviction as a dictum of pure reason—the certain and evident teaching of that science which underlies and gives validity to every other. I therefore agree with Prof. Max Müller in regarding it as a lesson which "true philosophy teaches us."

ST. GEORGE MIVART.

Oil on Troubled Waters.

IT may interest some of the readers of Captain Wharton's paper on this subject to have their attention called to a curious narrative in Bede, illustrative of the power of oil over troubled waters. When a certain presbyter, Utta, was sent from the North of England by Oswiu to fetch his bride from Kent, he applied to Aidan, the greatest teacher of his day, for his blessing. Aidan gave him not merely his blessing, but some consecrated oil, and told him that on his way back from Kent by sea he would encounter a storm, and thereupon he was to pour the oil on the sea, which would immediately become calm. It happened as St. Aidan had foretold. Utta and his fair charge were duly overtaken by a fearful tempest; the waves were breaking over the ship, when Utta bethought himself of Aidan and his oil. "Assumpta ampulla, misit de oleo in pontum, et statim, ut prædictum erat, suo quievit a fervore" ("Historia Ecclesiastica," lib. iii. cap 15). Aidan had been brought up at the monastery of Iona. Did the boatmen of the Western Islands in the seventh century know of this use of oil? and did Aidan bring the knowledge from thence that saved from shipwreck Utta and the bride Eanfleda?

EDW. FRY.

Were the Elephant and Mastodon contemporary in Europe?

ONE of the most effective services which NATURE does for the cause of science is to enable students who live far apart to exchange ideas in its correspondence columns. May I be allowed to ask a question of some interest, perhaps, to others besides myself? It is a singular fact that we probably know less of the *sub-aërial* conditions prevailing in so-called Pliocene times than we do of those of most geological horizons. The marine Mollusca of this age have been preserved in large numbers and in many places, but the remains of the land fauna are singularly sporadic and broken.

I know of no fragment of a land surface of this age which exists in Britain. In the Crags we have a very puzzling medley of mammalian bones and marine shells mixed heterogeneously, and pointing unmistakably to the beds having been rearranged, and, as the French say, *remanié*.

Unfortunately the Pliocene period has been largely defined on the evidence of these very unsatisfactory beds—unsatisfactory not merely because *it is certain* that the remains of land and marine animals are confusedly mixed up in them, but also because *it is exceedingly probable* that the debris of two geological stages have been mixed together also.

It seems clear to me that, if the Pliocene age is to be clearly defined, we must not rely upon the evidence of the English Crags for defining it, but go elsewhere—namely, to France, Italy, &c.

It is very well known that nowhere in France has the mastodon, which is generally accepted as a very typical Pliocene mammal, been found in the same beds with the elephant. In the English Crags, no doubt the older type of elephant (the *E. meridionalis*) and possibly also molars of the later forms (*E. antiquus* and *E. primigenius*) have occurred with mastodon remains and the remains of other so-called Pliocene beasts; but the mixed character of these deposits puts them out of court, and we are bound to follow the evidence of the French beds, which occur *in situ* and unmixed, if we are to be assured of our position.

My purpose in writing is to ask whether the Italian evidence is the same as the French. Unfortunately the Italian beds do not seem to me to have been studied with the minute care which they deserve. No doubt enormous numbers of mastodon remains and also of remains of *E. meridionalis* occur close together in Italian deposits, but so far as I know the question has not been critically tested as to whether they occur in the *same* beds

or not. Prof. Capellini, of whom I asked the question at the meeting of the British Association at Manchester, could not answer me. *Primâ facie* we should certainly expect the Italian evidence to support the French, but this is by no means the conclusion to be drawn from text-books, in which it is generally taken for granted that in Italy the elephant and mastodon have been found at the same horizon.

The question is one of very great interest and importance, and an answer to it would be especially valuable to me. Perhaps some of your readers may have the means of answering it.

HENRY H. HOWORTH.

21 Earl's Court Square, February 28.

True Average of Observations?

I HAVE long been dissatisfied with the method of taking the arithmetic mean as the most probable value of a *comparatively few* direct observations of a quantity. This is certainly the legitimate result of the theory of probability, or "method of least squares," when one knows nothing to guide one in giving more weight to one than to another observation.

But without knowing anything of the conditions under which the observations were made, or, otherwise, no choice among them being possible by considering these conditions, still, when one comes to compare the results among themselves, this comparison seems to me to afford means of judging between them. Thus, if all the results are plotted on sectional paper, they are found to be grouped closely together at one place and to be scattered wide apart at others. Now the most probable result (whatever be the right method of finding it) lies certainly somewhere about the place of close grouping; and it seems fair to consider those results that come near this place as the *better ones*, and to allow to them *more weight* than to the others in calculating the mean.

If the observations were extremely numerous, there can be no objection to taking the arithmetic mean as the true probable value. But one has usually to content one's self with a few only, and in order to get a better approximation in this case I have constructed the following formula. I would be glad if some of your correspondents will express their opinions as to its legitimacy. In a case of this kind one ought not to trust entirely to one's own judgment; one should submit one's own judgment to be checked by that of several others.

The method I propose is as follows.

First fix upper and lower limits outside which the true value cannot possibly lie, and reject absolutely all measurements outside these limits. The result will not be appreciably affected by taking these limits a little higher or lower, and it is better to err in taking them too wide apart than *vice versâ*. One usually has, or ought to have, a general notion of the quantity sought for, sufficient to determine these limits; but if this be not so, they may be determined by adding to and substracting from the arithmetic mean what is thought to be the maximum possible error.

Let x_1, x_2, x_3, &c., be the *excesses* of the various measurements above the lower of the above possible limits. Let x_0 be the *excess* above the same limit of the as yet unknown most probable value as determined by the formula below.

Attach to each x the weight $\left\{1 - \left(\frac{x - x_0}{x_0}\right)^2\right\}$, *and take as x_0 the mean of the x's with these weights attached.*

Note that equal weights are given to measurements equally above and below x_0. Also to an x coinciding with the lower possible limit, a weight zero is given. Zero weight is also given to an x as much above x_0 as the lower possible limit is below it.

The rule results in the following formula:—

$$\text{Weight for } x = 1 - \left(\frac{x - x_0}{x_0}\right)^2 = \frac{2x_0x - x^2}{x_0^2},$$

$$x \times \text{weight} = \frac{2x_0x^2 - x^3}{x_0^2}.$$

Therefore, the mean equals—

$$x_0 = \frac{2x_0\Sigma x^2 - \Sigma x^3}{2x_0\Sigma x - \Sigma x^2}.$$

This is a quadratic for x_0, the solution of which is—

$$x_0 = \frac{3}{4}\frac{\Sigma x^2}{\Sigma x}\left\{1 + \sqrt{1 - \frac{8}{9}\frac{\Sigma x \Sigma x^3}{(\Sigma x^2)^2}}\right\}.$$

Of course the labour of finding this mean is greater than that of finding the arithmetic mean; it involves summing the first, second, and third powers. But the method is only intended to be used when the number of values to be dealt with is not large, and with the help of a table of squares, cubes, and square roots, the work is not really very laborious.

It is easy to prove that this result is identical with the arithmetic mean in the following three cases: (1) all the x's equal; (2) the x's all equidistant, *i.e.* forming an arithmetic progression; (3) the x's infinitely numerous.

The practical meaning of the rule may perhaps be made clearer by the annexed table, giving the weights attachable to various values of x where x_0 is taken equal to unity.

x	1 or	·9 1·1	·8 1·2	·7 1·3	·6 1·4	·5 1·5	·4 1·6	·3 1·7	·2 1·8	·1 1·9	0 2
$1 - \left(\frac{x - x_0}{x_0}\right)^2$	1	·99	·96	·91	·84	·75	·64	·51	·36	·19	0

The following is a numerical example:—

x	x^2	x^3
1·73	2·993	5·178
·89	·792	·705
·42	·176	·074
1·21	1·464	1·7715
1·17	1·369	1·6016
$\Sigma x = 5·42$	$\Sigma x^2 = 6·794$	$\Sigma x^3 = 9·330$

$$\sqrt{1 - \frac{8}{9}\frac{\Sigma x \Sigma x^3}{(\Sigma x^2)^2}} = ·162 \text{ and } x_0 = 1·0925.$$

The arithmetic mean or $\frac{\Sigma x}{5} = 1·084$.

Mason College, February 4. ROBERT H. SMITH.

Crepuscular Rays in China.

IMMEDIATELY after sunset enormous rays of light are frequently seen spreading from the part of the horizon where the sun has disappeared, and also—though somewhat fainter—from the opposite part of the horizon. Sometimes the rays stret ch right across the sky, and when strongly developed they appear first in the east, and then in the west, and resemble auroral rays, glowing in a yellow or red colour, while the sky between the rays is deep blue or greenish. They appear to be caused by invisible cirro-stratus clouds high up in the air. This phenomenon is never seen in England, or at any rate it is by no means so conspicuous as here. Ancient Greek mariners may have had their imagination impressed by a similar phenomenon, ῥοδοδάκτυλος ἠώς being so frequently mentioned in Homer.

Crepuscular rays at sunrise or sunset are seen at all seasons in Southern China, but they are most frequent at the height of the typhoon season, and most intense just before typhoons, which latter are indicated beforehand by crepuscular rays as well as by halos.

The following table exhibits the number of evenings when strong crepuscular rays were registered in each month of the past three years, and also the mean monthly frequency of the strongly developed phenomenon:—

	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1885	—	—	3	2	4	3	—	—
1886	—	1	1	1	3	7	—	1
1887	1	—	—	2	3	—	—	—
Mean	0·3	0·3	1·3	1·7	3·3	3·3	0·0	0·3

W. DOBERCK.

Hong Kong Observatory, December 31, 1887.

"An Unusual Rainbow."

I READ with interest a letter with the above heading in NATURE (vol. xxxvi. p. 581) from Mr. S. A. Hill of Allahabad, India, of date September 18, 1887. He describes a brilliant rainbow which he saw after the sun had set, and states that such a phenomenon "must be of rare occurrence," and that he had "never before seen anything similar, nor read anywhere a description of a rainbow after sunset." I had not read his letter when, on the

evening of the 1st inst. I observed a similar rainbow. I saw it first at 7h. 25m. p.m., the registered time of sunset here for that day. It lasted for nearly fifteen minutes. The western horizon was cloudy, and the sunset a fine one. The bow was exceedingly brilliant, and as far as I could judge, a perfect semicircle, the ends of the arc being about 4° above the horizon. There was a secondary bow equally perfect, and of remarkable brightness; the brilliant glow below the primary, and the marked dulness between it and the secondary, added to the beauty of the sight. After reading Mr. Hill's letter, I published my observations in a letter to the *Argus*, that others might confirm or correct them. I have received six replies, all in accord with my observations. One of my correspondents informed me that he had, some years ago, seen a lunar rainbow formed just before the moon had risen. H. M. ANDREW.

The University, Melbourne, January 26.

The Nest of the Flamingo.

IN an interesting article by Mr. Bowdler Sharpe, entitled "Ornithology at South Kensington," published in the December number of the *English Illustrated Magazine*, there is a description and figure of the flamingo's nest, and an opinion is expressed that the previously-held ideas about the nest being tall, and the female sitting upon it in a straddling manner, might now be considered as exploded.

I have seen numbers of these tall nests in the shallow pans of water—or "vleys," as they are locally called—in Bushmanland, Cape Colony, particularly at Klaver Vley. These quaint nests were built in the water where it was a few inches deep, and at a considerable distance from the shore. They were conical in form, about 18 inches high, and 6 inches in diameter at the top, with a shallow basin-like cavity for the eggs; built, so far as I can recollect, of slimy mud. To perform the office of incubation, the bird must have straddled over the nest. The species no doubt differs from the one described in the article. There should be no difficulty in securing specimens of these nests. Possibly the object aimed at in building the nests in the water is to secure them against some enemy, and the height of the nest, besides conveniencing the long-legged owner, provides for the rising of the water-level. E. J. DUNN.

Pakington Street, Kew, near Melbourne.

Dynamical Units and Nomenclature.

IN his review of Prof. MacGregor's "Kinematics and Dynamics," on page 361, Prof. Greenhill tilts a lance against those whom he terms mathematical precisionists. I do not know this book, and I hold no brief in its defence; but as I owe to these precisionists whatever clear ideas I have on mechanics, I feel bound to enter into the lists on their behalf, little as they need my aid.

Both the precisionists and practical men start with the same two dynamical quantities, which they respectively call *mass* and *force*, *weight* and *force*; of these they select arbitrary units, and respectively name them *pound* and *pound-weight*, *weight-of-a-pound* and *force-of-a-pound* (or *pound-weight* and *pound-force*).

To the single word *pound* the practical man does not, so far as I know, attach any single definite idea, and he cannot, therefore, use this word singly without introducing possible confusion; for it characterizes matter and force equally, and yet *is* neither. On this view Prof. Greenhill's own expression "the attraction of the earth on a pound," should for accuracy and consistency be "the attraction of the earth on the weight of a pound (*or* on a pound-weight)."

To the precisionist a pound *is* a certain mass, just as a foot *is* a certain length, so that the practical man's "weight of a pound" is simply the "pound" of the precisionist, who would no more dream of 'distinguishing' it as "the mass of a pound" than of distinguishing a foot as "the length of a foot."

The attraction of the earth on a certain amount of matter is called "the force of 10 pounds" by practical men, and "the weight of 10 pounds" by precisionists: these are purely *definitions*, so that the phrases are absolutely equivalent. If, then, in the specification of a force produced otherwise than by the attraction of the earth a precisionist is required to speak of it as "a force equal to the weight of 10 pounds," the practical man must follow suit with "a force equal to the force of 10 pounds." These expressions stand, or rather fall, together, and the consistent precisionist would specify the force as "10 pounds-weight" merely.

If, however, a *body*, such as a brickbat or the iron block supplied with a balance and called a "pound weight," is to be introduced into the specification, a precisionist would very properly say "a force equal to the weight of 10 brickbats or of 10 pound-weights"; and the *complete* idea hereby conveyed cannot be expressed by the practical man otherwise than by "the attraction of the earth on 10 brickbats or on 10 pound-weights."

In no way, then, is "a force equal to the weight of a mass of 10 pound-weights," the precisionist equivalent of the practical "force of 10 pounds," nor is it even consonant with precisionist nomenclature.

Since, therefore, the precisionist uses *mass*, *force*, *pound*, *pound-weight*, as the exact equivalents of the practical man's *weight*, *force*, *weight-of-a-pound*, *force-of-a-pound*, the advantage does not seem to lie on the side of the latter, more especially when he is untrue to himself in loosely using the word "weight" as often in the sense of "force" as according to his definition.

But so far both practical men and precisionists labour under the immense disadvantage of dealing with a variable force-unit which can be made precise only by a specification of place; and it is greatly to the credit of the latter that they have introduced a simple invariable force-unit by which all forces, whether due to gravitation or other physical action, may be expressed absolutely in a form which allows of direct comparison between them. With this unit ma is the correct measure of a force, and when Prof. Greenhill speaks of "the mathematician straining after the equation $F = ma$, when using the gravitation unit of force," I utterly fail to understand what is meant, considering that this expression of a force necessarily implies an *absolute* force-unit; and I further feel strongly tempted to deny that either for this unintelligible operation or for any other the precisionist ever uses g pounds as a mass-unit, though, if he ever does use a variable mass-unit in measuring the invariable mass of a body, he is surely countenanced by the practical man who does not hesitate to use a variable force-unit in measuring the invariable force exerted by a given spring compressed to a given extent. I might further add that the precisionist *never* measures the weight of a body in "pounds," even if he denotes it by w, and that, if he does sometimes denote this variable force by the same number irrespective of place, it is only when using the practical man's variable force-unit.

With regard to confusion arising from the use of the equation $w = mg$ any more than from the use of the equation $w = m$, this would be to me inconceivable, did I not notice that Prof. Greenhill uses the phrase "if the equation $w = mg$ is supposed to be used with absolute units." Does there indeed exist a single man who thinks that this equation can be used with other than absolute units? If such there be, to him certainly will confusion be not only possible, but probable too, and deservedly so; but to others there can surely be no more confusion in expressing a (precisionist) weight as m or mg indifferently than in expressing an angle as θ or $180\,\theta/\pi$, it being of course premised that the proper unit—(*precisionist*) *pound-weight* or *poundal*, *radian* or *degree*—is named.

Further, how it can be a solecism to measure pressure in poundals per square foot any more than in pounds-weight per square inch—which latter is the precisionist equivalent of what an engineer would loosely and most inaccurately call "pounds"—I am at a loss to understand, since pressure is the measure of the distribution of force over area, and a poundal is as much a force as "the force of a pound," and very much more definite. And how the expression of the (precisionist) weight of a body in poundals rather than in pounds-weight is a solecism also demands explanation.

Lastly, I must seriously protest against the suggestion that a precisionist should ever ask for, or want to buy, "half a poundal of tea": what he wants is the tea itself, the substance of it and not the earth's action upon it, and very rightly and properly he asks for "half a pound," which the consistent practical man would have to term "the weight of half a pound."

In the above I am not concerned to defend the practice of those mathematicians who select fantastic units of mass or force as a foundation for some puzzling questions of no utility whatever: I have merely attempted to define the position of the physicist or precisionist, and to rebut *seriatim* the charges brought against him in Prof. Greenhill's criticism.

February 27. ROBERT E. BAYNES.

Too many Decimal Places.

A COMMUNICATION in NATURE of January 26 (p. 294) ends with the sweeping suggestion "that, as a rule, only experimentalists are capable of judging the limits of accuracy of experiment, and that they may be trusted to save themselves trouble where trouble may be saved without sacrificing accuracy."

On the contrary, is it not true that experimenters, as a class, have shown a marked tendency to give unnecessary trouble, both to themselves and to those who utilize their re-ults, by using too many significant figures in their numerical work? The strictures of mathematicians have done much to check this tendency. But can it yet be claimed that their habits need no critical inspection in this respect? Not being prepared to bring forward statistics, I can only make this remark in the form of a query, which applies to the general statement quoted, rather than to the merits of the special discussion which gave rise to it. In vol. lxi. (1871) of the Journal of the Franklin Institute, Prof. Pickering has shown by graphical methods how greatly Regnault's coefficients may be simplified. J. RAYNER EDMANDS.

Harvard College Observatory.

"The Teaching of Elementary Chemistry."

IN NATURE of February 23 (p. 389), an anonymous correspondent, signing himself "Z.," draws attention to what he calls "a few highly misleading passages in the two books reviewed under the above heading in NATURE of January 19."

In the name of the authors of these books, I challenge "Z." to make good his statement that the passage which he quotes from p. 65 of the "Elementary Chemistry," concerning the reaction between sodium and water, is "highly misleading." We assert that the sentence is not misleading. The second statement quoted by "Z." is not quite correct: chlorine monoxide is prepared by passing dry chlorine over yellow mercuric oxide, which has been previously dried at 300°-400°, at the ordinary temperature, not over heated mercuric oxide, as stated on p. 116 of the "Elementary Chemistry." We thank "Z." for the correction. But, inasmuch as the result of passing chlorine over yellow mercuric oxide dried at about 100° is to evolve oxygen without forming chlorine monoxide, the correction does not affect the argument, and it may still be justly said that in making chlorine monoxide "we carry out a reaction in which oxygen is produced in presence of chlorine." The supposed contradiction found by "Z." between the directions given in the "Practical Chemistry" to the student who is burning a weighed quantity of magnesium—not to remove the lid of the crucible lest some of the magnesia should be "volatilized and lost"—and the statement in the "Elementary Chemistry," that "no compound of magnesium has been gasified," rests upon a verbal quibble. *Volatilized* and *gasified* have not precisely the same connotation. I confidently assert that no student is in danger of being misled by either of the statements which "Z." has quoted.

"Z." states that the results of an experiment on the reaction between potash and iodine, described on p. 63 of the "Practical Chemistry," contradict the sentence on p. 62 of the same book concerning the similarities between the chemical properties of chlorine, bromine, and iodine. I reply that "Z." has here shown himself to be unacquainted with the methods of chemical classification; and also that he has taken the word *similar* to mean the same as *identical*.

If "Z." will bring forward proofs that the statements he has quoted are "highly misleading," and will sign his name to the letter in which he states these proofs, I am ready to argue each point with him in detail. But, if "Z" continues to charge the authors of the books he has deigned to notice with making misleading statements, while he himself remains anonymous, I shall decline to take any notice of his communications.

Cambridge, February 29. M. M. PATTISON MUIR.

The Gale of March 11.

I BEG to inclose the readings of my standard Robinson's cup-anemometer during the gale of March 11:—

11-12	a.m.,	64 miles.
12- 1	p.m.,	67 ,,
1- 2	,,	71 ,,
2- 3	,,	73 ,,
3- 4	,,	63 ,,

General direction, S.W.; altitude, 600 feet above mean sea-level. C. E. PEEK.

Rousdon Observatory, Lyme Regis, March 13.

THE DISPERSION OF SEEDS AND PLANTS.

IN a recent number of NATURE (vol. xxxv. p. 151) I mentioned instances which had come under my observation, in which birds had taken an active part in the dispersion of seeds and plants. Since then I have come across further notes bearing upon the subject which is one of considerable interest and importance, as it throws a direct light upon some at least of the agencies whereby plant life has been distributed over the surface of the globe. Although birds, from their greater adaptability to rapid and extensive locomotion, are more concerned than any other animals in the dispersion of plants, they are by no means alone in this work.

It may seem strange, at first sight, to assert that cattle have been the means of distributing the seeds of certain plants from one country to another, but a statement is made by Grisebach[1] respecting *Pithecolobium Saman* (N.O. Leguminosæ), a large tree native of Tropical America, now naturalized in Jamaica, that the "seeds were formerly brought over from the continent [of America] by cattle." This statement has been carefully examined, and it is fully borne out by facts. Formerly, Jamaica, like Trinidad at present, was dependent for cattle on Venezuela. The food of the animals during their voyage consisted amongst other things of the pulpy legumes of *Pithecolobium Saman*. The seeds being very hard were uninjured by the process of mastication and digestion, and they were dejected by the animals in the pastures, where they germinated and grew up into large trees. In this instance the seeds were carried across the sea a distance of about a thousand miles, and there is no doubt that the cattle were directly concerned in their introduction. Indeed, without them the seeds, even if accidentally introduced amongst the fodder, would not have been placed under such circumstances as would have enabled them to give rise to plants. In the first place, by being passed through the animals the seeds were softened and the period of germination hastened. In the second place, being embedded in the droppings of the animals the seeds had a suitable medium to protect and promote germination; and this medium enabled the young plants to withstand the season of drought which is incidental to almost every tropical country. In this instance we have cattle not only the means of introducing the seeds of a valuable tree, but also involuntarily instrumental in establishing the tree in a new country, and providing shelter, shade, and food for their progeny. Those acquainted with the guango or rain-tree, as this *Pithecolobium* is locally called, will fully realize its value as a shade and food-tree for cattle, and they will also appreciate the singular concourse of circumstances by means of which such a tree was introduced to a new country by the very animals which required it most.

It is possible there may be some who will doubt the possibility of seeds retaining the power of germination after undergoing the processes of mastication and digestion, and especially in the special case of ruminating animals. There is, however, very clear evidence on the subject. It is a common occurrence in India to utilize the services of goats to hasten the germination of the seeds of the common *Acacia arabica*, known as the babul. This tree belongs to the same natural order as the *Pithecolobium*, and grows in the poorest and driest soils of India. The babul seeds will not germinate readily in the hot weather, and it is the regular habit, in order to save a season, for a person desirous of a crop of seedlings to make a bargain with a herdsman or a neighbour who possesses a flock of goats to quarter them for some days in a small inclosure in which they are fed on babul leaves and pods. The droppings of the animals contain a certain number of seeds which are uninjured, and these now readily germinate, and give rise to plants the same

[1] "Flora, British West India Islands," p. 225.

season. I am informed by Dr. Watt that in India "several other plants are treated in the same way." The seeds of the several species of cultivated Guava are hard and do not easily germinate. These, however, are said to germinate more freely and readily when they are picked up in night soil.

While on this subject I would mention that when at St. Helena in 1883 I expressed some surprise that no attempt was made to utilize "urban" manure in the neighbourhood of Jamestown, when the land was so impoverished and yielded such poor crops. I was met by the fact that if such manure was largely used the land would become overrun with plants of the prickly pear, *Opuntia Ficus-indica*, the fruit of which is largely consumed by the inhabitants. There is little doubt that the seeds of this plant, like those of the Guava, and I suspect also species of *Passiflora*, which are swallowed whole, are capable of germination after they have passed through the human body. Another instance occurs to me where the use of manure has been the means of distributing an undesirable plant on cultivated lands. In many tropical countries a grass known as Para, Mauritius, or Scotch grass, and sometimes as water grass (*Panicum barbinode*), has been introduced from Brazil, and highly esteemed for its rapid growth and nourishing properties. It grows well in moist situations, on the banks of streams, and even in soils so swampy as to be suitable for nothing else. In such situations it spreads rapidly and yields abundant food for cattle and horses. Nothing, however, could be worse than this grass for cultivated areas, where the land is required to be kept free from weeds, and where crops of sugar-cane, coffee, tea, and cacao are raised. It has been found that where animals are fed on this grass the joints even after passing through the animals have been known to grow. Hence the manure, if freshly used, has been the means of establishing the plant over wide areas.

In a recent work Mr. Ball has drawn attention to numerous introduced plants which are met with in South America. He naturally mentions the cardoon, the wild state of the common artichoke, which is now more common in temperate South America than it is anywhere in its native home in the Mediterranean region. Darwin [1] doubts whether any case exists on record of an invasion on so grand a scale. Several hundred square miles are covered with this introduced plant, which has over-run all members of the aboriginal flora. The introduction of the cardoon appears to have been effected directly by man for the purpose of contributing to the food supply of cattle; but as regards another widely-spread plant the mode of its introduction is not clearly known.

Mr. Ball states:—"As to many of these [introduced South American plants] it appears to me probable that their diffusion is due more to the aid of animals than the direct intervention of man. This is specially true of the little immigrant which has gone farthest in colonizing this part of the earth—the common stork's-bill (*Erodium cicutarium*), which has made itself equally at home in the upper zone of the Peruvian Andes, in the low country of Central Chili, and in the plains of North Patagonia. Its extension seems to keep pace with the spread of domestic animals, and as far as I have been able to ascertain it is nowhere common except in districts now or formerly pastured by horned cattle. It is singular that the same plant should have failed to extend itself in North America, being apparently confined to a few localities. It is now common in the Northern Island of New Zealand, but has not extended to South Africa, where two other European species of the same genus are established." [2]

Erodium as a genus is separated from the true Geraniums amongst other reasons on account of the tails of the carpels being bearded and spirally twisted on the inside. It is possible that these characteristics have enabled the seeds to attach themselves to the legs and bodies of cattle and so effected their distribution over wide areas in such situations as are favourable to their growth.

In the Island of Jamaica we have a remarkable instance of the naturalization and wide distribution of an introduced plant in the case of the Indian mango. In an official Report, published in 1885, I stated that to the mango, possibly more than any tree in the island, is due the reforesting of the denuded areas in the lower hills; and as in consequence of the changes taking place in the climate members of the indigenous flora are unable to maintain their ground, it is fortunate the island possesses in a vigorous and hardy exotic like the mango the means of counteracting the baneful effects of deforestation. It specially affects land thrown out of cultivation, and the sides of roads and streams where its seeds are cast aside by man and animals. It practically re-clothes the hills and lower slopes with forest, and it enables the land to recuperate its powers under its abundant shade-giving foliage.[1] It is strange that in Ceylon, which is so much nearer the home of the species, the mango does not spread by self-sown seedlings. This corroborates Mr. Ball's statement with regard to *Erodium cicutarium*. The latter is widely spread in South America, but only sparingly found in other countries under apparently exactly corresponding conditions. We cannot say why such anomalies exist. They do exist, however, and offer problems which can only be solved by a closer study of the conditions of plant life, and the interdependence of plants and animals acting and reacting one upon the other.

The orange-tree was introduced to Jamaica more than a hundred years ago. It is now found practically wild over the settled parts of the island, and the fruit is exported to the value of nearly £50,000 per annum. Up to quite recently very few trees were planted. Nearly the whole were sown by the agency of frugivorous birds, who carried the seeds from place to place and dropped them in native gardens, coffee plantations, sugar estates, and grass lands. In such localities the orange-trees grew and flourished, and now a demand has arisen for the fruit in the United States an important industry has been established, the active agents in which have been birds. The agency of birds in the distribution of the seeds of plants is too large a subject to be discussed at length here. A valuable contribution of facts in this direction has lately been made by Dr. Guppy in his important work on the Solomon Islands. As the most recent addition to our knowledge of what takes place in oceanic islands at the present time it deserves careful attention. It will suffice only to quote one or two sentences:—"Whilst through the agency of the winds and currents the waves have stocked the islet with its marginal vegetation, the fruit-pigeons have been unconsciously stocking its interior with huge trees, that have sprung from the fruits and seeds they have transported in their crops from the neighbouring coasts and islets. The soft and often fleshy fruits on which the fruit-pigeons subsist belong to numerous species of trees. Some of them are as large even as a hen's egg, as in the case of those of the species of *Canarium* ('Ka-i'), which have a pulpy exterior that is alone digested and retained by the pigeon. Amongst other fruits and seeds on which these pigeons subsist, and which they must transport from one locality to another, are those of a species of *Elæocarpus* ('toa'), a species of laurel (*Litsea*), a nutmeg, (*Myristica*), an *Achras*, one or more species of *Areca* (palm), and probably a species (of another palm) *Kentia*."

D. MORRIS.

[1] "Naturalist's Voyage round the World," by Charles Darwin, new ed. 1870, p. 119.

[2] "Notes of a Naturalist in South America," by John Ball, F.R.S., London, 1887, pp 164, 165.

[1] Annual Report, Public Gardens and Plantations, Jamaica, for the Year 1884, p. 45.

ON THE APPEARANCES PRESENTED BY THE SATELLITES OF JUPITER DURING TRANSIT.

A PAPER was read by Mr. Edmund J. Spitta, at the November meeting of the Royal Astronomical Society, of especial interest to those who have devoted their attention to Jovian phenomena. As the paper itself is a long one, being the result of over four years' work, we must refer our readers for details to the paper itself; but, speaking briefly, the author observes that since the discovery of the satellites by Galileo in 1610, astronomers have been puzzled by their discordant appearances during transit, but more especially by the fact that these phenomena do not apply equally to all the satellites, or even in some instances to the same satellite in two successive revolutions. It appears that notably the fourth—the farthest from its primary—as it approaches the disk of Jupiter, becomes rapidly and increasingly fainter until it arrives at contact. When once on the limb it shines with a moderate brilliancy for about ten or fifteen minutes, then becomes suddenly lost to view for another period of about the same duration, and lastly reappears, but as a dark spot which grows darker and darker until it equals the blackness of its own shadow on the planet. The appearance presented by the second satellite, however, is entirely different, for it seems never to have been seen otherwise than pure white during transit; whereas the first and third differ yet again from the preceding two. The former is sometimes a steel-gray, and at others a little darker, whereas the latter has been seen perfectly white, and yet so black as to be mistaken for the fourth; both appearances having been witnessed by Maraldi as far back as 1707, and that too in successive revolutions.

The author seems to have spent some years in examining these phenomena on all possible occasions, and under different conditions, such as before, during, and after opposition; and to have collected all published and unpublished observations; and also to have devised an occulting eye-piece—movable shutters in the focus of a Ramsden eye-piece—for the express purpose of shutting off the light of Jupiter; but, to use his own words, "without adding to the pre-existing knowledge of the subject."

The fact of having witnessed, when on the banks of the Rhine in 1886, the transit of a brilliantly illuminated ship's lantern as a dark spot on the disk of the rising full moon, suggested the carrying out of a series of experiments to ascertain the proportions of light which two bodies must possess, so that the smaller should appear gray or black when superimposed on the larger; and it was hoped that if the facts and figures thus experimentally obtained corresponded with the albedos of the satellites themselves as compared with Jupiter, it would not be unreasonable to suspect that the abnormal appearances presented by the satellites depended on functional idiosyncrasies of the eye itself, rather than upon physical peculiarities of the Jovian system.

Space will not allow a description of the experiments, which were somewhat numerous, the photometer employed being an adaptation of that arranged by Prof. Pritchard, of Oxford; but, speaking in short, small disks of different tintings of Indian ink, representing the satellites, were superimposed on larger ones of various sizes of pure white cardboard, and it was found that, with certain restrictions, the difference of albedo (a term expressing "the relative capacity for reflection of diffused light from equal areas") between the smaller and the larger caused the gray and black appearances, and that they were not due to any difference in the *quantity* of light reflected from either. For a moon to appear gray or black, a difference of albedo was required of 0·42 in the first case, and of 0·87 in the second, whilst moons of a superior albedo remained white during transit.

Further, the effect of one moon approaching another, was gone into, and the fading of the smaller was likewise found to be in direct proportion to the relation its albedo bore to that of the greater, and was in no way connected with the *amount* of light reflected by either. The effects in the appearance of the same little moons when in transit over different portions of a sphere were also studied, and, strange as it may seem, *the whole of the phenomena of the dark transit were thus accidentally reproduced*, and this caused much surprise, seeing it was brought about by such simple means. The concluding experiments consisted in photometrically ascertaining, for the first time, the reflective ability of different portions of an unpolished sphere; and the results obtained are set forth in the following abridged table; column 1 giving the exact angle of the observation, and column 2 the resulting albedo.

30°	...	...	·735
40	...	...	·500
50	...	...	·367
60	...	...	·323
65	...	...	·261
70	...	...	·172
75	...	...	·133
80	...	...	·080
83	...	...	·049
86 30′	...	...	·027

A large number of facts and figures having been ascertained, attention was then directed to obtaining the relative albedos of the real satellites themselves as compared with Jupiter. The reduction of the observations was attended with several difficulties, each of which had to be dealt with; but one of them especially deserves a passing mention, and it is this, viz. that the eye does not seem to be impressed in the photometer with the light coming from an object of sensible area, such as Jupiter, to the same extent as it is from a point of light such as is shown by the satellites. A suggestion from Capt. Abney, however, relieved the difficulty, and, this systematic error removed, the results came out in an extremely satisfactory manner, for it was then found that the albedos of the satellites corresponded very approximately with the requirements of the experiments, as the following abridged table shows; in column 1 is shown the number of the satellite, in column 2 its difference in magnitude with that of Jupiter, and in column 3 the resulting albedo.

I.	...	8·12	...	·656
II.	...	8·40	...	·715
III.	...	7·88	...	·405
IV.	...	8·73	...	·266

Thus is it shown to be more than probable that the reason the fourth satellite is uniformly black during transit, when it has passed its period of disappearance, is owing to its albedo being so low as to grant the difference between it and the background necessary for a body to appear black when superimposed on another as ascertained by the experiments. Its preliminary whiteness and disappearance are also shown to be a question of relative albedo, for they are due to the fact that a sphere at its limb and edges loses so much in reflective ability, that up to that moment, the satellite possesses sufficient albedo (as compared with the background in that situation) to maintain its whiteness. So too with the second satellite: its albedo proves to be so high that it is capable of preserving its brilliancy throughout the entire transit. The third and first satellites evidently possess sides of differing albedo, one high enough to maintain a brighter aspect than the other, or even, as in the case of the third, to make it appear white when one side is presented to the earth, and dark when the other. In conclusion, to quote from the original paper, "it is not unreasonable to conclude that these anomalous phenomena are due to functional idiosyncrasies in the eye itself, rather than to physical peculiarities of the Jovian system."

THE MONSOONS.[1]

EVERY School Board pupil who reads a shilling primer of physical geography knows that the monsoons are periodical winds which blow over the Indian Ocean at different seasons of the year; but very few, even among regular meteorologists, are fully aware of the interesting but complex nature of the details of these phases of atmospheric circulation.

The two publications which are the subject of this notice contain a vast amount of information and research connected with these winds. The charts of the Bay of Bengal consist of a series of maps of mean pressure, resultant wind, and ocean currents for every month of the year; with a page of descriptive letter-press for each. They were compiled by Mr. W. L. Dallas, and are published in the inconvenient size of 23 by 18 inches.

The memoir on the winds of the Arabian Sea is a long and exhaustive report by the same author, with a critical and theoretical discussion of the results obtained all over the North Indian Ocean. This is published in a large quarto form, and contains small-scale charts of mean pressure, and of both wind force and direction, for every month of the year; while similar maps of temperature and vapour-tension are given for the months of April, May, and June only.

Space will permit us to notice only the extreme conditions which characterize the months of January and July, or the most pronounced periods of the north-east and south-west monsoons respectively. It will be well to take the north-east monsoon first, as it is much the simpler of the two. One of the most important results of Indian research has been to modify the crude idea that the north-east monsoon blows directly all the way from the great Siberian winter anticyclone down to the equator. Now it has been shown that there is in the month of January a small anticyclone over the Punjab, and an area of high pressure over the Persian Gulf.

This fact is of far more than local importance. The typical distribution of pressure over the globe consists of an equatorial belt of low pressure, with a belt of anticyclones on either side, about the line of the two tropics. Heretofore we have been constrained to look on the Siberian anticyclone—centered near the Arctic Circle—as the representative of the tropical belt of high pressure, but now there is the strongest presumption that these smaller anticyclones are the true, but diminutive, equivalents of the tropical belt.

There is a curious irregularity in the sweep of the isobars and winds over India towards the equator. The charts indicate a local depression all along the west coast of India, and a slight general protrusion of pressure over the Bay of Bengal. This latter is important, as we shall have to refer to the converse condition in the opposite monsoon.

The conditions of the south-west monsoon are a good deal more complicated, for in July we have to explain the following relations of pressure and wind. A belt of high pressure runs along the twentieth degree of south latitude almost from Australia to long. 70° E.; but then the isobars mount up to the equator along the coast of Africa. An irregular area of low pressure lies over Scinde, but the baric slopes all round are by no means symmetrical. The most noticeable irregularity is an area of relatively low pressure over the south-west of the Bay of Bengal, so that the mean isobar of 29·80 which runs towards the north-east from Africa to near Bombay, bends then to the south-east until it arrives near Trincomalee, in Ceylon, when it turns again to the north-east. The wind conforms partially to this distribution of pressure. South of the Line the south-east trade blows with great uniformity, crosses the equator with a regular sweep into the Arabian Sea, turning first to the south-west, and eventually to the west, between Karachi and Bombay. But in the Bay of Bengal the situation is rather different. The depression, before noted, is associated with a west-north-west wind over Southern India, but a light westerly current and rainy weather prevails all over the south of the Bay, from the latitude of Ceylon, down to the equator, while a strong south-west monsoon blows all up the Bay itself. Hence a ship going up to Calcutta will find the south-east trade replaced by light irregular winds between south and west, with much rain, from the equator to about 10° N., before she encounters the fresh south-west monsoon in the upper part of the Bay.

Mr. Dallas gives many interesting details in this memoir, such as a discussion of the so-called "soft place" in the monsoon between Bombay and Aden. This is a region described in the East Indian sailing directories as lying along, or about, the ninth parallel in the Arabian Sea; but the present series of observations afford very little evidence of the existence of this tract of quiet conditions.

The author seems to find some difficulty in explaining the cold air found along the African coast during the height of the monsoon, but this is almost certainly due to the cold water which wells up from below, as the hot surface water is driven to the north-east. The weather shore of a tropical coast in a steady atmospheric current is always cold for the same reason.

Mr. H. F. Blanford has worked out the precipitation of the south-west monsoon in his great memoirs on "Indian Rainfall," and has brought out most clearly a great meteorological principle. He finds that even with the saturated atmosphere of the Indian Ocean—air that contains nearly twelve grains of water in a cubic foot, as compared to about six grains in our own climate—no great precipitation takes place without dynamical cooling. That is to say, unless the air is forced upwards by local obstacles, &c., and so cooled by expansion, no great rainfall can occur.

But the great question, about which there is still some doubt, is the precise relation of the south-west monsoon to the south-east trade. Dové started the theory that the south-east trade turns to south-west after crossing the equator, owing to the influence of the earth's rotation; and there can be little doubt that in the Arabian Sea the trade wind does sweep directly across the Line and grow into the monsoon.

But in the Bay of Bengal, Mr. Blanford finds that the south-west monsoon is not linked up habitually with the south-east trade, though it is so occasionally; and he considers that the monsoon is drawn from a reservoir of air over the equatorial zone fed by the south-east trades, but that it is not the south-east trade simply diverted from its ordinary course.

This opinion is based on the following facts, brought out by the charts under review, for—

(1) The south-east trades are strong, but the winds are light and variable from the equator to 10° N., above which fresh winds are again developed. There is, then, some hitch in the sweep of the south-east current across the equator.

(2) The winds just north of the line are nearly from the west, while they flow from south-west at the top of the Bay. Theoretically the wind should get more and more westerly the further north we go.

(3) The south-east trade is tolerably dry; the equatorial belt of westerly winds is very wet and squally; while the precipitation of the south-west monsoon is not very great out at sea.

Though these facts undoubtedly indicate some irregularity in the flow of the south-east trade across the

[1] "Weather Charts of the Bay of Bengal and adjacent Sea north of the Equator." Issued by the Meteorological Department of the Government of India. (Calcutta, 1887.)
"On the Winds of the Arabian Sea and Northern Indian Ocean." By W. L. Dallas, late of the Meteorological Office, London. Published by the Meteorological Department of the Government of India. (Calcutta, 1887.)

Line, still we are constrained to think that the south-west monsoon is still part of the same system. If the monsoon was independent of the trade, there must be a belt of high pressure between the two; and of this there is absolutely no trace.

We must therefore look to some explanation other than the conception of an independent circulatory system over the Bay of Bengal; but materials are at present wanting to form a definite conclusion on the point at issue. There are two ways by which the question could be settled.

A few sets of observations of cloud-motion on ships coming up the Bay from southward, would almost infallibly give decisive results. If the upper clouds over the west winds, just north of the Line, come from the south or south-east, the surface wind has been drawn across the equator; but if, on the contrary, the clouds drive more and more from the north of west the higher they are, then the circulation over the Bay of Bengal is not fed directly by currents which have crossed the line.

A set of daily weather charts for the whole Indian Ocean would also clear away many doubts. When differences of pressure are small, and winds are variable, charts of mean monthly isobars, and of resultant winds, are very delusive; for the average relation of pressure, wind, and weather, may be quite different from that on any actual day.

The materials at present available point unmistakably to some connection between the anomalous wind and weather in the southern portion of the Bay, and the local area of low pressure over Southern India. It is very conceivable that the whole width of the south-east trade does not cross the equator with an unbroken front; but that for some reason or other a great local eddy may be developed in the Bay of Bengal. No river ever flows regularly, but is broken up into ripples and backwaters; and though there are many differences between the flow of water and of air, still there are certain properties common to the motion of every fluid.

Very few English meteorologists care much for theoretical discussions of air motion; but the Indian workers use mathematics freely in their investigations. Mr. Dallas calculates the flow of a current of air from 10° S. latitude to 10° N., according to the formula given by Mohn and Gulberg. He takes a gradient directed N. 30° E., across the Arabian Sea, and notes the difference both of force and direction between the observed and calculated winds. No doubt there is a certain accordance between the results so obtained; but still there are errors, which, taken with other things, suggest that the theory is still imperfect.

According to the formula—a modification of Ferrel's theory—when air flows northwards down a gradient, the angle between the wind and the gradient should decrease as we approach the equator, disappear altogether on the line, and then gradually increase as we proceed further north. But in practice the trade keeps steadily in the south-east from about 20° S. almost to the equator, then turns rather suddenly to south-west, and the monsoon advances steadily in that direction from about 5° to 20° N. In the opposite monsoon, the north-east winds run steadily from about 20° N. down to the line, and then turn rapidly to north-west.

It is well known in our own latitudes that, though the wind rotates in contrary directions round cyclones and anticyclones, the sweep of the wind is usually less than the curvature of the isobars would suggest. For instance, if an anticyclone lies to the north of Great Britain, all the winds will often be from about north-east instead of sweeping gradually from north-east through east to south-east. This and many other similar observations point to a north-east and south-west set of the winds all over the northern hemisphere, which has not yet been accounted for by any theory.

In conclusion, we may remark how thoroughly the author has discussed the subjects of his memoirs; though some will doubtless differ considerably from him in the theoretical portion of his work. India presents a field for research unique from that in any other part of the world; and those who are acquainted with the magnificent equipment, order, routine, and system of inspection inaugurated by Mr. Blanford, will feel confident that every year will add to our knowledge of a region that presents the most fascinating problems to the student of atmospheric dynamics. RALPH ABERCROMBY.

NO. 2 MUSEUM, KEW.

THE Museum of Monocotyledonous Products in the Royal Gardens, Kew, better known, perhaps, as No. 2 Museum, which was recently closed for rearrangement, has been again opened to the public. The entire collection has been classified according to the plan of the "Genera Plantarum," so that the whole of the collections contained in Museums Nos. 1 and 2 are now arranged according to the system adopted by Bentham and Hooker.

A new room which was added to the Museum a few years since has now been utilized; this has given space that was much needed for the proper display of the products of such important natural orders as *Scitamineæ*, *Bromeliaceæ*, *Amaryllideæ*, *Liliaceæ*, *Palmeæ*, *Aroideæ*, *Cyperaceæ*, and *Graminaceæ*. In the first named order, a large number of valuable economic plants are included, such as ginger, turmeric, cardamoms, arrowroot, bananas, and others; while in *Liliaceæ* we find sarsaparilla, asparagus, onions, squills, medicinal aloes, and New Zealand hemp. All these have had much more space given to them than hitherto, and the fine collection of native New Zealand garments made of the indigenous hemp (*Phormium tenax*), which are rapidly becoming scarce, are now opened out and fully shown. A very large increase of space has been given to the *Palmeæ*, and as it is one of the most important orders to mankind generally, especially in tropical countries, it was but fitting that this unique collection of palm products should be fully displayed. In such a series as that at Kew it is difficult to particularize any one exhibit as more important than another, but we may draw attention to the fine set of specimens illustrating the coco de mer, or double cocoa-nut of the Seychelles (*Lodoicea sechellarum*). This comprises a fine series of fruits, including a model of the fruit in which the nut is inclosed, made and presented by the late General Gordon, of the so-called double or usual form, as well as quadruple, sextuple, and others, besides seeds showing the mode of germination, very fine male spadices, and carved shells. The series of products of Palmyra palm (*Borassus flabelliformis*) is also a very complete one, comprising sections of the trunk, both longitudinal and transverse, toddy collecting apparatus and various manufactures from the leaves.

In the *Gramineæ*, which was very much crowded throughout, a large increase of space has enabled the interesting collections of maize, sorghums, sugar-cane products, rice, and the numerous grains of India, to be easily examined, while in the *Cyperaceæ* the Indian mats from the culms of *Cyperus Pangorei* and *C. tegetum* and other products of the order have been opened out, and now form a striking series.

APPARATUS FOR EXPERIMENTS AT A HIGH TEMPERATURE, IN GAS UNDER HIGH PRESSURE.[1]

A DIFFICULTY often experienced in laboratories is how to raise a body to a high temperature while surrounded by a gaseous atmosphere under considerable pressure.

[1] Translated from *La Nature*, February 11, 1888.

The apparatus which I constructed several years ago makes it possible to bring bodies to a temperature approaching that of the fusion of platinum, whilst maintaining them in a gaseous atmosphere, of which the nature and pressure may be varied at will.

This apparatus (Fig. 2) is composed of a mass of steel

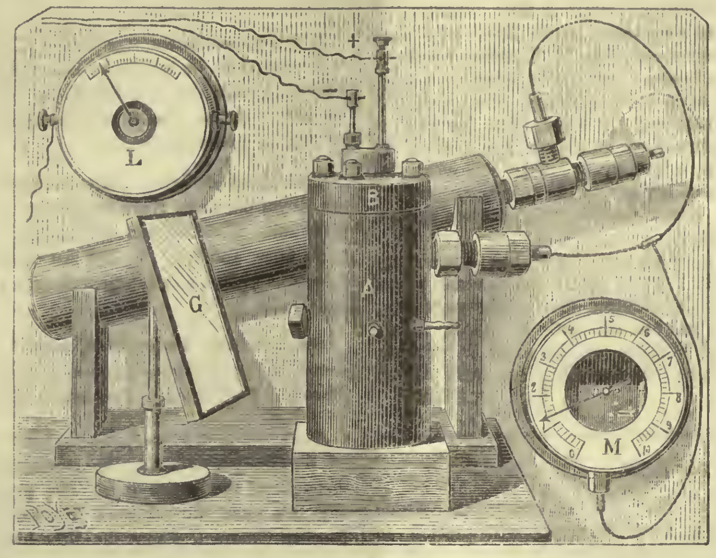

FIG. 1.—Apparatus of M. Cailletet. A, mass of steel with cylindrical bore, with its stopcock B (see the details in Fig. 2); G, mirror permitting the reaction to be seen; M, manometer; L, amperemeter.

A, in which there has been hollowed out a cylindrical space of about a quarter of a litre capacity. This species of test-tube may be closed by means of a metallic stop-

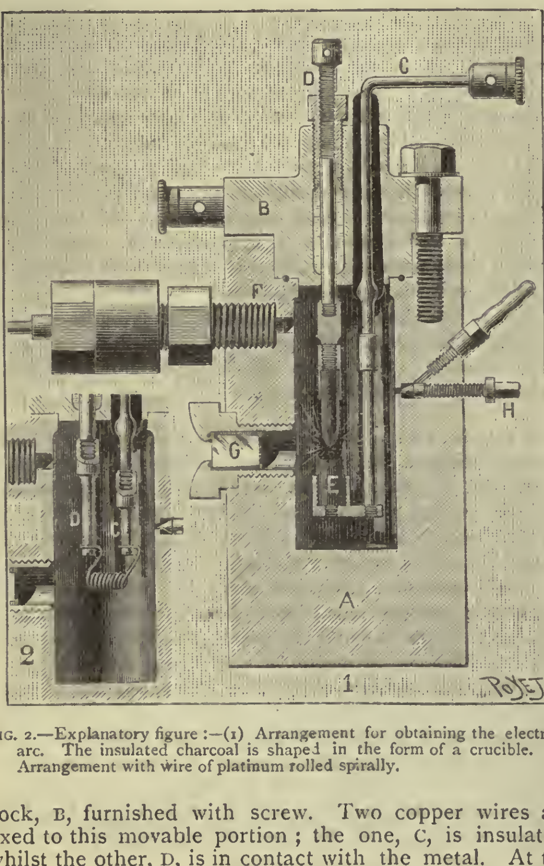

FIG. 2.—Explanatory figure:—(1) Arrangement for obtaining the electrical arc. The insulated charcoal is shaped in the form of a crucible. (2) Arrangement with wire of platinum rolled spirally.

cock, B, furnished with screw. Two copper wires are fixed to this movable portion; the one, C, is insulated, whilst the other, D, is in contact with the metal. At the ends of these two wires there is fixed, according to the requirements of the experiment, either a sheet of platinum moulded into the form of a crucible, or a wire of platinum rolled spirally, a kind of receptacle for the body experimented on, and which is brought to the desired temperature by the passage of an electric current. Two or three accumulators are sufficient for these experiments. A fragment of gold, placed in the spiral, melts in a few seconds. When it is desired to maintain the temperature long, the exhausted accumulators are replaced by others in readiness, simply by use of a commutator. The high temperature developed by the electric arc may also be turned to account; in that case two charcoal rods are arranged, of which one, movable, is fixed to the extremity of a screw, D, capable of being adjusted from the outside in order to place it in communication with the other charcoal rod, E, insulated and shaped in the form of a crucible.

The block of steel is pierced by an orifice, F, connected by a metallic capillary tube with the reservoir which contains the compressed gas. A window furnished with a thick glass, G, allows the phases of the experiment to be followed by looking in an inclined mirror, so as to be secure from all danger in case of the glass breaking. Lastly, the gases contained in the apparatus may be collected, by means of a stopcock at the screw H, in cases where it is desirable to analyze them.

The gas used for the experiments is compressed previously in a holder by means of the mercurial pump, a description of which I have already published; it is also easy to employ the carbonic and sulphuric acid furnished by commerce.

A metallic manometer fixed to the apparatus renders it possible to ascertain that the pressure of the gases exercises an energetic cooling influence upon the bodies which are heated by the electric current.

Thus, the current which causes the fusion of the wire or sheet of platinum produces only a sombre red temperature when the pressure is sufficiently great. I have been able to lessen this cause of cooling, by placing the body on which I was experimenting in a small test-tube, which resists the motion of the gases, and which is not repre-

sented in the figure. I have repeated, with this apparatus, the classical experiment of Hall on carbonate of lime. A fragment of chalk, heated in a spiral of platinum, diminishes sensibly in volume, while it is being changed into a hard body of a brownish-yellow colour, which dissolves slowly in acids, at the same time liberating carbonic acid. Also, our fellow-worker, M. Debray, has long since shown that Iceland spar can be carried to a high temperature in carbonic acid without being changed, and without losing its transparency. I have also found that a crystal of spar transformed to chalk on the surface by the action of heat under ordinary pressure recovers the lost carbonic acid, but not its primitive transparency; I have not been able to effect fusion of the spar in the course of my experiments.

To sum up, the apparatus which I have the honour to make known, and which I have used for several years past, in experiments upon the electric light under pressure, researches which I have carried on with M. Violle in his laboratory at the Normal School, will be able to render, I hope, numerous services to chemists as well as to mineralogists.

L. CAILLETET.

NOTES.

AT the Bath meeting of the British Association, which will begin on September 5, Prof. Schuster will preside in Section A (Mathematics and Physics); Prof. Tilden in Section B (Chemistry); Prof. Boyd Dawkins in Section C (Geology); Mr. Thiselton Dyer in Section D (Biology); Colonel Sir C. W. Wilson in Section E (Geography); Lord Bramwell in Section F (Economic Science and Statistics); Mr. W. H. Preece in Section G (Mechanical Science); and General Pitt-Rivers in Section H (Anthropology).

THE Croonian Lecture of the Royal Society will, at the request of the Council, be delivered this year by Prof. W. Kühne, of Heidelberg. As is well known, Prof. Kühne has for many years devoted attention to the endings of nerves in muscle, and in the Croonian Lecture he proposes to dwell on the light thrown on the nature of muscular contraction and nervous action by the study of these nerve-endings. Since the rooms of the Royal Society are not well adapted for showing illustrations to large audiences, the lecture, which will be largely illustrated, will be delivered, by the permission of the Managers of the Royal Institution, in the lecture theatre of the Royal Institution. The date fixed is Monday, May 28, at 9 p.m.

IN reply to a question put by Lord Herschell in the House of Lords on Monday, Lord Cranbrook stated that he had come to the determination to recommend the issue of a small Royal Commission to inquire as to the necessity for a Teaching University for London, and he hoped that at no great distance of time it would be able to report upon the subject.

IN accordance with the rule which empowers the election of nine persons annually "of distinguished eminence in science, literature, or the arts, or for public services," Prof. A. W. Rücker, F.R.S., has been elected a member of the Athenæum Club.

THE Royal Meteorological Society's ninth annual Exhibition of Instruments will be held at the Institution of Civil Engineers, 25 Great George Street, Westminster, in conjunction with the Society's meeting on Wednesday, the 21st inst., and will be very interesting and instructive. The Exhibition is devoted to apparatus connected with atmospheric electricity. A most valuable collection of some fifty photographs of flashes of lightning from all parts of the world will be shown, as well as some curious effects of damage by lightning, including the clothes of a man torn off his body by lightning, &c. The Exhibition will remain open till Friday, the 23rd inst. Persons not Fellows, wishing to visit the Exhibition, can obtain tickets on application to Mr. W. Marriott, Royal Meteorological Society, 30 Great George Street, S.W.

A PLANT of the common coffee (*Coffea arabica*) is now loaded with ripe fruit in the palm-house at Kew. Seldom, even on tropical plantations, is a tree to be seen with such a crop. Such an object-lesson should not be missed by those who take an interest in economic botany.

THE March Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, contains papers on Forsteronia rubber, patchouli, West African indigo-plants, vanilla, streblus paper, urera fibre, and tea. In the last of these papers valuable information is given as to the growth of tea in Jamaica, Madagascar, and Natal.

AN excellent biographical sketch of the late Asa Gray, by James D. Dana, appears in the *American Journal of Science* for March. The article is also issued separately.

A HEAVY gale was experienced last Sunday in nearly all parts of the British Islands, the storm continuing in many places throughout the entire day. The greatest violence of the gale was felt over the southern districts of England and in the English Channel, where the direction of the wind was from the south-west and west. In Ireland, Scotland, and the North of England, the direction of the wind was easterly, the central area of the disturbance passing completely over the middle of England from west to east. At 8 o'clock on Sunday morning the centre of the storm was close to Pembroke, where the barometer was reading 28·57 inches, and at 6 o'clock in the evening it was over Lincolnshire, the barometer reading 28·8 inches. The storm afterwards crossed the North Sea, and at 8 o'clock on Monday morning the centre had reached Holland, and was still travelling in an easterly direction. At Greenwich the anemometer registered a pressure of 31 pounds on the square foot at 5 p.m. on Sunday, which is equal to an hourly velocity of about 80 miles. The feature of especial scientific interest with respect to this storm is the sudden manner in which it appeared on our coasts: it practically arrived without any warning, and appears to have been formed almost within the area of the British Islands. It would seem to be a secondary or subsidiary disturbance to the storm area which was situated over Scotland on Saturday, and was apparently formed in the south-western segment of the parent cyclone, which is the favourite position for storm development. The passage of such a storm across our islands illustrates very clearly the immense difficulty which underlies any system of forecasting.

IN vol. iii., part 2, of the Indian Meteorological Memoirs, recently published, Mr. Blanford has continued his discussion of the rainfall of India. Part 1, which dealt more particularly with the average conditions of rainfall, was fully noticed in NATURE (vol. xxxvii. p. 164). The part now in question relates to the variations and vicissitudes of rainfall in past years, and their connection with other elements. With the view of ascertaining whether any general laws can be detected, an endeavour is made to determine what peculiarities are associated with the different distribution of rainfall, *e.g.* the variations of prevailing wind currents, distribution of atmospheric pressure, and the frequency and courses of cyclonic storms. The periodical recurrence of droughts and famines since 1769 is recorded, and, from general conclusions drawn, it appears that serious droughts occur in Southern India at intervals of nine to twelve years, and that they generally happen about a year before the sunspot minimum. In Northern India, droughts sometimes occur in years of maximum sun-spots.

AT the meeting of the French Meteorological Society, on February 7, it was announced that M. Janssen had offered five prizes, consisting of silver medals, for the best works relative to the application of photography to meteorology, and M. Teisserenc de Bort offered a similar prize for the best measurements of the height of clouds. M. Moureaux (Secretary) presented a paper on the periodicity of disturbances of declination and horizontal force at Parc-Saint-Maur Observatory for the years 1883–87, showing, by means of curves, that the monthly values of both those elements exhibited two maxima at the equinoxes, and two minima at the solstices; and that, while the monthly variation of the number of disturbances appeared to follow a general law, the diurnal variation seems to be subject to complex laws. M. H. Lasne presented a note on the gyratory movements of the atmosphere, in connection with the experiments of MM. Weyher and Colladon on the motions of fluids. M. Maillot exhibited a kite, arranged to maintain a constant height for some time, and designed for the purpose of facilitating the registration of variations of temperature at certain altitudes.

A TETRASULPHIDE of benzene has been prepared in the pure state by Dr. Otto, of Brunswick (*Journ. für. prakt. Chemie*, 1888, Nos. 3 and 4). When a current of sulphuretted hydrogen gas is led through a warm dilute solution of benzene-sulphinic acid, $C_6H_6SO_2$, in alcohol, the sulphinic acid is reduced to phenyl-disulphide, $(C_6H_5)_2S_2$, a substance already well known. The behaviour, however, is entirely different when a very strong solution is employed: the liquid becomes rapidly yellow, and eventually monoclinic crystals of sulphur and a yellow oil separate. This yellow oil was found to consist of phenyl-tetrasulphide, $(C_6H_5)_2S_4$, the analyses indicating an exceptionally pure product, after careful separation from the free sulphur by dissolving in ether and subsequent evaporation. This tetrasulphide at the ordinary temperature is a very viscid, heavy, highly refracting oil, possessing an unpleasant odour reminding one of mercaptan. It is a comparatively stable compound, remaining unattacked on treatment with sodium sulphite, even when warmed for a long time; but on warming with colourless ammonium sulphide it is reduced to disulphide, polysulphide of ammonium being formed. According to Klason, phenyl-tetrasulphide is also the product of the action of dichloride of sulphur, S_2Cl_2, upon thio-phenol, $C_6H_5 . SH$, the mercaptan of the benzene series, and Otto shows that this is really the case, the reaction going best when the two substances are gradually mixed in carbon bisulphide solution. No extraneous heat is necessary, the operation being itself attended by a considerable evolution of heat. On distilling off the bisulphide of carbon, the resulting oil is found to be identical with the phenyl-tetrasulphide prepared in the above manner.

THE thirty-ninth Bulletin of the U.S. Geological Survey consists of a paper embodying the results of the investigations of Mr. Warren Upham upon the upper beaches and deltas of the extinct Lake Agassiz, which, in Glacial times, occupied the basin of the Red River of the North. Mr. T. C. Chamberlain, geologist in charge of the Glacial Division, in transmitting Mr. Upham's paper to the Director of the U.S. Geological Survey, for publication, wrote:—"This is but an initial contribution, embracing only so much of the data gathered as from their degree of completeness and interest warrant present publication as a record of results. The investigation is still in progress, and the general discussion of data and the eduction of conclusions are reserved until its completion. Meanwhile the great mass of carefully-determined facts here recorded will, besides their inherent independent value, be of important and immediate service to the students of other extinct and shrunken Glacial lakes."

THE U.S. Department of Agriculture has issued an interesting descriptive catalogue of manufactures from American woods, as shown in the exhibit of the Department at the Industrial and Cotton Exposition at New Orleans. The compiler is Mr. C. R. Dodge. He has brought together many interesting facts about the uses of woods in architecture and building, in transportation, in implements of industry, in articles relating to trade, in articles for man's physical comfort or luxury, and in articles for education, culture, or recreation. There is also a paragraph on "miscellaneous uses," under which are such headings as "Gun-stocks," "Artificial Limbs," "Crutches," and "Umbrella-sticks and Canes."

MESSRS. GURNEY AND JACKSON will issue in April the first part of "An Illustrated Manual of British Birds," by Mr. Howard Saunders. The work will be completed in about twenty monthly parts.

WE have received the eighteenth Annual Report of the Wellington College Natural Science Society. It contains a record of much good work done during the past year. The Report includes abstracts of lectures delivered before the Society, observations made of the plants, insects, and birds contained in the Royal Meteorological Society's lists, and a meteorological report for every day of 1887.

IN the twelfth Annual Report of the President of the Johns Hopkins University, Baltimore, Dr. Gilman says that during the last year the number of teachers in connection with the institution was slightly enlarged, and the number of students considerably increased. A new department of instruction—pathology—was initiated; a physical laboratory, the largest and costliest building yet erected for the University, was completed and occupied; a building was set apart for the petrographical laboratory; and an astronomical observatory, for the instruction of students, was equipped. The cost of the physical laboratory, including the land, furniture, gas-fitting, steam-heating, and astronomical dome (but not including large amounts paid previously for instruments and apparatus, and not including the dynamos, nor the telescope), stands, in the books of the treasurer, 174,765·86 dollars. This building will be used by classes studying mathematics, astronomy, and physics.

THE Calendar, for the year 1888, of the Royal University of Ireland has just been issued. The Drapers' Company have offered an exhibition of the average annual value of £35 for three years, to be awarded, on the result of the matriculation examination of this University, to the girl who, complying with certain conditions stated in the Calendar, shall be awarded either first or second class honours in at least two subjects, and who shall obtain the highest aggregate of marks at the examination to be held on July 4 next.

WE have received the Calendar, for 1887-88, of the Imperial University of Japan. An address by President Watanabe, on the occasion of the graduation ceremony, July 9, 1887, is printed as an appendix. If we may judge from the tone of this address, the University is in a prosperous condition, and doing justice, in its courses of instruction, to science no less than to literature and law.

ON February 12, Mr. Jeremiah Curtin read, before the Anthropological Society of Washington, a paper of some interest on the folk-lore of Ireland. Last year Mr. Curtin went to Ireland for the express purpose of finding out how far the old "myths and tales" were still alive in the minds of the people. He visited some secluded parts of the western coast, and "took down personally a large body of myths and stories, some very long, others not so long." "This collection of materials," he says, "is sufficient to fill a couple of 12mo volumes, and will

give some idea of what yet remains in the Celtic mind of Ireland. It is, however, but a small part of the mental treasure still in possession of the people."

In the Proceedings of the American Philosophical Society (July–December 1887) there is a most interesting paper by Dr. D. G. Brinton on ancient footprints in Nicaragua. The discovery of human footprints in volcanic rocks near the shore of Lake Managua, Nicaragua, under circumstances which seemed to assign to them a remote antiquity, was announced several years ago. Dr. Brinton refers especially to a specimen on tufa sent to him from Nicaragua, by Dr. Flint, an accurate representation of which accompanies the paper. It is the impression of a left foot. The total length of the impression is 9½ inches, the breadth at the heel 3 inches, at the toes 4½ inches. The apparent length of the foot itself was 8 inches. The instep was high, and the great toe large, prominent, and exceeding in length the second toe. The greatest depth of the impression is at the ball of the foot, the weight being evidently thrown forward, as in vigorous walking. At this part the maximal depression below the plane of the superficies is 2 inches. Dr. Brinton has no doubt as to the genuineness of the footprints; but their antiquity, he thinks, is uncertain. His own opinion is that there is not sufficient evidence to remove them beyond the present post-Pliocene or Quaternary period.

PROF. DAVID P. TODD, astronomer in charge of the recent American Eclipse Expedition to Japan, has issued a Preliminary Report (unofficial) on the total solar eclipse of 1887. Associated with this document is a Preliminary Report by Dr. W. J. Holland, naturalist of the Expedition.

WE have received an illustrated catalogue of the astronomical instruments and observatories of Sir Howard Grubb, Dublin. During the last few years Sir Howard Grubb has executed important astronomical work for many Governments, Universities, scientific Societies and Academies, and the catalogue affords striking proof of the care he takes to bring his various methods and processes to perfection.

MR. STANFORD has issued an interesting volume, by Mr. A. B. Macdowall, entitled "Facts about Ireland." It is an attempt to show, by means of curves, the recent history of various elements in the social life of the Irish people. Sections are devoted to population, agriculture, education, emigration, evictions and drunkenness, crime, consumption of spirits, bank deposits, &c., and occupations. The mode of representation, which has been made familiar to most people by weather charts, has enabled Mr. Macdowall to bring together in brief compass a great mass of information about some very complicated and difficult subjects.

LAST month Mr. J. Clayton read before the Bradford Naturalists' Society a paper on *Pinus sylvestris*. This fact is worth mentioning, because by using the autocopyist apparatus the author was able to give each member a sheet of drawings, and another of explanations. We have received specimens of these sheets, and it seems to us that the plan might often be adopted with advantage by readers of papers, and by lecturers, on scientific subjects.

INQUIRIES have been made by several correspondents as to the photographic apparatus used by M. Marey in obtaining the results as to the flight of birds set forth in the article in *La Nature* of which we lately printed a translation (p. 369). No complete account of the apparatus has yet appeared, but a summary of the facts relating to it was given in the *Comptes rendus* for July 3, 1882. A note on the subject was printed in the same publication on August 7, 1882. M. Marey proposes to give a full description in a work on the flight of birds and insects, which will be issued in the course of the present year. The object is to obtain an indication, at every instant, of the swiftness of the moving body which is to be photographed, in its passage from point to point. To secure this indication it is necessary to produce, at known intervals, equal to one another, and as short as possible, interruptions in the arrival of light into the interior of the photographic apparatus. These interruptions M. Marey obtains by causing to turn, before the objective, by means of machinery, a wheel which makes ten revolutions in a second. This wheel has ten spokes, each one of which, in its passage, interrupts the light. The "eclipses" thus caused occur, therefore, a hundred times per second; so that in the photograph the space between two consecutive points represents the space crossed by the photographed body in 1/100 of a second. In order to indicate the relative positions of the different parts of the body at the same instant, M. Marey makes one of the spokes of the wheel twice as large as the others. The result, of course, is that there is a longer "eclipse" at the moment when this spoke passes. This arrangement enables the observer to determine without hesitation the relative positions of the different points of the body at every tenth of a second; and it has also the advantage of facilitating the calculation of the times in which the movements are made.

AT Stevens's rooms, on Monday, an egg of the extinct great auk (*Alca impennis*) was sold to Mr. J. Gardner for £220. It belonged to the collection of Mrs. Wise, whose husband bought it of a dealer in Oxford Street in 1851 for £18. It was originally brought to England from Paris, and is now said to have been bought for America.

THE Exhibition of Japanese engravings at the Burlington Fine Arts Club, and that of Japanese pictures in the White Gallery in the British Museum, contain much that is of a specially scientific interest; as, indeed, could scarcely fail to be the case with such exhaustive and well-arranged collections of the pictorial art of a people who, beyond all others, went to Nature herself as the fount of their inspiration. Thus the collection of guide-books and topographical hand-books in cases K, L, and M, at the Burlington Club, give a remarkable picture of the physical features of Japan, and one that, taken in conjunction with such a work as Dr. Rein's, should be of much assistance to geographers. Probably no literature in the world is so plentifully supplied with guide-books as that of Japan; every province, town, and district, has one or more of its own. In many cases they are works of ambitious scope and wide utility. They indicate "all the spots famous for landscape beauties, collect learned records of the historical and legendary lore of the localities described, enumerate the various objects of curiosity and archæological importance preserved in the neighbourhood, contribute scientific notes upon the flora and fauna of the district, and open a fund of practical information as to industries, commerce, and a hundred other matters of interest both to resident and visitor." Botany is remarkably well illustrated in the books shown in case O; while the silhouettes in case H, traced with great accuracy from nature, represent almost every type of the lower middle class Japanese, and should be of some ethnographical value, especially as there is a considerable number of them. The many hundreds of birds and other animals represented in the British Museum collection (it will of course be understood that we are now referring to the whole of the Anderson Collection, consisting of about 4500 examples, not merely to the 273 on exhibition) would form a supplement to the works of Siebold and later writers. For the student of religions the Buddhistic pictures supply many details not to be found in any written records, and the many volumes of popular picture-books show a thousand elements of Oriental folk-lore, customs, and handicrafts that are now on the verge of extinction.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mrs. A. Ballard; a White-fronted Capuchin (*Cebus albifrons* ♂) from South America, presented by Mrs. E. A. Lediard; a Binturong (*Arctictis binturong*) from Malacca, presented by Mr. J. P. Rodger, of Selangor, Malay Peninsula; a Common Quail (*Coturnix communis*), captured at sea, presented by Captain Christian; a Brazilian Tortoise (*Testudo tabulata*) from Brazil, deposited; four Cape Colys (*Colius capensis*) from South Africa, received in exchange.

OUR ASTRONOMICAL COLUMN.

ANNALS OF HARVARD COLLEGE OBSERVATORY.—We have received Part 2 of vol. xiii. of the Annals of Harvard College Observatory, containing zone observations with the transit wedge photometer attached to the 15-inch equatorial. These observations were undertaken in order to extend our knowledge of the relative brightness of the fainter stars, and to determine the scales of magnitude employed in the estimates of certain observers, as compared with magnitudes as obtained by means of the meridian photometer. These zone observations were not, however, made with the meridian photometer, but with an adaptation of Prof. Pritchard's wedge photometer, which had been devised by Prof. Pickering. Instead of slipping the wedge along by hand, the telescope and wedge are fixed, and the star is carried from the thin part of the wedge towards the thick by the diurnal motion. An occulting bar is fixed near the thin edge, and the interval between the time of the occultation of a star by the bar, and its extinction by the wedge, is proportional to the magnitude of the star on the assumption of a uniform scale of absorption throughout the wedge. In this way the relative magnitudes, right ascensions, and (by estimating the point on the bar where they are occulted) the declinations of a number of stars were determined with great rapidity, and the results made comparable with magnitudes observed with the meridian photometer by the observation of a sufficient number of standard stars. The observations were made in three zones each 10′ in breadth, and lying immediately to the south of N. Decl. 1°, 50°, and 55°, the first zone being part of those observed more than twenty years ago by Prof. Bond, and the other two being situated on the north and south margins of the zone recently revised with the Harvard College meridian circle. A comparison of the D.M. magnitudes between the 7th and 9th with magnitudes as determined in the preceding manner show that the former closely correspond to the magnitudes derived from the mean of the three zones, the zone at 1° N. giving a value of about two-tenths of a magnitude less than the other two. But for fainter stars the three zones are in close accordance with each other, whilst the D.M. values give in comparison too small a magnitude, the difference increasing rapidly until 9·5 magnitude in the *Durchmusterung* is found to correspond to 10·5 with the wedge photometer. Prof. Bond's scale, on the other hand, corresponds fairly to that of the photometer from 7·0 magnitude up to 11·0, but beyond gives magnitudes which are too large, so that his 13·5 magnitude corresponds to about 12·5 of the wedge.

Prof. Pickering is still continuing the investigation, and proposes to give hereafter a far more complete comparison for D.M. stars brighter than 9·0 magnitude with magnitudes as given by the meridian photometer.

WASHINGTON ASTRONOMICAL OBSERVATIONS, 1883.—The volume of the Observations of the Naval Observatory, Washington, for 1883, has been published, and contains the usual routine observations, the bulk of the volume being devoted to the work with the transit-circle, beside three Appendixes, of which two, by Prof. Hall, on the orbits of the inner satellites of Saturn, and on the observation of certain stars for stellar parallax, have already been noticed. The third Appendix is on the Observatory temperature-room and the competitive trials of chronometers in 1884 and 1886. A number of double stars, the satellites of Saturn, Uranus, and Neptune, and the ring of Saturn, had been observed with the 26-inch equatorial; but no remarkable changes were noticed in the ring. The prime vertical was brought into use on November 14, 1882, and 580 observations of stars with small meridian zenith distances, at the times of the maxima of aberration, were secured. The meridian transit instrument of 5½ inches aperture, by Estel, was also used regularly, and 1408 observations secured with it. The 26-inch equatorial was used for the observation of minor planets, comets, and occultations. The Report of the Superintendent includes a notice of the Transit of Venus Expeditions of 1882, and of the reduction of the zone observations made in Chili in the years 1850-51-52, under Capt. Gilliss. A copy of the letter of the Superintendent, asking for a grant of $586,138 for the purpose of erecting the new Observatory, is also given, together with the recommendation of the architect that the entire amount be appropriated in one sum.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *MARCH* 18–24.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 18

Sun rises, 6h. 7m.; souths, 12h. 8m. 2·0s.; sets, 18h. 9m.: right asc. on meridian, 23h. 54·0m.; decl. 0° 39′ S. Sidereal Time at Sunset, 5h. 56m.

Moon (at First Quarter March 20, 21h.) rises, 8h. 52m.; souths, 16h. 26m.; sets, 0h. 10m.*: right asc. on meridian, 4h. 13·0m.; decl. 16° 30′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	5 23	10 42	16 1	22 27·5	8 51 S.
Venus.....	5 21	10 18	15 15	22 3·8	12 50 S.
Mars	20 47*	2 7	7 27	13 51·8	8 38 S.
Jupiter....	0 22	4 34	8 46	16 18·6	20 25 S.
Saturn....	12 22	20 21	4 20*	8 8·1	20 47 N.
Uranus ...	19 41*	1 16	6 51	12 59·9	5 38 S.
Neptune..	8 17	15 57	23 37	3 43·4	18 3 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

March.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
18	B.A.C. 1351	6½	18 42	19 49	162 287
18	75 Tauri	6	22 2	near approach	48 —
20	χ^3 Orionis	6	17 11	18 32	70 297
20	68 Orionis	6	22 21	23 25	136 302
23	B.A.C. 2683	6	0 23	1 13	154 264

March.	h.	
20	4	Sun in equator.
22	8	Jupiter stationary.
23	4	Saturn in conjunction with and 1° 21′ north of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
Algol	3 0·9	40 31 N.	Mar. 24,	1 44	*m*
λ Tauri	3 54·5	12 10 N.	,, 18,	20 57	*m*
R Canis Majoris	7 14·5	16 12 S.	,, 18,	23 32	*m*
T Hydræ	8 50·2	8 43 S.	,, 18,		*M*
W Virginis	13 20·3	2 48 S.	,, 18,	1 0	*m*
δ Libræ	14 55·0	8 4 S.	,, 21,	0 14	*m*
T Libræ	15 4·4	19 36 S.	,, 18,		*M*
U Coronæ	15 13·6	32 3 N.	,, 23,	23 32	*m*
S Scorpii	16 11·0	22 37 S.	,, 23,		*M*
S Ophiuchi	16 27·8	16 55 S.	,, 20,		*M*
S Herculis	16 46·8	15 8 N.	,, 18,		*m*
U Ophiuchi	17 10·9	1 20 N.	,, 21,	3 46	*m*
			,, 21,	23 52	*m*
X Sagittarii	17 40·5	27 47 S.	,, 18,	3 0	*M*
Z Sagittarii	18 14·8	18 55 S.	,, 19,	1 0	*M*
β Lyræ	18 46·0	33 14 N.	,, 24,	2 0	m_2
S Vulpeculæ	19 43·8	27 1 N.	,, 22,		*m*
χ Cygni	19 46·3	32 38 N.	,, 19,		*M*
S Sagittæ	19 50·9	16 20 N.	,, 19,	2 0	*m*
T Vulpeculæ	20 46·7	27 50 S.	,, 23,	1 0	*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers.

	R.A.		Decl.		
Near 55 Aurigæ ...	... 98	...	46 N.	...	March 14-25.
,, θ Ursæ Majoris ...	143	...	49 N.	...	March 20.
,, β Ursæ Majoris ...	162	...	58 N.	...	Rather slow.

THE PUBLIC GARDENS OF BRITISH INDIA, ESPECIALLY THE BOTANIC GARDENS.[1]

THE appearance of the hundredth Annual Report of the Royal Botanic Garden, Calcutta, is an event of no little interest in the botanical world, not alone for what it contains, but also for the evidence it affords of the vitality and vigour of the institution, the primary object of which was to disseminate useful information respecting the vegetable products of the possessions of the "Company," and to introduce exotic plants of economic value.

Dr. George King, F.R.S., the present able Superintendent, gives a concise history of the foundation and progress of the Garden down to the present time; and the appendices show that the establishment was never conducted with greater activity. We might make some interesting extracts from the present Report; but our object now is to give a foreigner's view of the principal horticultural establishments in India.

Mr. Warburg roughly classes the gardens under three heads, according to their degree of scientific and practical utility, as distinguished from purely pleasure-gardens, though no hard and fast line can be drawn, because some of the gardens are maintained partly for pleasure and partly for profit. There are only three real botanic gardens in India—we let Mr. Warburg speak for himself—namely, Calcutta, Madras,[2] and Saharunpore, unless we count the garden at Ganesh Khind, near Poona, which is often erroneously called a botanic garden. Besides these, there is the botanic garden at Peradeniya in Ceylon, which, however, comes under the Colonial Office. Of the officially so-called botanical gardens, two were originally founded as such by far-seeing officials: Calcutta by General Kyd in 1786, and the Ceylon Garden by Sir Joseph Banks in 1810; the latter having been established at Peradeniya ever since 1821. The origin of the Sarahunpore garden in the North-West Provinces I did not ascertain [it was originally a pleasure garden of the native princes, and when Lord Moira conquered the Mahrattas he caused it to be transformed into a botanic garden; and the first Superintendent was Dr. Govan (1816-23), who was succeeded by the better-known Dr. Royle, Dr. Falconer, and Dr. Jameson]; and the gardens of Ootacamund and Singapore have passed through various stages before attaining their present condition. Of agricultural experimental gardens I am acquainted with those of Kandesh (Bombay Presidency), Saidapet (near Madras), Nagpore (Central Provinces), and Hyderabad (in the Deccan).

There is also a horticultural garden in Lucknow, an agri-horticultural garden in Lahore (Punjab), and the beautiful garden at Madras belonging to an Agri-horticultural Society. Similar Societies exist in Calcutta, Rangoon, and probably in other places; the first publishing a special Journal.[3]

In almost every town where there is a considerable European population or garrison there are ornamental gardens or parks, called into existence by the demand, and almost necessity, for some such place for social recreation—riding, driving, and walking—in a tropical country, where many of the pleasures and amusements of our Europeans towns cannot be enjoyed. Then there are numerous extensive and costly gardens belonging to the native princes and nobles.[4]

Respecting the gardens having a practical aim, we may be very concise, as their objects are much the same, subject only to the climatal differences of the various provinces, and consequently the kinds of plants that may be profitably cultivated within their several radiuses of activity. The manner in which these practical ends are attained consists on the one hand of experiments and trials in the acclimatization of useful and ornamental exotic plants; and on the other hand of raising new and improved varieties of native plants; and when successful results follow, propagation on a large scale is practised for free distribution or sale. Thus, for instance, during the year 1884-85 the Calcutta Garden sent out 23,500 living plants to various places in India, and forty-two Wardian cases of plants to foreign countries. Further, some 3000 packets of seeds were distributed; yet the proceeds amounted to only 1075 rupees, because one of the principal functions of the Calcutta Garden is to provide the public gardens and pleasure-grounds with plants.

In the same year the Saharunpore Garden distributed as many as 42,000 plants and 21,300 packets of seeds; whereof 31,400 plants and 14,000 packets to private persons; the amount received being 8500 rupees. But ornamental plants, both as living plants and seeds, occupy the first position, while fruit-trees, timber-trees, and seeds of vegetables take a secondary place.

The Singapore Garden sent out the large number of 163,000 living plants in 1884. These figures, however, are merely extracted as examples of what is done by the different establishments, and afford no idea of their relative importance, inasmuch as the number of plants distributed by each one is subject to the greatest fluctuations; in illustration of which it may be mentioned that the Saharunpore Garden distributed 146,000 plants in 1882-83, against 42,000 in 1883-84; the difference being almost made up by 100,000 plants of agave. Similarly in 1884 the Horticultural Gardens in Madras sold 100,000 plants of the "Mauritius hemp," *Fourcroya gigantea*.

As already observed, the nature of the work of the different gardens varies according to the requirements of each district. In many parts, especially in Ceylon, the Nilghirries, British Sikkim, the interests of European planters have to be considered first; in the rice-growing districts of the Ganges, Malabar, and Ceylon, the things cultivated in the gardens and plantations engage special attention. In Bengal, jute, indigo, and to some extent opium, and in Central and Northern India improvements in the cultivation of cereals, are of primary consideration; while in the Bombay Presidency and some parts of Ceylon cotton is added thereto; often associated with the latter the sugar-yielding palm, *Borassus flabelliformis*. For the dry regions of the Punjab it is a question of finding suitable woody plants for afforestation, as well as for the saline soil of the North-West Provinces, in order to provide fuel for the agricultural districts, and thereby gain the dung of cattle for purposes of manuring. And among other things of vast importance is the conservation and renewal of the rapidly disappearing caoutchouc forests of Malacca.

The Singapore Garden has only been a scientific establishment since 1882, when it was placed under the direction of Mr. Cantley; but much has been done in these few years without destroying the natural beauties of the old garden. A small herbarium has been formed, and the most necessary buildings erected. The new plantations are, as far as possible, systematically grouped. A special charm of this Garden is a remnant of the original forest, traversed only by a few paths, where one can enjoy, in a small way, the delights of tropical vegetation without the fatigue attending excursions in pathless forests. The fern garden and the palmetum promise to be very rich and attractive; but a larger income is necessary to carry out the functions of a botanic garden fully and expeditiously. It is perhaps superfluous to add that the Director has to superintend the gardens and promenades of the town; but in order to understand the whole of the circumstances, it is important to bear in mind that he has also been placed at the head of the newly created Forest Department for the whole of the Straits Settlements—an arrangement which of course causes him no inconsiderable amount of additional labour.

Seeds and plants are continuously being distributed from Kew, where all new things are reported and presented, and where competent authorities are consulted on the merits of the samples sent in. At this centre advice is sought, and there is a constant interchange of ideas and experiences between it and the Indian establishments, the advantages of which are so evident that it is unnecessary to enumerate them.

With the exception of rice, tropical cultivation generally is so uncertain and subject to fluctuation, owing to the conditions of labour, communication, and credit, that improvements are very slow; and the experimental work is not so systematically con-

[1] Chiefly from an article by O. Warburg in vol. xliv. of the *Botanische Zeitung*.

[2] Mr. Warburg refers here doubtless to the Madras Presidency, as the botanic garden is at Ootacamund in the Nilghirries, and not at Madras. It should be understood that we are only extracting passages from a rather long article.

[3] And we may add that there is an experimental garden in the mountains at Mussoorie in connection with Saharunpore; another at Darjeeling, partly pleasure and partly practical; and an important experimental garden at Mongpo (Sikkim), under Mr. J. Gammie: the two last offshoots of Calcutta.

[4] We must pass on to what Mr. Warburg has to say concerning the conditions and functions of the botanical gardens and their adjuncts.

ducted as with us. There are too few officers, and everybody has too much to do; nevertheless many of the reports exhibit an amount of zeal and industry deserving of all the more recognition on account of the difficulties under which much of the work is done.

From this point Mr. Warburg explains and describes in some detail what has been effected by the combined action of Kew and the Indian botanic gardens in the introduction, resulting in the extensive cultivation, of economic plants of the first importance, such as the cinchona, tea, and coffee, the cultivation and manufacture of which have developed into industries of incalculable value. He further alludes to the cultivation of rubber-trees, ipecacuanha, fibre-yielding plants, &c., which is, in many instances, still in a more or less experimental stage. He also enters into particulars and comparisons of the climate of different districts in its relations to cultivation, and altogether his Report is an interesting and instructive one, containing much information new to the English public. He specially mentions the great interest taken in the Madras gardens by Sir Mountstuart Grant-Duff, and the material assistance he extended to Prof. Lawson. And he concludes with a brief review of the literature directly or indirectly connected with the botanic gardens of India, culminating in Sir Joseph Hooker's gigantic undertaking, "The Flora of British India." With regard to the intimate connection between Kew and the Colonial and Indian gardens, Mr. Warburg thinks it is at present most beneficial, though he looks forward to the time when they shall have developed so far as to be less dependent on a central institution.

SCIENTIFIC SERIALS.

Revue d'Anthropologie, troisième série, tome iii. fasc. 1 (Paris, 1888).—On the colour of the eyes and hair among the non-nomadic Tunisian tribes, by Dr. R. Collignon, based on the observations of Capt. Rebillet and Lieut. Fannezo. These observations, which were conducted in accordance with the methods employed in France for similar investigations, refer to more than 2000 individuals belonging to the "sedentary" or settled populations of the towns and rural districts. The men observed being all regular soldiers, the tables do not refer to any nomads of Arab race, since all the dwellers in tents are exempt from conscription in Tunis. Expressed in general terms, among these 2030 individuals, dark eyes occurred in 1543 cases, or 7·6 per cent., and light eyes in 69 cases, or 3·5 per cent.; while dark hair occurred in 1887 cases, or 92 per cent., and light hair only in 7 cases, or 0·4 per cent. On considering the data obtained from a comparison of the tables referring to different districts, it is found that the blond type occurs only sporadically, and almost exclusively in the littoral settlements, on which account Dr. Collignon thinks it may be assumed that its presence in the Tunisian population is due to the incidental amalgamation of foreign elements through invasion or immigration by more northern races.—On the colour of the eyes and hair in Denmark, by Herr Soren Hansen (communicated to the Society by Dr. Topinard). From this paper we learn that observations made on 2000 males of the age of twenty, belonging to the southern and eastern districts of Jutland, yielded the following results: light, *i.e.* blue, eyes, 1527; dark eyes, 65; leaving 408 of medium colour. In regard to the colour of the hair it was found necessary to establish four groups, which gave the following figures: dark brown, 306; medium, 1267; light (blond), 333; and red, 94. From this it would appear that the majority of the population have blue eyes, and medium brown, or chestnut, hair. A further analysis of Herr Hansen's tables shows that while the perfect brown type—*i.e.* where both hair and eyes are dark—occurs only in 2·7 per cent.; blond hair and light eyes are met with in 16·2 per cent. Finally the curious circumstance has been deduced that while light eyes are twenty-four times more frequent than dark ones, light hair is only seven times more frequent than dark hair; hence Dr. Topinard is led to ask whether the explanation of this peculiarity may not have to be sought in some general law by which in a mixed race, descended from blond and dark races, the eyes may be more generally transmitted from the former, and the hair from the latter.—On recruiting in the cantons of St.-Omer, by Dr. H. Favier. The enormous difference in the cantons north and south of St.-Omer in the number of persons available for military service has been attracting much notice among French officers of late years. According to M. Costa, who wrote on the subject in 1866, these differences are due to hygienic causes; the district north of St.-Omer, where the rejections are only 227 in 1000, being well adapted to agricultural and other rural pursuits, while in the southern canton, where the rejections amount to 342 in 1000 the lands are almost all marshy, exposing the inhabitants to fevers and other malarian influences by which the race is deteriorated. Dr. Favier does not believe that these causes affect the question in any way, but, even if they did so when M. Costa wrote, statistics prove that of late years, more especially since the stricter law of conscription of 1872 has been put into force, the south canton has shown a gradual diminution in the numbers of rejections; and while he denies the action of malarian causes or the influence of differences of ethnic origin between the people of the two cantons, he believes that to industrial centres, such as d'Arques in the southern canton, may very possibly be ascribed certain conditions antagonistic to the success of recruiting.—On the "*castellets*" of Mont Sainte-Baume in Provence, by Dr. Beranger-Féraud. The presence of numerous little heaps of stones on the higher peaks of Mont Sainte-Baume has repeatedly arrested the attention of strangers, and the fact of their having been deposited by the hand of man is now confirmed by Dr. B. Féraud, who last year made the ascent of the mountain for the purpose of investigating their character and purpose. These so-called "*castellets*" (little castles) are either composed of several stones forming a rude sort of pyramid, or of one large stone inserted in a fissure of the rocky soil. Although widely distributed, they are most frequent in the vicinity of the oratory of Saint-Pilon, where they are found at an elevation of nearly 1000 feet, close to the edge of the vertical wall of rock forming the northern boundary of the range. On inquiry he learnt that these structures were also locally designated *moulouns de joye* (heaps of joy), and that they were not alone intended to testify to the successful ascent of the pilgrims to the summit of St. Pilon, but were frequently designed to propitiate St. Magdalen, to whom prayers are made on the spot for approval of the special maiden whom the worshipper may desire to marry. In the latter case the mound is visited by the builder at the end of a year, and if he finds the stones undisturbed he considers that the saint approves of his choice; if, however, the heap is broken up, this is generally regarded as a decisive barrier against the intended marriage. In this superstition, Dr. B. Féraud sees a survival of the ancient usage of erecting stone monuments as altars, pillars, menhirs, &c., to commemorate some important personal event.—On inequality amongst men, by M. de Lapouge. In this address the view is boldly advocated that a man is what his birth made him, and that education can do no more for him than develop the pre-existing germs derived from his progenitors in accordance with the laws of heredity. This reasoning is extended to classes, nations, and races, who are assumed to be unequal, and incapable of attaining to an equal degree of perfection. The writer divides men into four classes, in the first of which he places those possessed of creative and initiative faculties above their fellows, while it is to the relative numerical preponderance of this class over the others that he refers the undoubted superiority of one race over another. He thus sees in the dolichocepalic blonds the most favoured of all the races of humanity, since, from the dawn of history, all heroes and leaders among men have belonged to this type. In modern times the Anglo-Saxon race has owed its superiority to the preponderance of this dolichocephalic element. He believes that France is suffering from the diminution of this type in its population, together with the rising predominance of the brachycephalic type to which the lower classes of the community belong, while he anticipates as inevitable a great deterioration of the general national character through the amalgamation of the two. Similarly he sees in the present movement for raising the negro races a deep source of danger in the future to the more highly gifted Aryan races, who may in time find themselves beaten down by the brute force of teeming masses of inferior brachycephalic peoples. Such are some of the leading points in M. de Lapouge's treatise, which, notwithstanding its redundancy of diction, and the dogmatism with which certain views are maintained, is a highly interesting, suggestive, and learned contribution to ethical inquiry.

Bulletin de l'Académie Royale de Belgique, December 1887.—On some new derivatives of normal heptylic alcohol compared with their homologues, by C. Winssinger. After describing the mode of formation and special properties of normal heptylic alcohol, of the chlorides of heptyl, heptylic mercaptan, oxy-

sulphide, sulphone, and some other new bodies, the author develops some general considerations on the homologous series to which belong the heptylic sulphureted derived substances. These considerations throw fresh light on the evolution of the physical and chemical properties of compound bodies through the various species of a common genus. Thus it is shown that the chemical character of the heptylic combinations must be considered as the development of properties whose source or origin is already found in the lower terms of the series of which heptyl is a member.—A contribution to the study of the development of the epiphysis and of the third eye in reptiles, by M. Francotte. This third eye, of the invertebrate type, already described by Graaf and Spencer, is here exhaustively studied in a large number of reptilian embryos from the province of Namur, in all of which it is very distinctly traced from the epiphysis at the roof of the thalamencephalon to the complete development of the pineal organ. In one species of lizard this eye passes through a series of successive phases each realized in a permanent way in one or other of the adult reptiles. But in all of them the optic nerve has disappeared, which connected the organ with the nerve-centres for a short time in the embryonic state.—This number of the *Bulletin* contains an exhaustive memoir on the fresh-water fishes of Belgium, by Baron Edm. de Selys Longchamps.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 16.—"A new Method of determining the Number of Micro-organisms in Air." By Prof. Carnelley, D.Sc., and Thos. Wilson, University College, Dundee. Communicated by Sir Henry Roscoe, F.R.S.

This is a modification of Hesse's well-known process. It consists essentially in the substitution of a flat-bottomed conical flask for a Hesse's tube. Its chief advantages are: (1) much smaller cost of flask and fittings as compared with Hesse's tubes; (2) very much fewer breakages during sterilization; (3) great economy in jelly; (4) freedom from leakage during sterilization; (5) results not vitiated by aërial currents.

"Notes on the Number of Micro-organisms in Moorland Air." By the same Authors.

A number of determinations made last August "on the heather" in the north of Forfarshire show that the pure air from the hills and moors far removed from towns was free from Bacteria, but contained on the average 3·5 moulds per 10 litres of air. In winter the number would be still less.

Linnean Society, March 1.—Mr. Carruthers, F.R.S., President, in the chair.—An interesting collection of ferns from the Yosemite Valley was exhibited by Mr. W. Ransom, who also showed some admirable photographs of rare plants, many of them of the natural size.—Mr. J. E. Harting exhibited a coloured drawing, life-size, of a South American game bird (the Rufous Tinamu) which has been successfully introduced into this country at Brightlingsea, Essex, by Mr. J. Bateman, and made some remarks on its affinities, peculiarities of structure, and habits. In a discussion which followed Prof. Mivart, Mr. Christy, and Mr. W. H. Hudson took part, the last-named giving some account of the bird from personal observation of its habits in the Argentine Republic.—The first paper of the evening was then read by Mr. E. G. Baker on a new genus of Cytinaceæ from Madagascar. This curious plant, to which the author has given the name of *Botryocytinus*, grows parasitically on the trunks of a tree of the natural order *Hamamelideæ*. Its nearest ally is *Cytinus*, of which the best known species grows on the roots of the *Cistuses* of the Mediterranean basin. The Madagascar plant is without any stem, and the sessile flowers grow in clusters, surrounded by an involucre. Each cluster is unisexual, and the ovary is unicellular, with about a dozen parietal placentæ and innumerable minute ovules. It was discovered during a recent exploration of the Sakalava country, by the Rev. R. Baron, of the London Missionary Sooiety.—The next paper, by Mr. J. F. Cheeseman (communicated by Sir Joseph Hooker, F.R.S.), was entitled "Notes on the Fauna and Flora of the Kermadec Islands," and as regards the flora might be considered as supplementary to a paper on the flora of these islands, published by Sir Joseph Hooker more than twenty years ago (Journ. Linn. Soc., 1856). These islands, situated about 450 miles north-east of New Zealand, between that country and Fiji, were shown to be of volcanic origin, with a fauna and flora resembling to a great extent those of New Zealand. A few land birds were noted as common to New Zealand; and to the list of plants drawn up by Sir Joseph Hooker, from collections made by Macgillivray, several new species were added by Mr Cheeseman, chiefly ferns. A discussion followed, and in illustration of Mr. Cheeseman's remarks, Mr. J. G. Baker exhibited specimens of a new endemic *Duvalia* closely allied to the well-known *D. canariensis* of the Canary Islands and Madeira.

Geological Society, February 17.—Annual General Meeting.—Prof. J. W. Judd, F.R.S., President, in the chair.—The Secretaries read the Reports of the Council and of the Library and Museum Committee for the year 1887. The President presented the Wollaston Gold Medal to Mr. Henry Benedict Medlicott, F.R.S. He also handed to Dr. Archibald Geikie the balance of the proceeds of the Wollaston Donation Fund for transmission to Mr. John Horne, and the Murchison Medal for transmission to Prof. J. S. Newberry. The balance of the proceeds of the Murchison Geological Fund was handed to Dr. Henry Woodward for transmission to Mr. Edward Wilson; and the President presented the Lyell Medal to Prof. H. Alleyne Nicholson, one moiety of the balance of the proceeds of the Lyell Geological Fund to Mr. Arthur Humphreys Foord, and the second moiety of the balance of the proceeds of the Lyell Geological Fund to Mr. Thomas Roberts. The President then read his Anniversary Address, which we have already printed.—The ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—President: W. T. Blanford, F.R.S. Vice-Presidents: John Evans, F.R.S.; Prof. T. McKenny Hughes; Prof. J. Prestwich, F.R.S.; Henry Woodward, F.R.S. Secretaries: W. H. Hudleston, F.R.S.; J. E. Marr. Foreign Secretary: Sir Warington W. Smyth, F.R.S. Treasurer: Prof. T. Wiltshire. Council: W. T. Blanford, F.R.S.; John Evans, F.R.S.; L. Fletcher; A. Geikie, F.R.S.; Henry Hicks, F.R.S.; Rev. Edwin Hill; W. H. Hudleston, F.R.S.; J. W. Hulke, F.R.S.; Prof. T. McKenny Hughes; Prof. T. Rupert Jones, F.R.S.; Prof. J. W. Judd, F.R.S.; R. Lydekker; Lieut.-Col. C. A. McMahon; J. E. Marr; E. T. Newton; Prof. J. Prestwich, F.R.S.; Prof. H. G. Seeley, F.R.S.; Sir Warington W. Synth, F.R.S.; W. Topley; Rev. G. F. Whidborne; Prof. T. Wiltshire; Rev. H. H. Winwood; Henry Woodward, F.R.S.

February 29.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—An estimate of post-Glacial time, by T. Mellard Reade. The author showed that there exists on the coasts of Lancashire and Cheshire an important series of post-Glacial deposits which he has studied for many years. The whole country to which his notes refer was formerly covered with a mantle of low-level marine boulder-clay and sands, and the valleys of the Dee, Mersey, and Ribble were at one time filled with these Glacial deposits. These Glacial beds have been much denuded, especially in the valleys, where the rivers have cleared them out, in some cases, to the bed rock. Most of this denudation occurred during a period of elevation succeeding the deposition of the low-level boulder-clay. On this eroded surface and in the eroded channels lie a series of post-Glacial beds of a most interesting and extensive nature. They consist of estuarine silt and *Scrobicularia* clay covered by extensive peat-deposits, containing the stools of trees rooted into them. Upon these lie, in some places, recent tidal silts, and on the coast margin blown sand and sand dunes. The series of events represented by the denudation of the low-level boulder-clay and the laying down of these deposits is as follows:—(1) Elevation succeeding the Glacial period, during which time the boulder-clay was deeply denuded in the valleys. (2) Subsidence to about the 25-feet contour, when the estuarine silts and clays were laid down. (3) Re-elevation, representing most probably a continental connection with the British Isles, during which time the climate was milder than at present, and big trees flourished where now they will not grow. (4) Subsidence to the present level, the submersion of the peat and forest-beds, the laying down of tidal silt upon them, and the accumulation of blown sand along the sea-margin extending to a considerable distance in an inland direction. It was estimated, from a variety of considerations, that these events, all posterior to the Glacial period, represent a lapse of time of not less than 57,500 years, allotted as follows: 40,000 years for the elevation succeeding the Glacial period, measured by the denudation of the

boulder-clay in the valleys; 15,000 years for the accumulation of the estuarine silts, clays, peat, and forest beds; and 2500 years for the blown sand. In the discussion which followed the reading of this paper Prof. Prestwich, Mr. De Rance, Dr. Evans, and others took part.—Note on the movement of scree-material, by Charles Davison. Communicated by Prof. T. G. Bonney, F.R.S.—On some additional occurrences of tachylyte, by Grenville A. J. Cole.—Appendix to Mr. A. T. Metcalfe's paper "On Further Discoveries of Vertebrate Remains in the Triassic Strata of the South Coast of Devonshire, between Budleigh Salterton and Sidmouth," by H. J. Carter, F.R.S. Communicated by A. T. Metcalfe.

Mathematical Society, March 8.—Sir J. Cockle, F.R.S., President, in the chair.—The following papers were read:—Supplementary remarks on the theory of distributions, by Capt. P. A. MacMahon, R.A.—Complex multiplication moduli, by Mr. A. G. Greenhill.—Geometrical proof of Feuerbach's nine-point circle theorem, by Prof. Genese.—Isostereans, by Mr. R. Tucker.

Anthropological Institute, February 28.—Francis Galton, F.R.S., President, in the chair.—The election of Mr. Henry C. Collyer was announced.—Dr. Edward B. Tylor read a communication from Mr. Basil Hall Chamberlain, on the Japanese "go-hei," or paper offerings to the Shinto gods. In olden times the offerings were made of cloth, but later on, when Chinese civilization had brought a variety of manufactures in its train, hempen cloth ceased to be regarded as a treasure worthy of divine acceptance, and paper began to be used instead. The "go-heis" used by different sects differ slightly from one another, chiefly in the number of the folds: the Yoshida sect sanctions the use of four folds, while the Shirakawa sect has eight. There is said to be no symbolism attaching to the shape, number of folds in the paper, or the length of the stick; each sect has clung to its traditional practice in these matters. Specimens of "go-heis" were exhibited in illustration of the paper.—Mr. Henry Balfour exhibited a series of decorated arrows from the Solomon Islands, in illustration of his theory of the manner in which the decoration of the shafts was gradually developed.—Dr. Tylor gave a brief account of a paper by Mr. A. W. Howitt, "Further Notes on the Australian Class Systems," and in the course of the discussion the President showed a very simple method of understanding the complicated-looking system of Australian marriages, by supposing a cross-division of the tribes.

EDINBURGH.

Royal Society, February 6.—Sir W. Thomson, President, in the chair.—Prof. Crum Brown showed and described an apparatus for exhibiting the action of the semicircular canals. The apparatus is also capable of application as an instrument for the measurement of the irregularity of angular motion.—Mr. John Murray read a paper on the temperature and currents in the lochs of the west of Scotland, as affected by winds. He showed that when the wind is blowing off shore the warm surface water is blown outwards and cold water takes its place from beneath. When the wind blows on shore the warm surface water is driven inwards. This point is of great importance, as it has an evident bearing on the growth of coral-forming animals.—Mr. Murray also communicated a paper by Mr. W. G. Reid on the solution of carbonate of lime in sea water under pressure. The results of Mr. Reid's experiments show that the solubility is increased by pressure.—Mr. Murray then discussed the distribution of carbonate of lime on the floor and in the waters of the ocean.—Mr. John Aitken read a paper (see NATURE, March 1, p. 428) on the number of dust particles in the atmosphere, giving a full account of the apparatus used and the method of experimenting.

PARIS.

Academy of Sciences, March 5.—M. Janssen in the chair.—Remarks on the first volume of Fourier's works presented to the Academy, by M. G. Darboux. This volume of the complete edition of Fourier's works, now being issued with the aid of the Minister of Public Instruction, contains the full text of the "Théorie analytique de la Chaleur," carefully revised by MM. Darboux and Paul Morin.—On the transformation of the nitrates present in the soil into nitrous organic compounds, by M. Berthelot. The experiments here described have been carried out for the purpose of showing that the nitrates contained in the ground do not occur in an integral state even independently of the formation of the higher plants. On the contrary, they may be changed into nitrous principles of organic nature under the influence of chemical agents properly so-called, or of certain microbes present in the soil. It is suggested that these microbes assimilate the combined nitrogen when presented to them in a convenient form, preferring it to the free nitrogen of the atmosphere, thus reversing the action of the microbes of nitrification. The general inference is that the assimilation of the nitrogen of the nitrates by plants is accompanied, if not preceded, by their transformation into nitrous organic compounds in the earth under the influence of chemical reactions and special microbes. These microbes are perhaps the same as those which fix free atmospheric nitrogen in soil destitute of nitrates. In this way might be formed true azoic compounds derived at once from the oxygenated and hydrogenated compounds of nitrogen.—On perfect numbers, by Prof. Sylvester. A slight omission pointed out by M. Mansion in the author's recent paper on this subject is shown in no way to affect the validity of the demonstration.—On *Saccharomyces ellipsoideus* and its industrial applications to the manufacture of a barley wine, by M. Georges Jacquemin. A process is described by which a tartarized wort of barley is made to yield a true wine of pleasant taste, and more nutritive than grape wine, containing as it does more respiratory aliments, besides an albuminoid substance, and a larger proportion of phosphates calculated to restore the nervous system and the bony tissues. It also differs from white grape wine by being copiously precipitated by tannin, while a portion of the malt may be replaced by crushed grain (wheat or barley) that has not sprouted. This wheat or barley wine is stated to be equal in quality and cheaper than that of pure malt, and the vinous wort in question is an alcoholic fermentation of a totally distinct character from the ordinary yeast of beer.—Immediate solution of equations by means of electricity, by M. Felix Lucas. A method is described by which an algebraic equation of any degree with real numerical coefficients may be directly solved without calculations by means of electricity. The process here explained is much more rapid than the two methods indicated in previous communications. However high the equation, a single operation suffices to obtain all the roots, real or imaginary. "The power of electricity as a calculator is not to be limited."—On the electric conductibility of concentrated nitric acid, by M. E. Bouty. In previous papers it was shown that a very slight addition of alkaline nitrates to the acid increases its conductibility to a considerable extent. Here it is made evident that the addition of water also causes an increase of conductibility nearly proportionate to the quantity of added water. This approximate proportion is maintained even much further than with the nitrates, nearly to $NO_5,4HO$. A table is given showing the degrees of conductibility measured at 0° C., and referred to that of the normal solution of nitric acid at one equivalent per litre, the specific resistance being 4·59 ohms.—On cinchoniline, by MM. E. Jungfleisch and E. Léger. In previous communications the conditions were explained under which cinchoniline is formed and separated in the state of a di-iodhydrate. Here the authors deal with this base and its chief derivatives. That this substance, which has the formula $C_{38}H_{22}N_2O_2$, is isomerous with cinchonine, is made evident not only by the analysis of the base itself, but also of that of a large number of combinations. In ether it forms magnificent rhomboidal prisms, colourless, anhydrous, and often attaining a weight of several grammes. It dissolves readily in ordinary alcohol, but with difficulty in water, its aqueous solution giving a deep blue tint to turnsol (Dutch orchil), and a red to the phthaleine of phenol. Its basic and neutral salts present some remarkable crystallographic properties.—Products of the oxidation of the hydronitrocamphenes, by M. C. Tanret. From the oxidation of these substances the author has obtained several new compounds, which are here described. The new substance, answering to the formula $C_{80}H_{64}N_4O_{16}$, he proposes to call nitrocamphene (*azocamphène*), distinguishing its two modifications as cyanonitrocamphene and leukonitrocamphene. They are isomerous, their analysis yielding the same constituents.—On terpinol, an artificial reproduction of eucalyptol (terpane), by MM. G. Bouchardat and R. Voiry. These researches show that the terpinol of List is formed of a crystallized inactive terpilenol or terpol, $C_{20}H_{18}O_2$, boiling at 218° C.; of terpane, $C_{20}H_{18}O_2$, boiling at 175°, and capable of crystallizing at −1°; lastly of inactive terpilene, $C_{20}H_{16}$. Terpane, which term is here substituted for the older cineol, eucalyptol, cajeputol, spicol, &c., differs also from the active and inactive terpilenols by refusing to

combine either with the acids or the anhydrides to yield ethers. —Deleterious influence of alcohol on offspring, by MM. A. Mairet and Combemale. The results are described of some experiments on dogs, showing that their progeny were injuriously affected for two successive generations by the influence of alcohol administered under various conditions to the parents.

BERLIN.

Physiological Society, February 10.—Prof. du Bois Reymond, President, in the chair.—Dr. Baginski spoke on the origin and course of the auditory nerve. As the result of experiments on young cats, in which the peripheral end of this nerve had been destroyed, and which were then killed at the end of six weeks, he was able to follow the course of the degeneration by means of a series of sections through the root of the nerve to the posterior corpus quadrigeminum. In this way his earlier experiments on rabbits were fully confirmed. The degeneration spread to the olivary body of the same side, and was continued through the trapezium of the pons. After removal of the facial nerve the olivary body was similarly found to be affected. These observations are closely connected with those of von Monakow, who found that destruction of the auditory centre, described by Munk, led to a degeneration which could be traced right into the hinder corpora quadrigemina. At present no observations are in existence as to the course of the anterior root of the auditory nerve.—Dr. Rawitz gave an account of the results of his researches on the eyes of mussels, and explained the same by reference to preparations which he exhibited. Three distinct types may be observed among the eyes, sometimes to the number of one hundred, which occur on the edge of the mantle of Pecten. Each eye consists of an epithelial layer, a lens, and a retina composed of rods, whose ganglionic layer is on the side turned towards the light, while the rods are turned away from the light and abut on the tapetum and layer of pigment cells. The speaker had been able to trace the endings of the nerves through the cells connected with the rods into the rod itself. The mussels are only able to see with the central portions of each eye.—Dr. Virchow presented and explained a plaster cast of the gluteal region. He had had this cast made in order to throw light upon a deep furrow which extends from the gluteus maximus to the tensor muscle, and is not due to the edge of either of these muscles. This furrow may be observed in the sitting posture, and is due to the stretching by the point of the trochanter of a portion of the fascia which envelop the gluteus medius: by this means the gluteus medius is divided into two projecting portions. When this muscle contracts, the furrow disappears.

February 24.—Prof. du Bois-Reymond, President, in the chair. —Prof. Liebreich spoke on the testing of the action of local anæsthetics on animals. There are a number of substances which, when injected subcutaneously, give rise to a localized anæsthesia in the immediate neighbourhood of the place where they are injected. Antipyrin, sal-ammoniac, salts of tannin, resorcin, chloride of iron, and other substances have this action, although there is neither chemical nor physiological similarity between them. They possess, however, this property in common, that they all have a corrosive action on the tissues, when this expression is understood to imply any kind of alteration of molecular structure. The alkaloids, in the cases where they possess a local anæsthetic action, act in the same way, as, for instance, erythrophoein. Cocaine alone is an exception to the rule, inasmuch as it is a local anæsthetic, but does not corrode the tissues. When applied subcutaneously to man, the above substances either produce no localized anæsthesia, or one which is very imperfect. When testing the action of anæsthetics on the eye, it is essential to take into account the difference in sensitiveness of the conjunctiva and cornea, as already pointed out by Claude Bernard.—Dr. Virchow exhibited a plaster cast of the hip-region taken from a female corpse in a hanging position. It brought to light a whole series of most surprising relationships which can never be observed, in preparations made from a corpse in the recumbent position, as at all corresponding to those existing in the erect posture. One of the most striking facts is the considerable stretching of the sciatic nerve, which must be still greater when the leg is advanced, as in walking. —Dr. Virchow further spoke on the striæ medullares acusticæ in man, in connection with the statement made before the Society a fortnight before by Dr. Baginski. His experiments have shown, in correspondence with the results of many other observers, that the striæ can be traced through the raphe to the other side of the medulla. It must still remain an open question whether the fibres which lead to the anterior root of the auditory nerve have a different course in cats and rabbits (examined by Baginski) than they have in man (examined by the speaker), or whether in the above-named animals we have to deal with a frequently-recurring division and rearrangement of the fibres of any one tract.

Physical Society, February 17.—Prof. Helmholtz, President, in the chair.—Prof. Lampe made a report on McGregor's book, "An Elementary Treatise on Kinematics and Dynamics." —Prof. Börnstein exhibited an electricity-meter which enables the intensity of the current to be read off direct.—Dr. Gerstmann gave an account of a preliminary communication by Aubel on the influence of temperature and magnetization on the electrical resistance of bismuth.—Dr. Kötter spoke on a problem in the theory of projectiles—namely, that a bullet shot out of a rifle tends to deviate in a direction away from the side on which the bayonet is attached to the muzzle.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Johnston's Botany Plates (Johnston).—The Testing of Materials of Construction: W. C. Unwin (Longmans).—Leitfaden der Zoologie: Dr. B. Graber (Tempsky).—Science Sketches: D. S. Jordan (McClurg).—Home Experiments in Science: T. O. Sloane (Low).—Memoirs of the Manchester Literary and Philosophical Society, 3rd series, vol. x.; Proceedings of the Manchester Literary and Philosophical Society, vols. xxv. and xxvi. (Manchester).

CONTENTS.

THURSDAY, MARCH 22, 1888.

THE REVENUE METHOD OF ESTIMATING AND CHARGING THE DUTY ON SPIRITS.

THIS subject has attracted some notice since Sir Henry Roscoe put a question to the Chancellor of the Exchequer the other night in the House of Commons, as to whether his attention had been called to the fact that, owing to the present faulty system of charging the duty on spirits, a loss to the Revenue of from £60,000 to £80,000 per annum did not occur, without any corresponding benefit to the trader, and whether he would appoint a Departmental Committee to inquire into an improved system of estimating the percentage of spirit, as proposed by Dr. Derham. To this the Chancellor replied that the above estimated loss was based on an erroneous assumption, and that the introduction of the suggested system would be attended with difficulties comparable with those which would accrue to the substitution of the decimal for the present system of coinage. The grounds of these objections could not of course be given in answer to a question, and therefore the public are not yet in a position to judge how far the Revenue Departments can make them good. The statements of Dr. Derham are, however, perfectly plain, and demand plain answers.

They are (1) That the essential defect of the present system is well known and acknowledged by the Inland Revenue Department.

(2) That this defect depends on the erroneous assumption made by Sikes in constructing his tables that any given quantity of spirit does not alter in bulk or in strength with variations of temperature from the normal of 51° F.

(3) That the only argument which can be advanced in defence of the present system is that deficiencies at temperatures below 51° F. are compensated by over-estimates above 51° F., so that on the whole the Revenue neither gains nor loses.

(4) That on the contrary it is a fact that at least five-sixths of the spirits paying duty are taken out of bond during the nine cooler months of the year when the temperatures in warehouses range from 51° F. downwards, so that in the case of these spirits a constant loss accrues to the Revenue, and also to the trader from under-estimation of his stock, which he must dispose of at that estimate. Whilst in the summer months, owing to the construction of warehouses, evaporation of spirits, &c., the temperature of the spirits does not often exceed 51° F., so that there is but a slight compensating gain to the Revenue from that source.

(5) That the objections to the present plan can be entirely removed by adopting the suggested system, which, without altering the standard of measure—the proof gallon—without introducing any change in the notation of over- and under-proof, or any alteration calculated to confuse the trader, substitutes tables founded on a correct principle instead of an erroneous one, a scientific and exact for an unscientific and inexact instrument, which not only will facilitate the work of the Government officials, but will give correct results at all temperatures.

In consequence of the "*non possumus*" reply of the Chancellor, Sir H. Roscoe gave notice that he should move for a return of the number of proof gallons taken out of duty free warehouses for *consumption* last year. specifying the number of proof gallons at each degree of temperature when the account was taken for payment of duty; all spirits taken out for methylation, exportation, ships' stores, and removal to other warehouses, which do not pay duty, and which may have recently been distilled, being excluded. Such a return would either confirm or disprove Dr. Derham's contention, and there seems no reason why such a return should not readily be obtainable. It appears to us that the comparison of the introduction of the decimal coinage made use of by Mr. Goschen was an unfortunate one. The introduction of the decimal coinage would obviously occasion a great amount of confusion and perplexity, for a time at least, and would in the end only substitute one correct and convenient system for another equally correct though less convenient. The adoption of the improved system of charging duty on spirits would occasion, on the contrary, no material change, none likely to cause confusion or perplexity, but would replace an incorrect and inconvenient system by one at once correct and more convenient. The Chancellor moreover hinted that the vested interests of the trade in the property of the present instruments must be safeguarded. If the new and correct tables became legalized and were adopted, Sikes's hydrometer could equally well continue to be used, or any instrument can be employed which furnishes specific gravities. Mr. Goschen also stated that Dr. Derham's instrument is too delicate for ordinary use. This appears to us to be the weakest part of his argument. The ball in the new instrument, the most vulnerable part of the hydrometer, happens to be of the same size and strength as that in the ordinary Revenue instrument. It is true that a somewhat different method of attaching the poises is adopted, but this is a mere detail, and more a question of taste and opinion than of principle, and it is certain that the form can, if desirable, be made so as to be indistinguishable from Sikes's instrument, for the characteristic feature of the new system is to be found in the bulks and specific gravities of the poises, and not in the shape or size of the stem or bulb.

The Revenue authorities can scarcely, we imagine, fail to admit that certain defects in Sikes's system exist, for these can be demonstrated by reference to Sikes's own tables. Hence we incline to the belief that the erroneous assumption to which the Chancellor referred consists in taking for granted that there is a general correspondence between the temperatures of warehouses and that of the mean shade temperature of the country, for this is the assumption made by Dr. Derham. The returns which have been asked for will decide this point. But meanwhile it may be of interest to see how the large figures of from £60,000 to £80,000 per annum have been obtained. The Customs and Excise deal annually with some forty million gallons of proof spirit or their equivalent, which for the most part lie for a longer or a shorter time in warehouse. If we assume that the average strength of the spirit when removed from warehouse for consumption is 25 overproof, then thirty-two million gallons by measure are equivalent to the forty million gallons upon which the duty is charged, so that, if the rate of removal be

constant, 2,700,000 gallons by measure at 25 overproof are removed each month from bond. Now, taking the mean shade temperatures for the months from November 1886 to April 1887, it appears that, owing to the contraction in these 2,700,000 gallons per month, no less a quantity than 97,268 gallons of 25 overproof or of 121,586 gallons of proof spirit for the six months would accrue in the estimation as now carried on, and the duty on this amounts to £61,000, now lost to the Revenue.

It is true that probably these shade temperatures do not exactly represent the temperatures of the warehouse, which will be more equable, but then the contents of a warehouse if they lose their heat slowly also regain it slowly, and the low temperature contracted during a long and severe winter is perpetuated for a long period throughout the year; so that in all probability the average temperature of the bonded spirit is below 51° not only during six but during nine months of the year, and assuming that the mean temperature did not exceed 47° during these extra three months, the additional loss to the Revenue would amount to £10,000.

The foregoing statements have been published for some time, and have not been confuted except in the usual official Parliamentary style. This, we urge, is insufficient. What the public wants to know, and has a right to learn, is, what, if the Revenue authorities dispute these assertions, are their grounds for so doing? Should this information not be forthcoming, the opinion will gain ground that another Government Department is trying hard "how not to do it."

PRESTWICH'S "GEOLOGY."

Geology: Chemical, Physical, and Stratigraphical. By Joseph Prestwich, M.A., F.R.S., F.G.S., Correspondent of the Institute of France, Professor of Geology in the University of Oxford. In Two Volumes. Vol. II. Stratigraphical and Physical. (Oxford: Clarendon Press, 1888.)

It is just two years ago that we were called upon to notice the first volume of this important treatise; and the author of it has now signalized the completion of his labours at Oxford by giving to the world the second and concluding volume of the work. Its publication has long been eagerly looked forward to, and now that the book is before us, we may safely assert that it more than justifies the high expectations which have been formed concerning it; and we confidently predict that it will add to the already high reputation of the veteran geologist to whom we are indebted for it.

In reading the first chapter of the book, everyone must be struck by the fact that a distinct advance has been made in the mode of treatment of the great problems of stratigraphical geology. Speaking of the "order of succession" and "the breaks in continuity" in the series of stratified rocks, Prof. Prestwich writes:—

"The great time-divisions are of almost universal application; but the smaller 'breaks in continuity,' which are of frequent occurrence in all areas, are subject to constant differences of extent and value; consequently, in filling up the details of the several geographical areas, each one is found to have its own local stamp, and possesses its own special terms, some knowledge of which is as essential to the geologist as is the language of a country to the traveller, if he would pass through it with profit."

The author then proceeds to show how impossible is any universal scheme of geological classification, and to discuss the question, first raised by Edward Forbes and Prof. Huxley, as to how far geological equivalence is to be regarded as being identical with actual synchronism.

He insists that, in distant areas, strata cannot be correlated by identical *species*, but only by the presence of the same characteristic *genera*, and he fully admits the effects of migration of forms of life from one region to another in causing strata of different ages to present very similar faunas or floras. Such considerations as these, the author argues, must always prevent us from regarding the series of formations as being strictly contemporaneous in distant areas, or the breaks between them as being universal ones.

Prof. Prestwich points out some of the difficulties confronting geologists, in the following suggestive passage:—

"In Western North America the great break so conspicuous between the Cretaceous and Tertiary series does not exist, and there are passage beds having characters of the two periods in common. In a similar way the Carboniferous strata in America pass gradually into the Permian, without the unconformity which exists here. In India the Gondwána system forms a consecutive series from the base of the Permian to the top of the Jurassic strata. In New Zealand, again, no marked line can be drawn between the Cretaceous and Tertiary series, the Upper Cretaceous and Lower Eocene forming unbroken and continuous series."

He then proceeds to give not *one* table of classification for the sedimentary rocks, but *six* different ones, adapted respectively to Europe, India, North America, Australia, New Zealand, and South Africa. And having thus at the very outset shown what are the obstacles in the way of the exact correlation of distant deposits, and established a philosophical basis of classification for strata, he takes up the consideration in succession of the several great geological systems; he selects the method of beginning with the oldest, and passing upwards in the scale, candidly admitting, however, that the opposite plan is not without its merits and advantages.

The account given in successive chapters of the several formations, their typical development in this country, the groups of fossils by which they are distinguished, and their chief foreign representatives, is eminently clear and readable. This merit is the more conspicuous from the circumstance that the mass of detailed information to be selected from and arranged in writing a work on stratigraphical geology is so enormous and bewildering, that such works are very apt to suffer in their style, and to become heavy and encyclopædic in character. But Prof. Prestwich has admirably avoided this danger.

The author does not waste any time in discussing barren questions of nomenclature. In the case of the three systems of the older Palæozoic, he follows the common custom of calling the oldest "Cambrian," the second "Lower Silurian," and the third "Upper Silurian"; though pointing out in a footnote the significance of the term "Ordovician."

Very striking features in the book are the chapters in which are summed up the characteristics of the faunas

and floras of the Palæozoic, the Mesozoic, and the Kainozoic divisions respectively. In these reviews of the great geological epochs, the distinctive features of their life-history are ably summarized; and the subject is made clearer by the insertion of sixteen lithographic plates, the fossils represented upon these being very judiciously selected and admirably drawn.

Most readers will look with much interest to the later chapters, in which the author deals with the Tertiary and post-Tertiary deposits, the study of which has been so greatly advanced by the author's own researches. Prof. Prestwich has in several very important points modified some of the conclusions of his classic papers upon these questions. He now accepts, with most modern geologists, the term "Oligocene" as usefully embracing the strata known as the "Fluvio-marine strata" of the Hampshire Basin, and separates them from the Eocene proper. He also points out for the first time the close connection of the so called "Lower Bagshot Sands" with the London Clay, placing them in the Lower Eocene; while the Middle and Upper Bagshots of the London Basin, and the Bracklesham and Barton series of the Hampshire Basin, constitute his Upper Eocene. This view has recently been explained and defended in a paper which the author has read before the Geological Society, and is one which we think will meet with very general acceptance. In his account of the post-Pliocene (or, as he prefers to call them, the Quaternary or Pleistocene) deposits, it will also be seen that Prof. Prestwich has so far departed from his earlier published views as to admit the probability of some of the deposits which contain relics of human handiwork belonging to a period when glacial conditions prevailed in this country.

The work concludes with three chapters of a theoretical character. In the first of these Prof. Prestwich argues against the acceptance of any views, like those of Dr. Croll, which would define the exact date of the Glacial period by reference to astronomical events. Accepting the probability that man may have lived at the period of the greatest glaciation, the author boldly proceeds to challenge the common opinion that this period of glaciation must have been separated by an enormous interval of time from the present day. He even suggests that the Glacial period may not have had a duration of more than from 15,000 to 25,000 years, and the post-Glacial period he thinks may be restricted to 10,000 or 15,000 years!

The facts which seem to have had the greatest weight in leading Prof. Prestwich to these, at first sight, startling conclusions, are those connected with the movements of the great ice-sheets in Greenland. The recent observations of Rink and Helland seem to show that the data afforded by the diminutive Alpine glaciers are utterly inapplicable to the vast masses of ice which must have flowed over extensive areas during the Glacial period. While the Alpine glaciers progress at an average rate of a foot per day, the great Greenland ice-sheet advances 35 feet per day, and the effects produced in a given time by such rapidly moving masses must be proportionately great. There will doubtless be much difference of opinion among geologists upon the important suggestions made by Prof. Prestwich; but in any future discussions of the subject it must be admitted by everyone that the data upon which all our reasoning has to be based has been profoundly modified by the remarkable observations made by the Danish Scientific Commission upon the inland ice of Greenland.

In his penultimate chapter, the author points out the grounds for the view—of which he has long been one of the ablest advocates—that the earth's solid crust is a comparatively thin one; and he indicates the lines of argument by which the objections of mathematicians and physicists to such views can best be met. While demurring to the doctrine of the permanence of continental and oceanic areas, he justly points to the great effects which must result from the flow of ice-cold water over the bottom of the great oceanic depressions.

The last chapter, on "The Primitive State of the Earth," is an attempt to link the geological history on to that arrived at by the studies of the astronomer. Due importance is justly attached to the evidence afforded by meteorites, and an excellent summary is given of Daubrée's admirable researches. But here, as the author freely admits, he is on less secure ground than in the earlier chapters of his book, and the utmost he aims at doing is to supply a working hypothesis.

Both Prof. Prestwich and the Delegates of the Oxford University Press are to be congratulated upon the manner in which the work has been got up. The printing is admirably clear, and the woodcuts, most of which are original, are of exceptional excellence. The plates, which have been very skilfully drawn on stone by Miss Gertrude Woodward, exhibit the characters of the fossils illustrated in a manner superior to what we have ever seen attempted in any geological text-book. The coloured geological map of Europe, which has been prepared under the supervision of Mr. W. Topley, is brought up to date, and is very clear and serviceable.

To the splendid work now so auspiciously completed at the termination of the author's professorial career at Oxford, we heartily wish all the success it so well deserves.

J. W. J.

VACCINATION.

Cow-Pox and Vaccinal Syphilis. By C. Creighton. (London: Cassell, 1887.)

Vaccination Vindicated. By J. C. McVail. (London: Cassell, 1887.)

TWO new books have lately appeared on vaccination; one on the natural history of "Cow-Pox and Vaccinal Syphilis," by Dr. Charles Creighton; the other, "Vaccination Vindicated," by Dr. J. C. McVail.

The first mentioned, that by Dr. Creighton, is a very misleading work. The first four chapters are almost entirely devoted to a wholesale abuse of Jenner, and the fact that Jenner has called the cow-pox the "variolæ vacciniæ," is especially singled out for more than usual criticism; but the very virulence of the abuse will lead to its condemnation, and the memory of the man who deserves so well of his country will not therefore be unjustly thought of by his countrymen.

The whole burden of the rest of the book may be summed up in a passage that occurs on p. 155: "The real affinity of cow-pox is not to the small-pox but to the great-pox." Let it be remembered that these two diseases are placed together by the science of medicine

under the common order of zymotic diseases: is it wonderful that in some things they agree? The thistle and the sunflower both belong to the same natural order in botany, but are they identical? Dr. Creighton entirely suppresses every point in which the dissimilarity of the two diseases appears; but he insists upon nearly all those in which the similarity shows itself. In fact, his whole work is a piece of special pleading which anybody but a lawyer ought to be ashamed of.

It is easy enough to find some sort of resemblance between two inoculable diseases, and arguments of the kind found in Dr. Creighton's book might be multiplied greatly. We present him with another which we should have thought too good for him to have missed, but we will not promise him it will satisfy his readers more than those which he has himself adduced.

When the virus of the great-pox is taken early in the disease it will communicate its own specific characters, and will for a certain time render the individual to whom it is communicated immune from further contamination by the disease; but if the virus is taken in its later stages it will produce in the individual who is inoculated a sore which has often a tendency to ulcerate, to phagedena, &c., and this sore does not convey the constitutional symptoms of syphilis, nor does it render the individual immune from further manifestations of its own peculiarities. This similarity of the two poxes might, in Dr. Creighton's fashion, be shown in the behaviour of vaccinia, for if vaccine lymph is taken early it produces a constitutional disease of short duration which protects the individual from further vaccination for a time at least, and also from small-pox; but if the vaccine is taken late it produces a sore that has a tendency to ulcerate, to phagedena, &c., and which does not convey immunity to the individual against further vaccination or against small-pox. While referring to this point we may observe that all the bad results collected by Dr. Creighton following vaccination were from lymph taken at a too late stage, and the evils produced must be attributable to the ignorance of the vaccinator rather than the innate virulence of the lymph, or more fancifully still to any reversion to type.

Some of the points of dissimilarity he does not mention are these:—

Firstly, vaccination protects from small-pox when its virus is taken at the proper time, but it does not protect us against the great-pox.

Secondly, the microscopical appearances of a true chancre of early date are quite distinct from those of a vaccine vesicle.

Thirdly, the incubatory periods of the two diseases are utterly different.

It is easy to find points of similarity if we only look at the points of likeness. Thus with no great difficulty we might compare a man and a monkey, and it would not be difficult to argue that at a remote period of time they may have had a common ancestor; but now no one in his senses, except perhaps Dr. Creighton himself, would say that they are identical species.

Dr. McVail's book is altogether different. It is one of the most thoughtful works of the kind we have seen. We would commend it to everyone who is anxious to learn the truth about vaccination, especially to Members of Parliament and others who have a voice in the government of this great country, also to all medical men who neither have the time or inclination to dig deep into anti-vaccination literature. They will find complete answers to all the assumed evils of vaccination, as well as the evidences of the value of vaccination as a prophylactic against small-pox. There is also set forth what a fearful disease small-pox used to be, and how it has been robbed of its sting by vaccination.

Dr. Wallace's writing on this subject, whom we are ashamed as scientific men to find in the anti-vaccination ranks, are especially shown up and gibbeted. We may quote two passages in support of this assertion; they occur on pp. 70 and 87. Speaking of the possible errors in registration, Dr. Wallace instances three cases; of one of these Dr. McVail writes:—

"It is pitiful to think of Dr. Wallace as being driven to appeal for one of his three instances of incorrect registration to such ravings as those of Mr. Pickering. But the abuse of vaccination is so largely buttressed by the relation of 'cases,' that the examination of these samples specially selected by so able an author seems not without use as illustrating the character of the whole class."

And on p. 87 Dr. McVail says after careful reading: "I have been further forced to the conclusion that, in this matter, when Dr. Wallace says 'the point in question has been entirely overlooked,' *the statement is a misstatement*, and that when he says 'it is nevertheless a fact,' then *it is not a fact*." The book is throughout so carefully and faithfully written, and deals so well with the tactics of the anti-vaccinators, that it ought to have a wide circulation among those interested in the question. Many are interested in seeing conjuring tricks, and in witnessing optical delusions. So long as the tricks are not understood, there is an inclination in the minds of some to regard these tricks as more difficult of performance than they really are, and some may even attribute them to supernatural agency. The same kind of tricks are played by such men as Dr. Wallace on our literary pursuits. Dr. McVail's book may be taken as exposing the tricks, and showing the mechanism by which they are done.

ROBT. CORY.

OUR BOOK SHELF.

Animal Biology. An Elementary Text-book. By C. Lloyd Morgan. With Illustrations. (London: Rivingtons, 1887.)

THIS volume has been written to meet the requirements of those reading for the London Intermediate and Preliminary Scientific Examinations, as well as for the Oxford and Cambridge Local. In it special attention has been paid to embryology, and there can be little doubt that the information in this volume would enable the attentive student, with some preliminary assistance, to make very considerable progress in the study of both anatomy and physiology.

The author treats of the anatomy and physiology of the vertebrates as exemplified by the frog, the pigeon or fowl, and the rabbit, with occasional references to other types; and of the invertebrate types, the crayfish, cockroach, earthworm, snail, fresh-water mussel, liver-fluke, tapeworm, hydra, vorticella, and amœba, are selected.

The illustrations have been engraved after original outline sketches of the author's, chiefly from dissections or preparations made in the biological laboratory of University College, Bristol. They are all the better for

not being too pictorial; for a student, especially when left to his own resources, is often apt to be misled by over elaborated drawings.

In addition to the anatomical and physiological details given of each of the type forms selected, there is appended to each an excellent general summary of the life-history of the form; so that within the compass of a little over 350 pages we have a really valuable text-book of animal biology, which we would wish to place in the hands of all students. In Ireland, unfortunately, the Commissioners of Intermediate Education have omitted the subject of biology from the schedule for boys, and limited that for girls to the vegetable kingdom.

Practical Guide to Photographic and Photo-Mechanical Printing Processes. By W. K. Burton. (London: Marion and Co., 1887.)

IT is refreshing to find that the text of the second photographic work issued by these publishers is not made subservient to the advertisement of photographic specialties. The work before us is written by a gentleman well known for his practical rather than his theoretical acquaintance with photography. We thus have an account of the practical working of various processes, with a small modicum of theory. The chapters on silver printing and carbon printing are very clear and complete, and if followed out will lead the amateur to successful results. When we come to the photo-mechanical processes, however, there is at first sight presumable evidence of a lack of intimate knowledge of the subject. It may be, however, that there is a greater difficulty in describing these operations than in the ordinary printing processes to which we have alluded. We doubt very much if the descriptions given would enable a tyro to progress at a rapid rate. For the enthusiastic photographer who has time to experiment the directions would suffice to enable him to commence in the right way, and though at first he would inevitably blunder, yet he would after a sufficient number of disasters produce results which he might take a certain amount of pride in showing to his immediate friends, who would be likely to appraise them higher than at their market price.

In another edition we should recommend that the author should either expand the descriptions of his photo-mechanical processes, or omit them altogether. The work itself is nicely got up, the print is good, and the illustrations well executed.

A Treatise on the Diseases of the Dog. By John Henry Steel, M.R.C.V.S. (London: Longmans, Green, and Co., 1888.)

THOUGH the author of this manual does not claim to offer an original book on canine pathology, and though he assumes the modest *rôle* of compilator of canine literature—English and foreign—we venture to say that he is fully entitled to the claim of having produced an extremely useful work; useful in the first place to the veterinary profession, but not less useful to all those who, like sportsmen, dog-breeders, and dog-keepers, wish to possess a ready and authoritative book for study and reference.

All disorders to which the dog is subject are considered minutely, and in addition there are a great many useful data as to the anatomy and physiology of the canine organism well blended together.

The treatment of canine ailments, and the various methods of medical and surgical practice, form an integral part, and while the author's extensive practice enables him to speak with authority, he does not omit to mention the practice of others which he considers most commendable.

The numerous illustrations, copied from standard books, though not of the first order as regards execution and reproduction, nevertheless considerably enhance the text; this is particularly the case with those which illustrate the general appearance of the animal under the various severe internal disorders, as also those on medical and surgical practice.

But it must be regretted that in the illustrations on microscopic objects, of which there are a good many in this book, no statement is made in connection with the figures as to the amount of amplification under which the objects are supposed to be viewed. This is perplexing in itself, but becomes more so when we remember that there are other illustrations of anatomical parts which are represented smaller than natural size. But these minor details, which are easily corrected, cannot detract from the general usefulness of the work. E. KLEIN.

Management of Accumulators. By Sir D. Salomons. Third Edition. (London: Whittaker, 1888.)

THE author has considerably enlarged this edition of his work, and made it in some respects more complete.

The first part deals with accumulators, and principally with those of E.P.S., or Elwell-Parker type. The construction and principle of working of the cells is described, and hints are given as to the best method of setting them up and charging them. The ordinary causes of failure and the methods of guarding against them are discussed.

In the second part the arrangements of an installation for house-lighting are fully described, and hints, founded on the author's experience in lighting his own country-house for some years past, are given as to the management of engines, boilers, dynamos, lamps, switches, &c., as well as descriptions of the methods which he has adopted for so regulating the whole system by automatic appliances, that, as he says, "it is only needful to start and stop the engine, so that a man having no knowledge of electricity may be employed." He gives estimates for the capital expenditure and working expenses of installations of from 25 to 120 sixteen-candle power lamps. From these we learn that one of the latter size can be erected for £6 per lamp without accumulators, which latter add £3 per lamp to the cost, and the automatic regulating appliances bring up the cost to £10 per lamp. For fifty lamps the cost per lamp is about 50 per cent. greater, and for twenty-five lamps about twice as great. The annual cost, including interest and sinking fund, without accumulators, ranges from £2 16*s.* per lamp for 120 lamps to £4 4*s.* per lamp for twenty-five lamps, these figures being increased to £3 9*s.* and £6 respectively when accumulators and automatic regulators are used.

As was the case in the previous edition, there is much useful information in this book, but it is very badly written, so badly that the descriptions and explanations are often unintelligible. As an example we may quote from the chapter on the "Action of Cells with Dynamo" (p. 111). In discussing the relation between E.M.F. and current in machines of different types, he says, "Let us confine ourselves to the shunt dynamo, this has a falling curve, *i.e.* the E.M.F. falls as the current in the circuit is increased, due to two reasons, one is *the armature absorbs more power as the current is increased*" (the italics are ours); "and secondly the lowering of the outside resistance, to obtain an increased current, is in shunt with a fixed high resistance, viz. the shunt winding on the field-magnets, so that when the outside resistance is lowered to zero by short-circuiting the terminals, practically no E.M.F. exists, and no current passes."

Elementary Physiography. By J. Thornton, M.A. (London: Longmans, Green, and Co., 1888.)

THIS is an admirable introduction to the study of Nature by one whose experience in teaching must of necessity have indicated to him the requirements of beginners. The subjects are arranged according to the syllabus of the elementary stage of physiography, which will greatly extend the sphere of usefulness of the book. The treat-

ment is very detailed for an elementary book, but there is nothing beyond the capacity of those for whom it is intended. The author is of opinion—and we quite agree with him—that meagre accounts lead to inaccurate ideas, inasmuch as they are not of sufficiently general application. As far as desirable, and in accordance with the syllabus, simple experiments have been introduced. The main results of the *Challenger* Expedition are also explained, and illustrated by diagrams.

The astronomical portion leaves nothing to be desired. In addition to 150 excellent diagrams, there are ten maps, illustrating the distribution of temperature and pressure, volcanoes and earthquakes, &c. The diagram of the geological formations shows the general physical appearance of the strata, along with the characteristic fossils of each.

The book is beautifully printed, and is sure to win the favour of all who use it, whether as students or teachers.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Dr. Whewell on the Origin of Species.

IN his essay on the "Reception of the 'Origin of Species,'" Prof. Huxley writes :—

"It is interesting to observe that the possibility of a fifth alternative, in addition to the four he has stated, has not dawned upon Dr. Whewell's mind" ("Life and Letters of Charles Darwin," vol. ii. p. 195).

And again, in the article "Science," supplied to "The Reign of Queen Victoria," he says :—

"Whewell had not the slightest suspicion of Darwin's main theorem, even as a logical possibility" (p. 365).

Now, although it is true that no indication of such a "logical possibility" is to be met with in the "History of the Inductive Sciences," there are several passages in the Bridgewater Treatise which show a glimmering idea of such a possibility. Of these the following are, perhaps, worth quoting. Speaking of the adaptation of the period of flowering to the length of a year, he says :—

"Now, such an adjustment must surely be accepted as a proof of design, exercised in the formation of the world. Why should the solar year be so long and no longer? or, this being such a length, why should the vegetable cycle be exactly of the same length? Can this be chance? . . . And, if not by chance, how otherwise could such a coincidence occur than by an intentional adjustment of these two things to one another; by a selection of such an organization in plants as would fit them to the earth on which they were to grow; by an adaptation of construction to conditions; of the scale of construction to the scale of conditions? It cannot be accepted as an explanation of this fact in the economy of plants, that it is necessary to their existence; that no plants could possibly have subsisted, and come down to us, except those which were thus suited to their place on the earth. This is true; but it does not at all remove the necessity of recurring to design as the origin of the construction by which the existence and continuance of plants is made possible. A watch could not go unless there were the most exact adjustment in the forms and positions of its wheels; yet no one would accept it as an explanation of the origin of such forms and positions, that the watch would not go if these were other than they were. If the objector were to suppose that plants were originally fitted to years of various lengths, and that such only have survived to the present time as had a cycle of a length equal to our present year, or one which could be accommodated to it, we should reply that the assumption is too gratuitous and extravagant to require much consideration."

Again, with regard to "the diurnal period," he adds :—

"Any supposition that the astronomical cycle has occasioned the physiological one, that the structure of plants has been brought to be what it is by the action of external causes, or that such plants as could not accommodate themselves to the existing day have perished, would be not only an arbitrary and baseless assumption, but, moreover, useless for the purposes of explanation which it professes, as we have noticed of a similar supposition with respect to the annual cycle."

Of course, these passages in no way make against Mr. Huxley's allusions to Dr. Whewell's writings in proof that, until the publication of the "Origin of Species," the "main theorem" of this work had not dawned on any other mind, save that of Mr. Wallace. But these passages show, even more emphatically than total silence with regard to the principle of survival could have done, the real distance which at that time separated the minds of thinking men from all that was wrapped up in this principle. For they show that Dr. Whewell, even after he had obtained a glimpse of the principle "as a logical possibility," only saw in it an "arbitrary and baseless assumption." Moreover, the passages show a remarkable juxtaposition of the very terms in which the theory of natural selection was afterwards formulated. Indeed, if we strike out the one word "intentional" (which conveys the preconceived idea of the writer, and thus prevented him from doing justice to any naturalistic view), all the following parts of the above quotations might be supposed to have been written by any Darwinian. "If not by chance, how otherwise could such a coincidence occur, than by an adjustment of these two things to one another; by a selection of such an organization in plants as would fit them to the earth on which they were to grow; by an adaptation of construction to conditions; of the scale of construction to the scale of conditions?" Yet he immediately goes on to say: "If the objector were to suppose that plants were originally *fitted* to years of various lengths, and that such *only* have *survived* to the present time . . . as could be *accommodated* to it (*i.e.* the actual cycle), we should reply that the assumption is too gratuitous and extravagant to require much consideration." Was there ever a more curious exhibition of failure to perceive the importance of a "logical possibility"? and this at the very time when another mind was bestowing twenty years of labour on its "consideration." GEORGE J. ROMANES.

The Fog Bow.

THE complete theory of the rainbow, as developed by Sir George Airy (Camb. Phil. Trans., vi. p. 379, 1836), besides explaining the supernumerary bows, shows that the main bow has a radius somewhat smaller than that calculated on the ordinary geometrical theory. The smaller the drops the greater is the discrepancy. With the tiny drops composing a fog, the discrepancy is so marked that the bow receives a new name—the fog-bow, or "*arc-en-ciel blanc.*" Mr. Mohn's (NATURE, February 23, p. 391) nearly simultaneous measurements of the fog-bow and Ulloa's rings afford a capital opportunity of putting the theory to the test, for from the latter phenomenon we can readily calculate the average size of the particles.

Not having Airy's paper within reach, I have had to be content with the incomplete account given by Verdet ("Leçons d'Optique Physique," tom. i. p. 414). Assuming $\mu = 1{\cdot}333$, I find for the angular discrepancy—

$$\beta = 0{\cdot}467\, m\left(\frac{\lambda}{a}\right)^{\frac{2}{3}},$$

where λ is the wave-length, a the radius of the drop, and m is determined by the condition that the integral—

$$\int_0^\infty \cos \frac{\pi}{2}(w^3 - mw)dw$$

should be a maximum. This integral was calculated by Airy for a series of values of m, but Verdet does not quote the results. Some rough approximations lead me to the conclusion that m lies between $1{\cdot}0$ and $1{\cdot}3$, and very much nearer the latter.

For the radius of the first Ulloa's ring we have

$$\alpha = 0{\cdot}82\lambda/a.$$

Mr. Mohn measured this radius as 1° 31′. Using this value, and taking m as $1{\cdot}25$, I find β is the circular measure of 3° 24′. The geometrical theory gives the radius of the rainbow 42° 2′. So in this particular case the fog bow should have had the radius 38° 38′. Mr. Mohn gives two measurements, taken

shortly before that of the Ulloa's ring, 38° 48′ ± 48′, and 38° 28′ ± 22′ Thus the agreement between theory and observation is singularly perfect. JAMES C. MCCONNEL.

St. Moritz, Switzerland.

"The Teaching of Elementary Chemistry."

IN reply to Prof. M. M. P. Muir's letter, I wish to say that, judging from his answer, Prof. Muir does not seem to consider it necessary in books of which he is senior author to secure that accuracy of which, from his criticisms of the writings of others, one would expect to find him the champion.

The first extract from the books mentioned sounds curiously to chemists. I consider the statement misleading inasmuch as it appears to convey an idea as to the constitution of caustic soda which is not that generally entertained by chemists; that this is not the intention of the authors, however, is manifest from p. 247 of the "Elementary Chemistry," where the usual view is stated.

It is utterly untrue and misleading to state that, "inasmuch as the result of passing chlorine over yellow mercuric oxide dried at about 100° is to evolve oxygen without forming chlorine monoxide, ... it may still be justly said that in making chlorine monoxide 'we carry out a reaction in which oxygen is produced in presence of chlorine.'"

The facts are briefly these:—

(*a*) When chlorine gas is passed at ordinary temperature over yellow mercuric oxide, which has been previously heated to 300°–400°, chlorine monoxide is obtained.

(*b*) When a large quantity of chlorine gas at ordinary temperature comes rapidly into contact with yellow mercuric oxide which has been previously dried at ordinary temperature, a violent reaction, accompanied with evolution of light and heat, ensues, and nearly pure oxygen is the only gaseous product. If both the chlorine and the mercuric oxide be kept cool by means of a freezing mixture, chlorine monoxide is the only gaseous product obtained. With intermediate conditions of temperature, &c., mixtures in varying proportions of oxygen and chlorine monoxide are obtained. (Pelouze, *Annalen der Chem. und Pharm.* Bd. xlvi. 196.)

The formation of oxygen in the second case must therefore be due to the decomposition of already formed chlorine monoxide, or to the occurrence of a reaction the conditions of which render the existence of part of the chlorine monoxide impossible. I think the majority of chemists will agree with me that the appearance of oxygen under conditions which insure the non-existence of (or as itself a product of the decomposition of) chlorine monoxide, can scarcely be admitted as in any measure explaining the *formation* of the latter.

I do not consider it a "verbal quibble" to object to the use of the term "volatilized" as applied to the mechanical removal of particles of a solid substance.

As to the chemical properties of chlorine, bromine, and iodine, I should indeed be open to the gravest charges of non-acquaintance with chemical classification, had I suggested anything so idiotic as that, say, potassium hypobromite and potassium hypoiodite (if the latter exists) could be *identical.*

I called the passages I quoted misleading, because some of them at least were inaccurate. What amount of inaccuracy is required to make a statement misleading may be a matter for difference of opinion. Apparently it is so.

Prof. Muir states that he will decline to take any notice of my anonymous communications. This, at least, is safe ground; but I can wait for the second editions of the two books, and see if the inaccuracies are eliminated. In the second edition of "Elementary Chemistry" I hope Messrs. Muir and Slater will also describe the methods (omitted on p. 19) for removing air from oxygen. Whilst these methods remain unpublished, I prefer to remain Z.

"Kinematics and Dynamics."

MAY I ask a short space in your columns to refer to a few points in Prof. Greenhill's review of my book on "Kinematics and Dynamics," published in your issue of February 16 (p. 361). I shall be as brief as possible.

(1) "In questions involving the size of the earth (pp. 74 and 80), it is the circumference and not the diameter which should be given in metres, the circumference being 40,000,000 metres," the reason being, I suppose, that in illustrative problems round numbers should be employed as data, with the object of facilitating arithmetical calculation. There are doubtless advantages in this course, and in many problems I have adopted it. But should it be made an invariable rule? Problems based on exact data, such as the ones referred to, on pp. 74 and 80, have for many students a greater interest than those based on approximations.

(2) "The expression 'knots an hour' (p. 60) is irritating to a sailor." But the expression "knots" simply would be either misleading or puzzling to a student unacquainted with nautical abbreviations.

(3) "The formula $\frac{1}{2}v^2 = \frac{1}{2}v_0^2 + as$ is to be preferred to that on p. 34, $v^2 = v_0^2 + 2as$; in all cases the factor $\frac{1}{2}$ should go with the v^2 in the equation of energy." The formula quoted is not an equation of energy, but a kinematical equation. Equations of energy (see pp. 253, 256, 328) have in all cases the form approved by Prof. Greenhill.

(4) "In dealing with rotation, the author would do well to study Maxwell's geometrical representation of the direction by means of the screw, right-handed or left-handed." I have done so; but I find that students more readily grasp a specification of the direction of a rotation when it is made by reference to the face of a clock; probably because few of them are so familiar with right-handed and left-handed screws as they are with clock-faces.

(5) "In a linear strain the increment of distance of two points in the line of the strain is properly their *elongation;* while the ratio of the elongation to the original distance is called the *extension*, not the *elongation*, as on p. 167." And yet Thomson and Tait ("Elements of Natural Philosophy," § 139), Clifford ("Elements of Dynamic," p. 158), Minchin ("Uniplanar Kinematics of Solids and Fluids," § 78), and Ibbetson ("Mathematical Theory of Elasticity," § 53), all define elongation exactly as I have done.

(6) "The author, disregarding the vernacular use of the word 'weight,' defines the weight of a body as the force with which it is attracted by the earth" [I don't (see § 290); but let that pass], "but is at variance with his own definition in the statement of the majority of the subsequent examples, relapsing into the language of ordinary life." No references are given to these instances of backsliding. I have looked pretty carefully through the subsequent examples, and can find no case in which I have used the term referred to in any other sense than that given it by definition. I should be glad to have such slips pointed out to me, if there are any.

(7) "A collection of 500 different ways of spelling the name of the town of Birmingham has been made, and a similar collection could be made from the present treatise of different ways of expressing the simple ideas of the pound *weight* and the pound *force.*" It is true that these ideas are expressed by English writers in various ways. And it seems to me desirable that a student should be made acquainted with them. Surely in holding that I should choose one phrase and stick to it, your reviewer is blaming me for not being one of the "mathematical precisionists" at whom he sneers.

(8) "This terminology culminates in the solecisms that on p. 477 we must suppose pressure to be measured in poundals on the square foot in hydrostatical problems; and that if the equation $w = mg$ is supposed to be used with absolute units, the weight of a body is measured in poundals; as if a mathematician asked in a shop for 'half a poundal of tea, or tobacco.'" It is not quite correct to say that, in the hydrostatical equations referred to, pressure must be supposed to be measured in poundals per square foot. In fact it may be supposed to be measured in terms of the unit of pressure of any derived system, as, *e.g.*, the dyne per square centimetre, or even the pound-weight per square foot, provided only the density be measured in terms of the corresponding unit. I am aware that this mode of expressing hydrostatical equations is unusual, but it seems to me to have great advantages, and it was adopted both for this reason and for the sake of making the section on hydrostatics uniform with the rest of the book. With regard to the units in which weight should be measured, the practice of the tobacconist or the tea merchant is surely not our best guide.

(9) "Thus a mathematical precisionist, to express the simple idea of a force of 10 pounds, to be consistent should call it 'a force equal to the weight of the mass of 10 pound weights,' the absurdity of which is evident." The phrase inclosed in quotation marks is not quoted from my book. In my terminology the most precise of mathematicians would express the idea referred to

by the phrase "a force equal to the weight of 10 pounds," which is neither clumsy nor absurd.

(10) "Except for the parts criticized above, on the units of weight, mass, and force, the present treatise shows that the author has read with profit and discrimination the most recent treatises on dynamics." I have been under the impression that in my treatment of these units I had, in the main, followed the most recent treatises on dynamics. May I ask in which of them units are treated in what Prof. Greenhill considers the proper way?

I would like to say also that the elementary proofs of the chief properties of the common catenary, which are given by me, are, with slight modifications, those given in Prof. Goodeve's "Principles of Mechanics." My indebtedness to his book is acknowledged generally in the preface.

I fear my desire to be brief may have made me appear curt. Let me express, therefore, my appreciation of the trouble Prof. Greenhill has taken to form a just estimate of the merits of my book, and of the kindly way in which he has spoken of it.

J. G. MacGregor.

Dalhousie College, Halifax, N.S., March 1.

Coral Formations.

I am glad to see the theory that the internal lagoons of coral atolls are excavated by the chemical action of sea-water and the removal of carbonate of lime in solution is now being brought to the test of figures.

Mr. J. G. Ross (Nature, March 15, p. 462) calculates from his experiments that in this way a sheet of carbonate of calcium half an inch thick can be removed annually from the surface of a lagoon, but strangely adds, "In other words at the same rate it would require about a century to deepen the lagoon one fathom." According to this method of calculating, 144 years is "about a century."

These figures no doubt suit the theory of the formation of coral lagoons very well, but they appear to me quite destructive of the other and co-relative view that the platforms upon which atolls have been formed have been built up by the accretion of the dead shells of pelagic organisms showered down from the surface of the ocean together with the shells of those organisms which have lived on the bottom. I believe that at no place on the surface of the globe are such dead shells being supplied at a rate that would even balance this supposed rate of chemical destruction.

Yet if these figures be correct we shall have to reckon upon the removal from such platforms of more than half an inch annually in consequence of the quicker action which it is said takes place through greater pressure at greater depths.

If, therefore, we accept the dissolution theory of the origin of coral lagoons, it seems impossible to believe in the building up of platforms of calcium carbonate on volcanic or other peaks from varying and unknown depths to the levels necessary for the growth of reef corals. If, on the other hand, we believe that platforms are so built up, it appears equally destructive of the dissolution theory of the lagoons.

Dr. Darwin indicated this difficulty in his letter to me, published in Nature, November 17, 1887, p. 54, but the figures we are now supplied with enable us to realize it much more vividly.

T. Mellard Reade.

Park Corner, Blundellsands, March 16.

The Movements of Scree-Material.

I perused with interest the abstract of a paper on the above, read by Mr. Davison at the meeting of the Geological Society on the 29th ult.

The phenomenon seems somewhat akin to the movements in the "Stone Rivers" of the Falkland Islands, though another reason has been suggested by Sir Wyville Thomson as the cause of their progress.

Might it not be possible for motion to be produced in loose materials, and in the molecules of certain coherent substances situated at a high angle of slope, by continual though imperceptible vibrations in the earth's crust?

Apart from the changes wrought by alternating temperature, might not the "downward creep" in the lead on the roof of Bristol Cathedral—as observed by Canon Moseley—be due to a "settling down" of the molecules by the constant vibrations of sounds transmitted through the structure, and having their origin within and without?

Cecil Carus-Wilson.

Bournemouth, March 15.

Were the Elephant and Mastodon contemporary in Europe?

Mr. Howorth asks this question in Nature for March 15 (p. 463). Perhaps this extract from a translation of a note from Prof. d'Ancona, of Florence, will satisfy Mr. Howorth: "The soil of the upper Val d'Arno is ascribed to formations of the Pliocene period." In it have been found "*Mastodon avernensis, Elephas meridionalis.*" Twenty-four other animal remains are identified, all differing from the remains of the bone-caves. In both places respectively these relics belong to contemporary animals.

9 Sinclair Road, W., March 15.

H. P. Malet.

EXPERIMENTS IN MOUNTAIN BUILDING.[1]

The primary object of these experiments was to explain on what mechanical principles the remarkable rock-structures recently discovered by the Geological Survey in the North-West Highlands might have been produced. In experimenting on the behaviour of strata when subjected to horizontal pressure, it has been usual to regard large rock-masses as practically plastic bodies, and to imitate in the laboratory the great flexures and plications of Nature by compressing layers of clay, cloth, and other plastic or flexible substances. It was, however, evident, as soon as the true structure of the North-West Highland area was unravelled, that the rocks had, to a very large extent, behaved like rigid bodies under the enormous lateral pressure to which they had once been subjected. Instead of following the usual method of using plastic materials, the author therefore set to work to devise strata sufficiently rigid to snap rather than bend and become folded on the application of lateral pressure. It is to this peculiarity in the character of the materials, rather than to any great novelty in the methods, that the interesting results obtained are mainly due.

The experiments were of three distinct kinds. The first series was designed to explain the behaviour of strata when thrust horizontally over an immovable surface, and thus to throw light on the phenomena of "thrust planes," such as are now known to occur abundantly in the North-West Highlands between Loch Eriboll and Skye (see Nature, vol. xxxi. p. 33). To simulate natural strata, layers of damp sand, foundry loam, or in a few cases clay, with laminæ of dry stucco powder between, were employed. In a few minutes the anhydrous powder absorbed enough moisture from the damp beds to enable it to "set" into tolerably rigid sheets. The rock which had thus solidified *in situ*, was next compressed horizontally, by pushing in, by hand, or with the help of a screw, the movable end of the long box in which the strata were formed. One side of the box could be removed at pleasure, and at the end of each experiment it was lifted off, and the section inside revealed, so that it could be photographed or copied if desired.

Fig. 1, which is drawn to a scale of $\frac{1}{12}$ of the original, shows the character of the section produced after the end had been pressed in 20 inches. The central light-coloured band, bounded by stiff stucco laminæ, has undergone no folding, but has become heaped up by means of a series of slightly inclined reversed faults, along which the constant pressure from the right found relief. For this structure the author has proposed the name "wedge structure," as the advancing mass is really raised by being forced over a series of wedges of undisturbed rock.

After pushing the piled-up mass a certain distance

[1] Abstract of a Paper by Henry M. Cadell, B.Sc., F.R.S.E., H.M. Geological Survey of Scotland, read before the Royal Society of Edinburgh, February 20, 1888.

forward, the whole heap always showed a tendency to rise and ride forward *en masse* over the less disturbed beds in front. Fig. 2 shows a typical section produced at this more advanced stage of the movement. This new plane of shear may be called a "major thrust," as distinguished from the "minor thrusts" shown in Fig. ˜, and in the upper part of this figure. The structure of these artificial rock-masses bears a remarkable resemblance to that of the great thrust areas of Sutherland

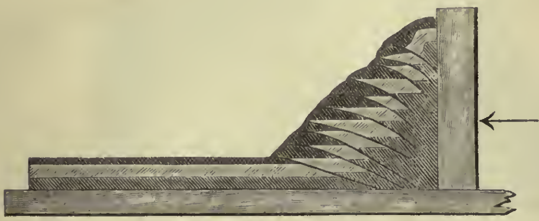

FIG. 1.

and Ross. Everywhere along that great region of earth movement major thrust planes are found truncating sets of minor thrusts, just as has taken place in this experiment. The extraordinary heaping up and local thickening of Silurian strata, and the superposition across their upturned edges of 'tuge slices of Archæan gneiss and Cambrian sandstone, are phenomena which, before the thrust-plane theory had been originated, were quite inexplicable.[1]

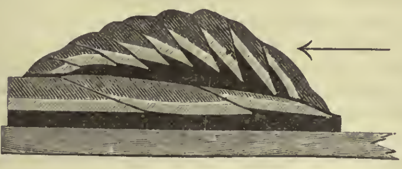

FIG. 2.

The second series of experiments was intended to ascertain how such great thrusts might have originated, and to trace their connection with folds and great terrestrial movements of upheaval and mountain building.

Stratified beds, similar to those employed before, were formed on a band of stout wax-cloth, about 2½ feet long, and 7 inches broad, secured at the ends to vertical blocks of wood. When pressure was applied to the ends, the wax-cloth was thrown into folds, but the folds did not

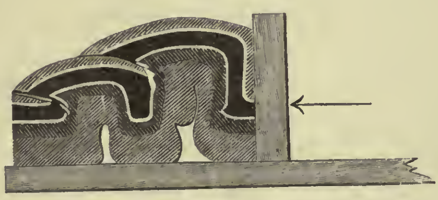

FIG. 3.

in all cases reach the surface, but found relief in thrusts, as shown in Fig. 3.

In this experiment an anticline was first formed at the end of the wax-cloth nearest the pressure. A thrust appeared at the surface, and, on examining the section, this was found to bend down and bury itself in the left monoclinal member of the fold. A second anticline was next started in advance of the first, and, on continuing the push, a second thrust, similarly situated with regard to the underlying fold, was produced. By this means it may be possible to explain how thrusts are connected with movements of deep-seated parts of the earth's crust, and also how, as in the Highlands, they occur over broad areas all inclined in the same general direction. If this section affords the true explanation of their origin, it is clear that thrusting is only a surface phenomenon, and that the complex structures of the North-West Highlands are structures which can only originate at the outer edge of a great mountain-system of elevation.

Fig. 4 represents a section produced with the same apparatus, but here the pressure was applied from both sides. An anticline was started at the centre of the

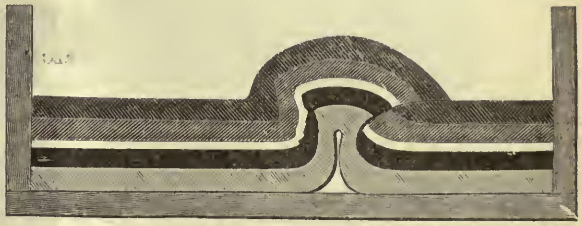

FIG. 4.

wax-cloth, and as the pressure was continued the strata were squeezed into a form closely resembling that known as "fan structure." Two small arches were next formed, one on each side of the original fold, and the pressure was continued. A second fan made its appearance outside the first, and at each side there was a tendency for thrusts to be produced, as shown in Fig. 5. Throughout the experiment the lowest stratum of damp sand next the wax-cloth was compressed and distorted, till, at the last stage of the movement, it became very much "staved together" above the synclinal folds of the wax-cloth on either side, and was completely "nipped out" at the crown of the central fold. During the movement in the mass it was, in fact, made to flow like a viscous body, along a series of approximately vertical planes, which in

FIG. 5.

Nature would be described as planes of foliation. This experiment, then, may help to explain not only the origin of the fan structure of the Alps, &c., but also the common occurrence in the centre of the fan of a core of crystalline rock with vertical foliation.

The experiments of the third series were modifications of those of Prof. A. Favre, of Geneva (see NATURE, vol. xix. p. 103), who covered a band of stretched caoutchouc with beds of adhesive clay, and on allowing the elastic sole to contract, observed the wrinkling up of the surface of the clay into a series of miniature Alpine ridges. The author modified Favre's experiments by separating the upper and lower portions of the clay with sheets of paper, so that the former could be stripped off at the end of the experiment without disturbing the lower part of

[1] The effect of major and minor thrusts is well seen in the section of the Durness and Eriboll district above the map in the second edition of Dr. A. Geikie's "Scenery of Scotland."

the section. After removing the superficial folded layer, the paper covering the lower bed was found to be covered with minute corrugations like those often seen on beds of mica-schist. On stripping off the paper, and again stretching the elastic substratum, the clay adhering to it did not become smoothed down to its original form, but split along a multitude of vertical rents, transverse to the direction of pressure, each of which corresponded to one

Section at head of Loch Eriboll.

of the little ripples on the paper before it was removed. The sides of the cracks were observed to be covered with minute vertical striations like the slickensides of a fault-fissure.

This experiment, the author suggests, may explain the vertical cleavage and foliation found in the deep-seated parts of many old mountain-systems.

SWISS FOREST LAWS.

THE Report of Mr. Conway Thornton to the Foreign Office, on the Swiss Forest Laws, is a careful and interesting piece of work. He divides his subject into two parts: in the first he treats of the history of forestry prior to 1875, the year in which the Act now in force, the Forestry Act of 1875, was proposed; and in the second part he deals with that Act, its provisions and its effects, and the measures taken under the "Règlement d'Exécution," which followed the Act, for the advancement of technical education amongst foresters in Switzerland. It is evident that from a very early date the various cantons endeavoured to preserve the forests. Thus, in 1314 the authorities of Zurich forbade "the felling, floating, or selling" of timber from the Sihlwald; in 1339, Schwyz forbade charcoal-burning near the chief towns of the canton, and a similar decree was promulgated in Fribourg in 1438. Industries using wood were in various cantons restricted in their operations; the laying out of new vineyards was prohibited under heavy penalties for centuries; and finally, during last century, the use of uncloven vine-props was forbidden. The exportation of timber took place only under great difficulties, and even the removal of timber from one place to another in Switzerland was, until 1848, very much restricted. In 1376, Zurich forbade clearings to be laid down in pasture, and Fribourg would not allow sheep-pastures to be established in clearings. Goats were not permitted to be let loose in the woods; and rosin-scrapers were excluded from many of the forests. None of these numerous decrees appear to have had much effect, the very number of them testifying to their powerlessness to check the evil. In many cases the general prohibition against wood-cutting gave way to a partial permission, as, for example, in Zurich, where the number felled was not permitted to exceed a stated total. This instance of Zurich gives us the first scientific treatment of the question, when the felling of the Sihlwald and other woods in the fourteenth century was regulated both as to the amount and the system of cutting.

In 1702, prior to which date attention was paid solely to the maintenance and protection of the timber, the Government appointed a Commission to inquire how the forests might be best preserved, enlarged, and improved; and subsequently issued a decree carrying the recommendations of the Commission into effect. In 1725, Berne followed the example of Zurich, and published forestry orders, which, like those of the latter, contained directions for the cultivation of timber and for permanent improvements. Similarly, in other cantons, improved systems were introduced; thus, in Fribourg, the compulsory planting of marshy meadow-land was decreed; in Lucerne a season was set apart for felling, the growth of oaks was recommended, and the formation of clearings was forbidden. In 1755 an excellent forestry code was drawn up by Joseph Wilhelm, Prince-Bishop of Bâle. About 1760, two scientific Societies—the Physical Society of Zurich and the Economical Society of Berne—made great efforts to introduce improved knowledge of woodcraft into Switzerland, and with this object they made strong representations to their respective Governments, and the Forestry Decrees of 1773 and 1786 were the results of their interference. The substance of these decrees may be stated to be the surveying of forests, the appointment of officials who would supervise planting, experiment on exotics, and help in teaching a more scientific system of wood-cutting. By means of these measures some real progress was made, which, however, was stopped by the general confusion during the beginning of this century; but, immediately peace was restored, the Helvetic Government turned their attention again to the forests, which by this time had suffered severely. Soleure was the first to start a system under which technical instruction, chiefly in forestry and geometrical surveying, was given to two citizens from each woodland district, the better qualified being chosen foresters. From this time until 1830, forest laws were drawn up universally, prescribing the modes in which timber was to be felled. Zug, in 1821, tried to give an increased value to her forests by endeavouring to extend scientific teaching among the people. In consequence of the disastrous floods in Switzerland in 1830, from this time we find that forest laws were more generally enacted and more rigidly enforced than they had ever been before. The number of officials was increased, and great attention was paid to their training. In fact, the spread of the science of forestry in Switzerland dates from this period. At first the people thwarted the officials in every way, but, becoming gradually enlightened as to the utility of the Government measures, they ceased from actual opposition. Even the most backward of the cantons began

to realize that their true interests lay in the preservation of the forests, both as a commercial speculation, having regard to the advancing price of timber, and as a support for precipitous ground, and on account of its domestic and national uses. With regard to the latter, it is worthy of note that the respective cantons, from the earliest times, supervised the numerous public woods; and that the frontier forests were always better looked after than any others, on account of their importance as a defence in time of war, and at the commencement of the eighteenth century woods were protected, as being safeguards against avalanches and landslips.

Hitherto the students trained in forestry had been sent to the schools in Germany, but in 1855 the Confederation took the matter up and established a Forestry School, in which henceforth Swiss students were educated in the art of wood-cutting and the kindred sciences. In 1858 a long and searching inquiry was made into the supposed connection of the forests and the course of the mountain torrents, and, as a consequence, the State aided the School of Forestry in their efforts to plant anew the ground where springs abounded, and officials were appointed for this purpose. With regard to these officials, mention of whom occurs in all the forest laws of Switzerland, we first hear of them in 1314, when, as in subsequent centuries, they were supposed to be aided by the inhabitants, every one of whom in a woodland district was sworn to disclose any breach of the decrees which came to his knowledge. For centuries these officials were mere guardians, commonly called *Bannwärte;* but the punishment of offenders rested with councils of magistrates, &c. The ordinary forest-keeper was generally nothing more than an intelligent wood-cutter; but when it was seen that some technical teaching was necessary, the skilled man, and, later still, the man with a knowledge of natural science and mathematics, was always preferred. In 1868 the disastrous floods gave a fresh impetus to the spirit of inquiry into the action of the forests on the rainfall and the course of the torrents; and we find in the revised Federal Constitution of 1874 an article inserted, giving the Federation control over the forests and waterways, and authority to interfere in any way they might think fit. Under this article two officials were appointed—the Federal Inspector of Forests, and also a Sub-Inspector. The Forestry Societies unanimously adopted a programme which, being presented to the Federal Council, was embodied in the Forest Law proposed by the Council in 1875. This proposed enactment led to much discussion in the Assembly, but was finally passed by both Houses on March 24, 1876. The district to be subject to the law included not only the high mountain ranges, but also the hills bordering on the plains, as sharing in the protection afforded against floods and avalanches by the works which were intended to be undertaken in the former. The district was bounded by a line starting from the east of Lake Leman along the south of the plain between the Alps and Mount Jura, thence to the north of Lake Constance—that is, a tract of country in all about 60 per cent. of the whole of Switzerland, or 6,750,000 acres, about 15·8 per cent. of which was forest land. It was decided that the rights of private owners should not be infringed except in case of necessity—that is to say, where the woods of private owners were "protecting" woods; in other words, where, on account of their position, they might have an influence on the climate, avalanches, landslips, &c. Each canton was required to maintain an efficient staff of officials; and to each individual who had received technical training an area of about 17,500 acres was assigned if in the plains, and 25,000 acres on the mountains. All the woods under official supervision, including, of course, private woods which came under the class "protecting" woods, were to be demarcated, all clearings were to be immediately planted afresh, and where necessary new forests were to be created, the Federal treasury bearing from 30 to 70 per cent. of the cost, or, in the case of replanting protecting woods, from 20 to 50 per cent., according to the difficulty and the importance of the works, which were always required to receive the approval of the Inspector-General before the Federal subvention was granted. All servitudes or easements in "protecting" woods were to be redeemed within ten years, and no new ones were permitted to be created. Anything which might endanger the utility of the forests was strictly forbidden; cattle were not allowed to graze, nor could leaves be collected except in fixed spots. To this enactment was added a "Règlement d'Exécution," which provides, among other things, for the course of education to be given to each student of forestry by the canton to entitle it to the Federal subsidy. The time of the course is not to be less than two months, which may be divided into two half-courses of a month each, but the whole course must be taken within a year. Instruction must be given in the following subjects:—(1) Forest-surveying and measurement in detail; calculations of the dimensions and value of single trees, and of outlying tracts of wood; road-making; safeguards against avalanches, &c. (2) Study of the different kinds of timber and of noxious plants. (3) Elementary knowledge of soils, and of their component parts. (4) Fundamental notions of the laws of climate and meteorology. (5) Cultivation and care of forests. (6) Book-keeping and other general branches of instruction valuable for under-foresters. A preliminary and a final examination are prescribed, and no license is granted except on good answering in the latter. The Federal Government pay the teachers, who are appointed by the canton subject to the approval of the Federal Government.

At the outset there were great difficulties in carrying out this law. Some of the cantons had not their codes of regulations drawn up till 1881, and, with the exception of the cantons of Zurich, Fribourg, and Vaud, the survey was not quickly completed. In 1886, however, the Army Staff finished the triangular survey intrusted to them. In 1886 the redemption of servitudes prescribed by the Act was not ended, and up to that time £9150 had been thus expended. There is not in the cantons an uniform organization for carrying out the Forest Law, and Dr. Fankhauser, one of the highest officials of the Forest Department, does not think that such an organization is possible, having regard to the differences in position and ideas of the various cantons. At the present time each canton possesses in a measure its own scheme of forestry organization. There are, however, two main systems in existence in the Federal district, the first of which prevails in the central, eastern, and southern parts of Switzerland. Each canton is divided into districts of from 17,500 to 35,000 acres each, and over each district the canton places an officer who has received scientific training; under him are the keepers and deputy-foresters, chosen by the owners from among the students of the local forestry school, and paid by them. Each deputy has about 3000 acres to take care of, and has but to carry out the orders of his superior as to felling, clearing, and replanting. In the next, however, a different system obtains. Here the country is far less mountainous, and the inhabitants industrial rather than agricultural in their pursuits. In these cantons the district forester has from 7500 to 17,500 acres under him, and in this district he marks out all the fellings to be performed, and in fact does everything but the manual labour, which he leaves to his inferiors. This district includes, among other cantons, Zurich, Berne, Lucerne, and Neufchâtel, where timber being very high in price, and the opportunities of sale being numerous, the country is frequently reafforested by private individuals, while in the other cantons the State is forced to do nearly everything. The cantons not within the control of the

Federal law differ from those here spoken of in their organization. In Bâle Campagne with its 37,000 acres of forest, 75 per cent. of this being public, has no officials whatever. Laws have been passed, but the people set them at naught; and similarly in Thurgovie there is the greatest opposition to any interference with what the people consider to be their ancient rights; and here also there are no officials, except one who has the care of 300 acres of State forest.

The salaries of the forest officials vary very much in the different cantons, but even in the best-paid districts the remuneration is very modest. Under-foresters receive sometimes a fixed salary, sometimes only daily wages when employed. If the former, the sum varies from £24 to £48; occasionally it reaches £60. If the rate of pay is per day, which is unusual, it is generally fixed at 4*s*. District foresters usually receive from £88 to £112 a year. In Uri, however, £120 is given, and in Glarus and a few other places as high as £160 per annum. Cantonal forest inspectors receive from £120 to £180 a year, besides allowances, which are always given to the higher officials when travelling on duty, ranging from 5*s*. to 8*s*., with the cost of the journey.

NOTES.

We regret to announce the death of Signor Giacomo di Brazza, brother of the Governor of the French Congo Settlements, also an African traveller well known by his investigation of the Ogowé River. He died at Rome, aged thirty.

Herr Andor Semsey has presented the sum of 8000 florins (£800) to the Natural Science Society of Budapest, for the printing of a work by Herr Otto Hermann on Hungarian birds.

The International Congress of Americanists, which met in 1886 at Turin, proposes holding its seventh session at Berlin early in the month of October. The Organizing Committee already includes such well-known names as Virchow, Reiss, and others.

Mr. A. W. Pickard-Cambridge has taken first place in Classics among the senior students at the last Cambridge Local Examination, and has been offered, in consequence, an Exhibition at St. John's College, Cambridge. He has won this honour at an almost unprecedentedly early age, being only fourteen years old. He has been a pupil of Weymouth College for the past four years, and is the son of the Rev. O. Pickard-Cambridge, F.R.S., the well-known naturalist.

A report of the Cambridge Local Examinations and Lectures Syndicate laying down a scheme for the examinations for commercial certificates has been confirmed by grace of the Senate. The examination is to be wholly separate from the local examinations, there being no papers of questions common to the two, and no common classification of successful students. The standard set by the Syndicate is that suitable for well-prepared students of seventeen. Amongst the compulsory subjects are arithmetic, and physical and commercial geography, whilst the optional subjects include algebra and one of the following five subjects in elementary science: (1) inorganic chemistry, theoretical and practical; (2) organic chemistry, theoretical and practical; (3) mechanics, including hydrostatics and pneumatics; (4) sound, light, and heat; (5) electricity and magnetism.

According to the *Oldham Evening Express* of March 16, what is described as a full-grown summer butterfly took refuge from a blinding snowstorm in a dwelling-house at Lusley Brook, near that town. The wings are said to be beautifully variegated; and on obtaining shelter in a warm room the butterfly thoroughly revived.

At the last meeting of the Calcutta Microscopical Society a paper was read by Mr. Simmons on the mango weevil, a pest which is spreading rapidly in India. He has devoted much attention to the weevil, and in this paper he gives much useful information as to its geographical distribution, the extent of the damage done by it, with the observations of English and American entomologists on its ravages among fruit. This lecture is believed to be the first attempt made in India to systematically study the habits of the weevil.

The Fund which has been established by Mrs. Elizabeth Thompson, of Stamford, Connecticut, "for the advancement and prosecution of scientific research in its broadest sense," now amounts to $25,000. As accumulated income is again available, the Trustees desire to receive applications for appropriations in aid of scientific work. This endowment is not for the benefit of any one department of science, but it is the intention of the Trustees to give the preference to those investigations *which cannot otherwise be provided for*, which have for their object the advancement of human knowledge or the benefit of mankind in general, rather than to researches directed to the solution of questions of merely local importance. Applications for assistance from this Fund, in order to receive consideration, must be accompanied by full information, especially in regard to the following points:—(1) Precise amount required. Applicants are reminded that one dollar is approximately equivalent to four English shillings, four German marks, five French francs, or five Italian lire. (2) Exact nature of the investigation proposed. (3) Conditions under which the research is to be prosecuted. (4) Manner in which the appropriation asked for is to be expended. All applications should be forwarded to the Secretary of the Board of Trustees, Dr. C. S. Minot, Harvard Medical School, Boston, Mass., U.S.A. It is intended to make new grants at the end of 1888. The Trustees are disinclined, for the present, to make any grant exceeding $500.

The following is a list of the grants already made from the "Elizabeth Thompson Science Fund":—(1) $200 to the New England Meteorological Society, for the investigation of cyclonic movements in New England. (2) $150 to Mr. Samuel Rideal, of University College, London, England, for investigations on the absorption of heat by odorous gases. (3) $75 to Mr. H. M. Howe, of Boston, Mass., for the investigation of fusible slags of copper and lead smelting. (4) $500 to Prof. J. Rosenthal, of Erlangen, Germany, for investigations on animal heat in health and disease. (5) $50 to Mr. Joseph Jastrow, of the Johns Hopkins University, Baltimore, Md., for investigations on the laws of psycho-physics. (6) $200 to the Natural History Society of Montreal, for the investigation of underground temperatures. (7) $210 to Messrs. T. Elster and H. Geitel, of Wolfenbüttel, Germany, for researches on the electrization of gases by glowing bodies. (8) $500 to Prof. E. D. Cope, of Philadelphia, Penn., to assist in the preparation of his monograph on American fossil vertebrates. (9) $250 to Mr. W. H. Perkin, Jun., for experiments on the synthesis of uric acid. (10) $125 to Mr. Edw. E. Prince, of St. Andrews, Scotland, for researches on the development and morphology of the limbs of Teleosts. (11) $250 to Mr. Herbert Tomlinson, of University College, London, England, for researches on the effects of stress and strain on the physical properties of matter. (12) $200 to Prof. Luigi Palmieri, of Naples, Italy, for the construction of an apparatus to be used in researches on atmospheric electricity. (13) $200 to Mr. Wm. H. Edwards, of Coalburg, W. Va., to assist the publication of his work on the butterflies of North America.

The latest reports received by the Hydrographic Office of the United States about the logs of the great raft abandoned south of Nantucket about three months ago, prove that,

though they are now widely separated, their general drift has been in an east-south-east direction, the logs being found a little to the southward of this line. That they were not carried more to the northward and eastward by the Gulf Stream, as would be expected, was probably due to the strong north-west winds which prevailed during the latter part of December and the first part of January. Fortunately, no vessel has been disabled by collision with them, although the German ba k *Bremen*, which was in company with the logs for five days, in latitude 39° north, longitude 62° west, had her sheathing torn and rudder injured.

We have received from Mr. R. T. Rohde, of the New Oriental Bank, a well-known authority on questions connected with currency and banking, a pamphlet entitled "A Practicable Decimal System for Great Britain and her Colonies." In criticizing the Report on Decimal Coinage of the Parliamentary Committee of 1853, he proposes, amongst other things, to preserve the sovereign as the standard unit of the country, but to call it five dollars British sterling, each dollar being divisible into 100 cents, a cent thus being nearly one halfpenny in value; the sovereign and half-sovereign to remain, as before, the only gold coins in the country, the latter to be legal tender for an amount not exceeding £5, and the former for any amount. In the silver and copper coinage he would not make any alteration. He also advises the allowing of the use of the cental of 100 avoirdupois pounds, divisible into any decimal subdivision of such pound avoirdupois; the using of the foot as the standard measure, such foot being divisible into 100 equal parts, ten of which make one decimal inch. As a measure of capacity, he would suggest a vessel equal to one-tenth of an Imperial gallon, such ves el to contain one pound avoirdupois of distilled water at a temperature of 60° F.

An interesting experiment in the planting of waste saline tracts in India has been carried out by Mr. Maries, superintendent of the gardens of the Maharajah of Durbhunga. The results have been communicated to the Agricultural Department, Bengal, and are contained in the last report of the Director. Mr. Maries says that six years ago, when he went to Durbhunga, he did not know what to do with patches of saline soil, on some of which not even weeds would grow. He dug the soil to the depth of two feet, and planted it thickly at the commencement of the rainy season with trees which had been grown in pots till they were about three feet high. In three years the ground was filled with roots, and to all appearances the salt had gone. When the trees were thinned out last year, leaving only the best, the ground was found to be in good condition. Similar experiments have been carried out in other places, and now Mr. Maries has splendid plantains growing on soil which a few years ago would not even grow a weed. He employed various kinds of trees in his reclaiming operations, but he says that the best were the *Inga Saman*, or rain trees, and the *Albizzia Procera*. The former is valuable as producing an enormous quantity of surface-feeding roots, and these decaying yearly leave a rich vegetable deposit on the soil. The trees soon completely change the character of the soil. The timber is excellent for fuel, and the trees bear lopping well. It is such an enormous water absorber that it would most probably be very useful in swampy places as a fever preventive, like the willow which is planted in China around the villages in the rice districts.

M. Léotard, Secretary to the Scientific Society of Marseilles, describes, in a recent issue of *La Na'ure*, the appearance of certain peaks of the Pyrenees as seen from Marseilles and its neighbourhood. Every year, about February 18 and October 31, Mount Carrigou, situated in the Eastern Pyrenees, and 2765 metres above the level of the sea, may be distinguished from Notre-Dame de la Garde in Marseilles, projected on the disk of the sun as the latter is about to set. From the top of Marseille-Veyre, 8 kilometres south of the town, the same observations may be made about February 13 and October 28. A straight line drawn from Notre-Dame de la Garde to the summit of Carrigou is 253 kilometres. Both Carrigou and the peak of Treize-Vents have been seen frequently since 1808, and this year M. Léotard and some of his colleagues made observations on the subject, and secured illustrations.

Referring to a journey of exploration in Australia which M. Ernest Favenc proposes to take, the *Colonies and India* says that no group of colonies in the world have taken more interest in exploration than those in Australia. In Melbourne especially, scientific Societies have given attention to this subject. It appears that the design in respect to a trip which M. Ernest Favenc proposed to take has now assumed a definite shape, and that he will pursue his object if only the Victoria and New South Wales branches of the Royal Geographical Society of Australasia will subscribe the necessary funds to send a surveyor with him. His intention is to start for Western Australia, there to inspect a large area of unstocked country, and subsequently to undertake a trip into the unexplored region between the tracks of Forrest and Warburton. If he finds the season favourable, then he proposes to make south, cutting the tracks of the other explorers at right angles. It should be mentioned that the explorer makes it a condition that the surveyor shall also be a fair mineralogist and know something of botany. Horses, saddlery, and rations will be found by the leader, but the passage, instruments, and salary of the surveyor selected are to be provided by the Societies named. When the matter came before the Victorian branch, at a meeting of the Council, a sub-committee was appointed to deal with the applications for the post of surveyor, which, it is believed, will be numerous.

The storm which was experienced on the Atlantic coast of the United States on the 11th and 12th inst. was apparently due to a disturbance which was situated over Georgia on the 10th, and which subsequently moved rapidly up the American coast. The storm apparently commenced with a warm southerly wind and heavy rain, which changed very suddenly to a north-westerly gale and violent snowstorm. The character of the storm was that common to the blizzard of the United States, and the intense cold of the north-westerly wind was evidently due to the rear of the disturbance stretching for a long distance over the cold continent of America. The loss occasioned by the storm, both to life and property, is immense.

The Italian Meteorological Office has issued a report on the climate of Massowah, based upon the observations made with standard instruments by the officers of the Italian expedition, between May 1885 and September 1887. The discussion is divided into two periods (1) May 1885 to May 1886; and (2) June 1886 to September 1887. The results show that the mean monthly temperature is above 86° in the months May to October. The maximum occurs in August: 108° in 1886, and 101°·8 in 1887. The minimum occurs in February; in two ten-day periods the thermometer fell to 66°, but there is little difference between January and February. Rainfall is very scarce and erratic, the fall of a few days may exceed that of the rest of the year. In the first twelve months 4·1 inches fell on thirty-four days; in the second, 4·3 inches on twenty-six days. The prevalent winds are northerly and southerly. The latter predominated from June 1885 until the end of the year; from January 1886 until September 1887, northerly winds prevailed. The above temperatures, while showing that Massowah is very hot, are lower than those sometimes quoted, apparently owing to more careful exposure in the present investigation.

The New York Academy of Sciences (says *Science*) was organized in 1817 as the Lyceum of Natural History. It is fourth in point of age among American scientific Societies. The name and constitution were changed in 1876. The Annals, begun in 1824, have been distributed in all lands, and have given world-wide reputation to the Society. The Transactions, begun in 1881, give a record of the meetings, papers, and discussions, are published in monthly or bi-monthly numbers, and make an octavo volume each year. The library now numbers over eight thousand titles, and is especially rich in sets of the publications of foreign Societies. It is now on deposit in the Library Building of Columbia College, and is accessible to the public from 8 a.m. to 10 p.m. every day of the year except Sundays. The cabinet was destroyed by fire in 1866. Previous to that date it was the principal collection in the city, and did a noble work. The Academy has long looked forward to the time when it could secure a building of its own, such as the corresponding Societies in Boston and Philadelphia have long enjoyed. It is not to the credit of New York that its oldest scientific organization, after nearly three-quarters of a century of steady and persevering activity, should be still unprovided with a building, while many other cities can show noble monuments of scientific interest and public spirit. Why should not the recent meeting of the American Association in this city be permanently commemorated by the erection of a fire-proof building for the accommodation of the Academy, or perhaps of several other Societies under the same roof—a building which should be at once a benefit and an honour to the metropolis of America? The interest of the community has been aroused and quickened in the direction of science by the meeting of the Association, and the Academy of Sciences would now invite the citizens of New York to take a greater interest in its work.

Pure trichloride of nitrogen has at last been prepared and successfully analyzed by Dr. Gattermann, of Göttingen. The first result of these researches upon this terribly explosive substance brought to light the fact that the chloride of nitrogen prepared as usual by the action of chlorine gas upon ammonium chloride is by no means a homogeneous substance, that it really consists of a varying mixture of several chlorides. Moreover, it was found that the longer the time during which the chlorine was allowed to act, the more nearly the composition of the product approached NCl_3; but pure NCl_3 can never be obtained in this way, owing to the excess of ammonium chloride always present. Dr. Gattermann, however, prepared a quantity of this crude product, as richly chlorinated as possible, washed it well with water until all the sal-ammoniac was removed, drained it as free as might be from the water, and then led over it a rapid stream of chlorine. The resulting oil was again washed, carefully dried, happily without accident, and finally analyzed. The percentage of chlorine found was almost identical (89·17) with that required for NCl_3 (89·10). The success of these dangerous operations is all owing, it appears, to the fact that they were performed upon dull wintry days, when the sun's actinism was very low; indeed, Dr. Gattermann was almost led to believe that the disasters which have imparted to the history of this compound so tragical a character must have been owing to some fault of the experimenters. But at last—it was about the thirtieth preparation—the oil quietly reposing in the chlorinating apparatus suddenly exploded with its usual detonation. At the same moment Dr. Gattermann noticed that the sun had broken through the clouds, and was shining upon his apparatus. Here then was the cause of these apparently spontaneous explosions: chloride of nitrogen is violently dissociated by the wave-motion of light. Following this up, it was found that the burning of a piece of magnesium ribbon in proximity to the oil was quite as effective in producing an explosion. Finally, Dr. Gattermann has determined the temperature of dissociation of the compound. About half a gramme was heated in a thin-walled tube placed in a beaker of liquid vaseline, the thermometer being read off by means of a telescope placed at a safe distance. As high as 90° C. the oil remained unchanged, but at 95° it exploded with such violence that the whole apparatus was destroyed. One feels much regret on reading Dr. Gattermann's concluding observations, in which he states that his eyes and nerves have been so much affected that he is obliged temporarily to give up all further work upon this interesting substance.

Messrs. Crosby Lockwood and Son are about to publish the following books:—"Waterworks: being Notes on the Storage of Water in Reservoirs, &c.," by Charles Slagg; "Practical Surveying: a Text-book for Students preparing for Examinations or the Colonies," by George W. Usill; "Granites and our Granite Industries," with numerous illustrations, by G. F. Harris; a treatise on "Asbestos, and the Asbestos Mines of Canada," by Robert H. Jones; "The Mechanic's Workshop Handy-book," by P. N. Hasluck; and the fourth edition of "A Treatise on Metalliferous Minerals and Mining," by D. C. Davies.

In a recently-published Report on the Fisheries of New South Wales, Mr. Griffin, the American Consul at Sydney, refers to the great wealth of the colony in this respect, which is totally neglected. Up to the present, no attempt has been made to develop an export trade in fish. In fact, there are only eleven hands employed in the whole colony in fish-curing, with a capital of no more than £250, and the output does not annually exceed £200 in value. Yet the amount of tinned fish imported by the colony last year exceeded 2000 tons, of which about one-half was from the United States, and almost all the remainder from Great Britain. With regard to the species of fish suitable for preserving which are to be found in the waters of the colony, the mullet (*Mugil grandis*) is there in abundance, and when well cured is superior to anything of the kind in the world. Generally, it may be said that the fish fauna of Australia differs very little from similar species in Europe and America. The most remarkable fish in Australia is the *Phyllopteryx*, described as "the ghost of a sea-horse with its winding sheet all in ribbons about it; and even as a ghost it seems to be in the last stage of emaciation, literally all skin and grief."

The resistance of pollen to various external influences is the subject of a recent inaugural dissertation by Herr Rittinghaus in Bonn (*Naturf.*, 1, 1888). As to temperature, he found most pollen able to bear 90° C. half an hour, without losing the power of germination. A temperature-maximum was reached at 104°·5 for ten minutes. In conditions favouring germination, pollen does not bear such high temperatures as in the air-dry state. A moderately raised temperature (32°) accelerates growth of the pollen tubes. Low temperatures (*e.g.* under 9°) prevent germination, though a cooling to 20° for forty minutes can be borne without injury. As to *liquid* chemical reagents, the plasma of pollen proved very sensitive to antiseptics (more so, as a rule, than micro-organisms), but the resisting power is pretty different in different sorts of pollen. Chloroform *vapour* acting for twenty minutes was fatal, bromine vapour in five minutes, ammoniacal vapour in ten to twenty minutes. Rotation, several hours, of a spherical vessel holding pollen with nutritive solution, did not prevent free germination. The retention of the power varies widely in different plants. Thus, *Cyclamen* lost it soonest, in seventeen days; while *Clivia*, a narcissus, still had it on the sixty-sixth day (*Pæonia* fifty-eight, *Camellia* fifty-one, *Azalea* forty-two). The average is thirty to forty days.

A recent number of the *Indian Agriculturist* contains a notice of a little book written in Bengalee, by a Hindoo gentleman, Nidhiram Mookerjee, and published at the Bangabasi Press, Calcutta. The work is on pisciculture, and gives us the results of the labours of an ardent student of fish and their

habits. He established a fish-farm on his own estate, and watched over it for many years. He divides his subject into five parts. In the first place, he discusses the fish supply of Bengal, and in doing so shows that the supply is frequently not equal to the demand—a fact due chiefly to the absence of skilled fishermen. And so it happens that at various seasons breeding and unmatured fish are brought to market to meet the demand. The second chapter treats of the best food for fish; the third of hatching and breeding, and the proper precautions to be taken at those times. The fourth part deals with the question from a commercial and speculative point of view. A little capital, the author says, if wisely invested in pisciculture and in fisheries produces a greater return than in any other industry; for while, as Prof. Huxley says, an acre of land will produce in the year a ton of grain or two or three hundredweight of meat, the same extent of water in a good fishing-ground will yield a greater weight of fish in a week. The author begs of his countrymen to pay attention to this much neglected subject; he puts his practical experience before them, and thinks, that in a country like Bengal, where fish forms a large portion of the dietary of the people, it is a pity that more is not known of this subject. One of the most valuable portions of this little work is the fifth, in which he gives a scientific description and classification of almost all the known fish in the waters of Bengal, with their Bengalee equivalents.

THE additions to the Zoological Society's Gardens during the past week include two Stock-Doves (*Columba œnas*), British, presented by Lieut.-Colonel W. G. Dawkins; a Gayal (*Bibos frontalis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

DISTRIBUTION OF THE SUNSPOTS OF 1886 AND 1887.—Prof. Spoerer points out in a short note in the *Astronomische Nachrichten*, No. 2828, that the predominance of the southern hemisphere over the northern as to the numbers and areas of sunspots which they have displayed has continued throughout the two years just past. It would seem, indeed, as if the maximum for the southern hemisphere had fallen later than for the northern, for after the last return of the great group of November 12–25, 1882, the latter hemisphere became comparatively quiescent for a considerable time, and from that date the predominance of the southern hemisphere has been almost uninterrupted, the displays it exhibited during the latter part of 1883 and the earlier months of 1884 being so considerable and so numerous as to make the date of maximum the same for the sun as a whole as for the southern zone. So in the decline since the maximum, not only has the mean spotted area of the northern hemisphere been scarcely more than half that of the southern, but the running down in latitude has been more marked in the former than the latter. Thus in 1884, the northern zones above lat. 25° were already free from spots, whilst in the south the zone 25° to 30° was still occupied. In 1886 spots had ceased to be seen in the zones north of N. lat. 20°, but were still seen in the corresponding southern belt; whilst in 1887 they had almost vanished from the zone N. lat. 15° to 20°, though still fairly numerous at a like distance from the equator on the other side. The actual distribution of the spots is shown by Prof. Spoerer in the following table:—

Year.	+20°		+15°		+10°		+5°		0°		−5°		−10°		−15°		−20°		−25°	Totals. N.	S.
1886		17		30		40		14		50		45		68		47		5		101	215
1887		2		22		15		14		19		56		27		14				53	116

THE TOTAL ECLIPSE OF THE MOON, JANUARY 28.—By the kindness of Dr. E. Lindemann we are enabled to give the following further list of occultations observed during the total eclipse of the moon on January 28:—Amherst, U.S., 7; Clinton, U.S., 3; Copenhagen, 25; Harvard College, U.S., 23; Madrid, 20; Montreal, 6; Moscow, 15; Nice, 24; Princeton, U.S., 8; Toulouse, 13; Utrecht, 15; Washington, 11; West Point, U.S., 2. The weather was also favourable at the Birkdale Observatory, Southport, and at Berlin and Dun Echt; but at the last two Observatories, and also at Lord Rosse's, the occultations were not observed. The sky was cloudy at Herény, O'Gyalla, Quebec, Rio Janeiro, Stockholm, and Vienna.

SPECTROSCOPIC DETERMINATION OF THE ROTATION PERIOD OF THE SUN.—Mr. Henry Crew, Assistant in Physics at the Johns Hopkins University, has recently published (*American Journal of Science*, February 1888) a series of observations made with a fine Rowland grating of 14,436 lines to the inch, of the relative displacement of certain lines in the solar spectrum, as given by the opposite limbs, with a view to determine the rotation period of the sun. The result which he obtained from 455 settings in the course of observations ranging over four months and a half, gives, for the mean equatorial velocity, $v' - v'' = 2{\cdot}437 \pm {\cdot}024$ miles per second, corresponding to a true period of 25·88 days. But an unexpected and remarkable circumstance was brought out by the investigation, in that the observations seemed to show a gradual *increase* of daily angular motion with higher heliographical latitude, whilst, as is well known, Carrington found a *decrease* of such motion for the spots. Mr. Crew gives for the equation of this change—

$$v = 1{\cdot}158 \cos \chi^\circ (1 + 0{\cdot}00335\ \chi^\circ),$$

whence we have for the daily angular motion of any point in the reversing layer—

$$\theta = 794' (1 + 0{\cdot}00335\ \chi^\circ),$$

whilst Carrington obtained for the sunspots—

$$\theta = 865' (1 - 0{\cdot}191 \sin \tfrac{7}{4}\chi^\circ).$$

The greatest irregularities in the value of $v' - v''$ occurred between the latitudes 15° and 25°, *i.e.* in the chief spot zone.

It should be added that different lines gave different values of $v' - v''$, with nearly as large a range as the different latitudes did, but there appeared to be no connection between the order of the velocities and the order in which the elements causing the lines observed are generally supposed to be distributed in the solar atmosphere. The double line, 1474 K, of which one component is due to iron, and the other is the line of the corona, gave no evidence of variation in width on one limb, as compared with the other, so if the two lines be produced by absorption from different layers, those layers cannot be drifting with respect to each other at a higher rate than one-third of a mile per second.

The spectrum of the fourth order was used throughout. Attempts were made to measure the relative displacement of the D_3 line, as given by opposite limbs, but with this dispersion the definition was not sufficiently good to permit satisfactory measures of the line to be made.

NEW MINOR PLANET.—A new minor planet, No. 273, was discovered on March 8, by Herr Palisa at Vienna. This is Herr Palisa's sixty-first discovery.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *MARCH* 25–31.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 25

Sun rises, 5h. 51m.; souths, 12h. 5m. 54·3s.; sets, 18h. 20m.: right asc. on meridian, 0h. 19·4m.; decl. 2° 6′ N. Sidereal Time at Sunset, 6h. 34m.

Moon (Full, March 27, 22h.) rises, 15h. 12m.; souths, 22h. 28m.; sets, 5h. 29m.*: right asc. on meridian, 10h. 43·4m.; decl. 10° 51′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	5 10 ...	10 27 ..	15 44 ...	22 40·8 ...	9 9 S.
Venus ...	5 10 ...	10 23 ..	15 36 ...	22 36·7 ...	9 59 S.
Mars ...	20 12* ...	1 34 ...	6 56 ...	13 46·0 ...	8 10 S.
Jupiter ...	23 54* ...	4 6 ...	8 18 ...	16 18·6 ...	20 25 S.
Saturn ...	11 54 ...	19 53 ...	3 52* ...	8 7·6 ...	20 48 N.
Uranus ...	19 12* ...	0 47 ...	6 22 ...	12 58·8 ...	5 32 S.
Neptune..	7 49 ...	15 30 ...	23 11 ...	3 44·1 ...	18 6 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

March.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
28 ...	80 Virginis	... 6 ...	19 49 ...	20 43 ...	10 230
31 ...	η Libræ ...	... 6 ...	1 20 ...	2 30 ...	61 234

March.	h.	
28 ...	2 ...	Mercury in conjunction with and 0° 2′ north of Mars.
29 ...	2 ...	Mars in conjunction with and 2° 35′ south of the Moon.
31 ...	1 ...	Mercury at greatest elongation from the Sun 28° west.
31 ...	2 ...	Saturn stationary.
31 ...	19 ...	Jupiter in conjunction with and 3° 32′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·4 ...	81 16 N. ...	Mar. 28,	5 3 *m*
S Piscium	1 11·7 ...	8 20 N. ...	,, 31,	*M*
Algol	3 0·9 ...	40 31 N. ...	,, 26,	22 33 *m*
			,, 29,	19 21 *m*
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,, 25,	19 8 *m*
			,, 26,	22 24 *m*
S Cancri	8 37·5 ...	19 26 N. ...	,, 25,	20 14 *m*
δ Libræ	14 55·0 ...	8 4 S. ...	,, 27,	23 48 *m*
U Coronæ	15 13·6 ...	32 3 N. ...	,, 30,	21 15 *m*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 26,	4 32 *m*
			,, 27,	0 40 *m*
W Sagittarii ...	17 57·9 ...	29 35 S. ...	,, 28,	3 0 *M*
R Scuti...	18 41·5 ...	5 50 S. ...	,, 25,	*m*
R Delphini	20 9·5 ...	8 45 N. ...	,, 28,	*M*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	,, 30,	20 0 *M*
			,, 31,	22 0 *m*
δ Cephei	22 25·0 ...	57 51 N. ...	,, 27,	1 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near β Draconis	263° ...	49° N.	
,, ζ Draconis	260 ...	63 N. ...	March 28. Rather slow.

GEOGRAPHICAL NOTES.

In a previous number we referred to the return of M. Edouard Dupont, Director of the Brussels Natural History Museum, from his visit to the Congo for the purpose of scientific exploration. Some of the results of his visit he described the other day to the Belgian Society of Engineers. M. Dupont pointed out that the African interior is drained mainly by four great rivers—the Nile, the Niger, the Zambezi, and the Congo—each of which has to break through the low range that bounds the interior somewhat saucer-shaped table land. The Congo, before making its great final effort, is to some extent dammed back into the reservoir known as Stanley Pool. M. Dupont's journey extended from the mouth of the river to the *embouchure* of the Kassai. The subsoil of the Lower Congo he found to be a soft and impure limestone covered with sand and clay. The mountainous region begins before arriving at Boma, and may be divided into three sections, according to the composition and aspect of the rocks. There is in the first place granite, gneiss, mica-schist, quartzite, and amphibolic rocks, in strongly inclined beds, and extending from Fetish Rock, below Boma, to the neighbourhood of Isanghila. The river from Vivi rushes in a series of cataracts through a gorge 55 miles long. Then follow schists and sandstones; and a little beyond Isanghila, at the great bend of the Congo, appear masses of limestone, very similar to those of the Meuse, and which alternate with the schists for about 35 miles. Then follow schists and red sandstones to beyond Manyanga. At Isanghila the banks rise into walls, some 700 feet high, of rough-grained, almost horizontal sandstone. This ends at Stanley Pool, where begins the Upper Congo. There is an immediate change in the strata. Some coherent sandstones show themselves at the base of the new deposits, and are topped by a great mass of soft sandstone, of the whiteness of chalk. M. Dupont traced these new rocks to the mouth of the Kassai, where there was nothing to indicate that they soon came to an end. He believes, on the contrary, that they constitute the subsoil of the greater part of the Upper Congo. M. Dupont is convinced, from his observations on the Congo, that the waters in the interior of Central Africa were at one time accumulated in a great lake, of which Stanley Pool is the last remnant. Gradually rising to the height of the mountains that bordered the plateau, they at last overtopped them, and, rushing down towards the Atlantic, gradually scooped out the channel now occupied by the Lower Congo. Stanley Pool, he considers, is the final stage of this supposed great internal lake.

A Brussels telegram announces that Lieut. Van Gele has at last succeeded in tracing the connection between the Mobangi and the Wellé, proving that the latter flows into the Congo, and is not the upper course of the Shari, thus solving one of the few remaining hydrographical problems in Africa.

In Ergänzungsheft No. 89 of *Petermann's Mitteilungen*, Prof. R. Credner concludes his very valuable monograph on "Reliktenseen,"—lakes which have remained behind after the departure of the sea from a particular area, as contrasted with continental lakes, which have from their origin been altogether independent of the sea. In the present instalment Prof. Credner deals in detail with the geological evidence, and with the various classes of "Reliktenseen" and the mode of their formation. He divides such lakes into three great classes: (1) such as have been formed through the damming up and isolation of parts of the sea through the elevation of the land above sea-level, as in the case of Lake Pontchartrain and the Kurische Haff; (2) such as are due to the isolation of basin-formed depths of the ocean-bed as a result of "negative changes in level"—emersion lakes, as Loch Lomond and Lakes Wetter and Wenner; (3) those caused by the retirement or shrinking of mediterranean seas, as the Caspian and Lake Aral.

At the last meeting of the Royal Geographical Society, Mr. Douglas W. Freshfield read a paper giving the results of his visit to the Caucasus last summer in company with M. de Dechy. Mr. Freshfield dealt at great length with the orography, the glaciation, geology, and ethnology of the Caucasus, and it is impossible to give an adequate idea of his important paper in a note. We can only refer to one or two important corrections which he made in the prevalent statements about the Caucasus. Some existing misconceptions are due to the fact that the Russian staff map embraces only the lower features, the higher ranges being unmapped. Mr. Freshfield dealt mainly with the part of the chain between Elbruz and Kazbek—the Central Caucasus. The geological structure of the chain has been represented with general accuracy by M. Ernest Favre, a son of the well-known Genevese geologist, who visited it in 1868. The backbone, composed of two or more ridges closely parallel, with many short spurs, is in great part gneiss or granite mixed up with crystalline slates. By what seems a strange freak of Nature, it is, east of Adai Choch, rent over and over again to its base by gorges, the watershed being transferred to a parallel chain of clay slates ("Palæozoic schists"), which has followed it from the Black Sea. There are clay-slate formations north as well as south of the granite backbone; but on the north they take the form of rolling downs—of any peaks they ever had they have long been denuded. What the mountain climber looking out from any northern outlier of the granite chain sees is a limestone crest, turning its precipitous face towards the snows, sinking gradually to the low foot-hills which fringe the steppe. It is pierced by deep romantic defiles through which the glacier torrents make their escape. South of the Caucasus, parallel to, but much further from the main chain, runs a line of limestone heights, the most conspicuous summits of which are the Quamli, close to the Rion, and the Nakerale range, the limit of the Radsha. At the foot of the latter lie the coal-mines of Khebouli, recently connected with Kutais by a railway. Over the summit plateau spreads one of the noblest beech forests in the world, varied by an undergrowth of azaleas, laurels, and box, such as we try vainly to imitate in our English parks. Parallel chains and longitudinal valleys characterize this portion of the chain. In the most reputable treatises it is stated that there are not 50 square miles of glaciers in the Caucasus altogether. Mr. Freshfield shows that such a statement is ludicrously absurd. The glaciers of the main chain are many, and some of them are enormous. Among those that have the largest basins Mr. Freshfield mentions, between the Djiper Pass and the Mamisson on the south side, the Betsho, the Ushba, the Gvalda, the Thuber, the Zanner, Tetnuld, and Adish, the Sopchetura at the western and at the eastern source of the Rion. On the north side there is a great glacier in every glen; the Karagam and the Bezingi are the largest; next come the Dychsu, the Zea, the Adyrsu, and Adylsu, and a host of others lying not only on the main chain, but on its spurs, which are glaciated to an extent of

which the Ordnance map gives no hint. On the "Palæozoic schist" range, south of Suanetia, there are glaciers not very inferior to those of the Grand Paradis group, near Aosta. Dismiss for ever, Mr. Freshfield says, that preposterous fiction about the 120 square kilometres of ice in the Caucasus. It is too soon to say how many square kilometres there really are. One estimate, Von Thielmann's, would make the extent covered by ice close upon 2000 square kilometres, or equal to that in Switzerland—political Switzerland, not the Alps. Mr. Freshfield dwelt on many other points in connection with this interesting range, his notes on the inhabitants of the Caucasus being specially valuable, correcting as they do many prevalent errors.

OUR ELECTRICAL COLUMN.

CONSIDERABLE attention has been drawn to the peculiarities of manganese steel by a paper read before the Institution of Civil Engineers, by Mr. Hadfield. Not only is such steel entirely non-magnetic, but its electric resistance is extremely high. Prof. Fleming (*Electrician*, March 9) gives the following figures:—

German silver	20·9	·044
Platinoid	32·8	·021
Manganese steel	68	·122

The first column gives the resistance in microhms per cubic centimetre at 0° C., and the second column the average percentage variation of resistance per 1° C. between 0° and 100° C. These figures agree very well with those given by Prof. Barrett at the British Association meeting at Manchester.

HEIM has been investigating the electro-positive character of magnesium, with the view of replacing zinc in primary batteries. He finds that in a Daniell cell its E.M.F. is 2 volts, in a Grove cell it gives 2·9 volts, and in a Leclanché cell 2·2 volts. In a bichromate cell it gives as much as 3 volts.

MAGNESIUM can now be produced for about 8*s.* per lb., but local action is considerable, and its constancy uncertain. Hence, except for exceptional circumstances, its practical use is still questionable.

PROF. OLIVER LODGE has been giving some admirable lectures on lightning-protectors at the Society of Arts, and has pronounced the use of copper for such purposes as doomed. He argued that the supposed area of protection was mythical, and that the true way to protect a building was Maxwell's cage. He advocated iron, and showed copper to possess "inertia" to such an extent as to render its use dangerous. He also found that under certain circumstances, such as sudden violent discharges, untempered by time, points were of no use, but he suggested the use of barbed wire along the ridges and eaves of roofs.

THAT careful and accurate worker, Prof. Roberts-Austen submitted a paper to the Royal Society on the 15th inst., in which he narrated his recent inquiries into the mechanical properties of certain alloys that will have an important bearing on the metallic conductors employed in electrical enterprises. He has found that the tenacity of pure gold is very much diminished by the smallest admixture of impurities, and that this follows the order of the atomic volumes of the elements. Those elements the atomic volumes of which are higher than gold greatly diminish its tenacity. Doubtless the same principle is applicable to copper and other metals. The abnormal price of copper has raised a great demand for some better conductor than iron, or some improvement of iron in this respect.

DERHAM'S HYDROMETER.

THE Revenue system of estimating the duty on spirits consists of hydrometer, and tables of strengths for each degree of temperature from 30° to 80° F. When constructing the present Revenue tables of strengths, Sikes ignored the expansion and contraction of spirits due to variations of temperature from the standard temperature of 51° F., and assumed that the strength of any given sample of spirits remained the same at all degrees of temperature. From this false assumption it follows in practice, for example, that 100 gallons 40 overproof at 51° are estimated at 98·9 gallons at 30°, and 101·6 gallons at 80°, of the same strength as at 51°; reducing these quantities to the standard of proof strength, we have—

At 30°	...	98·9 × 1·40 = 138·5 gallons of proof,
51°	...	100·0 × 1·40 = 140·0 ,, ,,
80°	...	101·6 × 1·40 = 142·2 ,, ,,

showing a discrepancy of over 3½ gallons, although the same actual quantity of spirit is present in each case.

In its original construction, Sikes's hydrometer was not intended to furnish specific gravities, but simply so many indications, respectively corresponding to the strengths in his tables. But it has since been found necessary to supply a table of specific gravities corresponding to the indications of the instrument. It is well known that scientific precision cannot be attained in experiments with the hydrometer, consequently the specific gravities in this table are far from accurate: for example, the specific gravity at the proof point, to the accurate definition of which the Inland Revenue attaches so much importance, is given as ·9233, instead of ·9236. The whole specific gravity table is in fact incorrect, the error sometimes amounting to two subdivisions of the stem. The errors, however, arising from this source are trifling compared with those inherent in the tables of strengths. For the purpose of constructing correct tables of strengths, the best data and those susceptible of the most accurate determination are the specific gravities of the spirits and the percentage by weight of alcohol they contain. The specific gravity of proof spirit, as defined by the Spirit Act is ·9236; therefore the weight of one gallon is 9·236 pounds. Proof spirit contains 49·3 per cent. by weight of alcohol, of specific gravity ·79385 at 60°; therefore one gallon of proof spirit contains—

$$\frac{9 \cdot 236 \times 49 \cdot 3}{100} = 4 \cdot 553 \text{ pounds of alcohol.}$$

To determine the true ratio of any spirit to proof spirit nothing more is required than to ascertain the weight of alcohol in one gallon of the spirit, and to divide that weight by the pounds of alcohol in a gallon of proof spirit; for example, spirit having a specific gravity of ·825 at 60° weighs 8·25 pounds per gallon; its percentage by weight of alcohol is 89·13; therefore one gallon contains—

$$\frac{8 \cdot 25 \times 89 \cdot 13}{100} = 7 \cdot 353 \text{ pounds of alcohol,}$$

equivalent to

$$\frac{7 \cdot 353}{4 \cdot 553} = 1 \cdot 615 \text{ gallons of proof spirit.}$$

Or 100 gallons are equivalent to 161·5 gallons of proof spirit, and the spirit is said to be 61·5 overproof. It is obvious that although the bulk and specific gravity of a spirit vary with the temperature, the percentage by weight of alcohol it contains does not vary from that cause. The specific gravity of the spirit in the preceding example is ·839 at 30°; the weight of one gallon therefore is 8·39 pounds; its percentage by weight of alcohol is 89·13 as before; therefore one gallon contains—

$$\frac{8 \cdot 39 \times 89 \cdot 13}{100} = 7 \cdot 478 \text{ pounds of alcohol,}$$

equivalent to

$$\frac{7 \cdot 478}{4 \cdot 553} = 1 \cdot 642 \text{ gallons of proof spirit.}$$

The strength of the spirit, therefore, at 30° is 64·2 overproof.

It should be here pointed out that the diminished bulk of the spirit at 30°, as compared with its bulk at 60°, is exactly compensated, in estimating the equivalent value in proof gallons, by the increased strength at the former temperature; for 100 gallons of spirit 61·5 overproof at 60° contract to 98·33 gallons at 30°; and, reducing to proof strength—

100 × 1·615 = 161·5 gallons of proof spirit,		
98·33 × 1·642 = 161·5	do.	do.

whence it is evident that, by the employment of correct tables of strengths, the estimate of the equivalent value of a given quantity of spirit in gallons of proof spirit would be identical at all degrees of temperature. The spirit tables published by Dr. Derham, to which Sir Henry Roscoe lately called the attention of the Chancellor of the Exchequer, are calculated on this principle.

Dr. Derham also supplies what has long been wanted, a scientific hydrometer having a succession of poises to continue the series the indications of which are also specific gravities. It is well known that, in order to effect this, the increment to the total bulk of the instrument with each successive poise should be the bulk of the graduated stem. Bates's saccharometer is a more or less successful mechanical adaptation of this requirement. But it had escaped previous inventors that, in order to perfectly satisfy the conditions of the problem, the specific gravities of the successive poises should bear an exactly defined relation to the specific gravities to be indicated by the instrument. The principle upon which the calculation of the hydrometer is based is that—

$$\frac{\text{weight}}{\text{bulk}} = \text{specific gravity.}$$

Let W = weight of hydrometer; B = bulk of hydrometer; G = initial specific gravity of the instrument; g = specific gravity of any poise; a = the number of degrees of gravity indicated in the length of the stem; and unity = bulk of graduated stem; then, since the bulks of the poises must be multiples of the bulk of the graduated stem, according to their position in the series,

$$n = \text{bulk of } n\text{th poise.}$$
$$ng = \text{weight of} \quad ,,$$

By the definition of specific gravity,

$$\frac{W}{B} = G\text{; and } \frac{W}{B-1} = G + a,$$

whence

$$W = BG\text{, and } \frac{BG}{B-1} = G + a,$$

and

$$Ba = G + a.$$

Again, generally, with nth poise attached,

$$\frac{BG + ng}{B + n} = G + na,$$

whence

$$g = 2G + (n+1)a.$$

And if the hydrometer were intended to indicate gravities from ·780 to 1·000, the value of the stem being ·020, and the initial specific gravity accordingly of each range ·800, ·820, ·840, &c., the successive specific gravities of the poises would be 1·60, 1·62, 1·64, &c.

THE CŒLOM AND THE VASCULAR SYSTEM OF MOLLUSCA AND ARTHROPODA.[1]

THE object of the author was to establish the fact that the system of blood-containing spaces pervading the body in Mollusca and in Arthropoda was not, as sometimes (and indeed usually) supposed, equivalent to the cœlom or perivisceral space of such animals as the Chætopoda and the Vertebrata, but was in reality a distended and irregularly swollen vascular system—the equivalent of the blood-vascular system of Chætopoda and Vertebrata. Hence he proposed to call the body-spaces of Mollusca and Arthropoda "hæmocœl," in contradistinction to "cœlom." It had been held by previous investigators that in Mollusca and Arthropoda the cœlom and the vascular system were united into one set of spaces—whether by a process of gradual fusion, or owing to the fact that the two systems had never been differentiated from a common original space representing them both in the ancestors of these two great phyla. the author stated that he had been led to the view which he now formulated by his discovery of distinct spaces in both Mollusca and Arthropoda, which appear to be the true cœlom, and are separate from the swollen vascular system.

In Mollusca the pericardial space is the chief representative of cœlom. It is usually taught that the pericardium of Mollusks contains blood, and is in free communication with veins; but the author had succeeded in showing by observations on the red-blooded *Solen legumen* (already published, *Zoolog. Anzeiger*, No. 170, 1884), and by more recent careful investigation of *Anodonta cygnea*, *Patella vulgata*, and *Helix aspersa*, that the pericardium has no communication with the vascular system, and does not contain blood. The perigonadial spaces (so-called generative glands) and the pericardial space (which has arborescent tubular outgrowths in some Lamellibranchs forming Keber's organ) are, then, the cœlom of the Mollusca. It is quite distinct from the hæmocœl. In Cephalopods, and in the archaic Gastropod Neomenia, the pericardial and perigonadial cœlomic remnants are continuous, and form one cavity. There is strong reason to believe that in ancestral Mollusks the hæmocœl was more completely tubular and truly vasiform than it is in living Mollusks. In the later Mollusks the walls of the vessels have swollen out in many regions (especially the veins), and have *obliterated* the cœlom, which has shrunk to the small dimensions of pericardium and perigonadium. There are, however, many Mollusks with complete capillaries, arteries, and veins, in certain regions of the body. These had been recently studied by the author by means of injections, and by silver impregnation, and drawings illustrative of them were exhibited to the Section.

With regard to the Arthropoda, Prof. Lankester formulated the same view, viz. that the ancestral blood-vessels have swollen and enlarged, especially the veins, so as to form large irregular spaces, which have blocked up and so obliterated the previously existing cœlom. Nevertheless the cœlom still persists in some parts of the Arthropod body quite separate from the swollen blood-vascular system. It persists as the tubular generative glands (perigonadium), and also as a system of small spaces (lymph-system) in the connective-tissue of Astacus and of Limulus, and as the internal terminal vesicle of the green glands and other nephridia present in various Arthropoda. Prof. Lankester stated that he had been led to this view with regard to the vascular system and cœlom of the Arthropoda by the results of his histological investigations on the vascular system and connective-tissues of Astacus and Limulus, and by the results obtained in his laboratory by Mr. Gulland in studying the development of the nephridial "coxal gland" of Limulus (already published, with note by Prof. Lankester, in the *Quart. Journ. Micr. Sci.*, 1885, vol. xxv. p. 515). He had also been led to this view by the attempt to explain theoretically the origin of the peculiar structure of the Arthropod's heart and blood-holding pericardium.

The Arthropod's heart and pericardium are absolutely peculiar to the group, and characteristic of all its members—even of Peripatus. The author had asked himself how the existence of a tubular heart with paired valvular apertures in each segment of the body—lying within a blood-holding sac—could be explained. He conceived that it might best be explained by that tendency of the veins to dilate and to form irregular large blood-sinuses, which on other grounds we have reason to consider as a structural tendency of Arthropods. Each pair of valvular apertures in the Arthropod's heart represents a pair of distinct tubular veins which in the ancestors of the Arthropoda brought blood to the heart from the gills. These veins have dilated, and their adjacent walls have been absorbed, so that we now have, instead of a series of veins, a great continuous blood-sinus on each side of the heart or dorsal vessel.

Capillaries of the finest dimensions were shown by Prof. Lankester to exist in certain parts of Astacus and of Limulus. In studying these he had come across the remnants of cœlom. Between the capillaries and unconnected with them—in the connective-tissue of both Astacus and Limulus—is a system of spaces containing a coagulable fluid. (These spaces were described and figured in Limulus in 1884 by Prof. Lankester in the *Quart. Journ. Micr. Sci.*) It is into this system of spaces that the tubular nephridium which becomes the coxal gland of Limulus opens. Hence these spaces are remnants of the cœlom, elsewhere blocked up and obliterated by the swollen veins which form the hæmocœl. The tubular generative glands of Arthropods are to be explained as perigonadial cœlom communicating with the exterior through modified nephridia. Beddard's discovery of such a condition of the ovary and oviduct in the earthworm Eudrilus is confirmatory of this explanation.

The views which had been thus arrived at by Prof. Lankester and very briefly indicated in the note in the *Quart. Journ. Micr. Sci.*, 1885, p. 515, have received a startling and demonstrative confirmation in Sedgwick's brilliant results as to the development of cœlom and hæmocœl in Peripatus, published in the *Quart. Journ. Micr. Sci.*, February 1888, and announced early in 1887 to the Cambridge Philosophical Society.

[1] Abstract of a Paper read in Section D, at the Manchester meeting of the British Association, by Prof. Ray Lankester, F.R.S.

THE TEETH OF THE MYXINOID FISHES.

IN the course of my work upon the morphology of the Vertebrata, it has occurred to me to ascertain how far the generally accepted account of the structure of the teeth in Cyclostomata exhausts the facts at our disposal. The inquiry is one of extreme interest in relation to the disputed affinities of this group with the other fishes. It is well known that Balfour regarded the Myxinoids as the survivors of a very primitive group which had never possessed true jaws. Dohrn, on the other hand, while holding that these fishes retain very many primitive characters, has always asserted their degenerate nature as a canon of his doctrine of the ancestry of Vertebrates. He has endeavoured to produce evidence of this in several of his "Studien," but so far as I am aware, the secondary character of the sucking mouth of the group has never yet been fully proved.

In Balfour's "Comparative Embryology" (vol. ii. p. 264), we read, "I am acquainted with no evidence, embryological or otherwise, that they (the Myxinoid fishes) are degraded gnathostomatous forms."

As the nature of the mouth in this group was one of Balfour's arguments against Dohrn's gill-cleft origin of the mouth of all Vertebrates,[1] and as my own views of the nature of the hypophysis cerebri are also affected by Balfour's reasoning, I may perhaps be allowed to state why I attach great importance to the structure of the teeth in the Myxinoids. With the exception of these animals and Amphioxus, all Vertebrates are known to possess true teeth and true jaws; but it appears to me that if it can be shown that the Myxinoids present traces of true teeth, it must be assumed that they once had true biting jaws. For true teeth are necessary appendages of biting jaws, while they are never found except when true jaws are present. It is important to note that Huxley long ago insisted upon the presence, in the lamprey, of a true mandibular jaw-apparatus, homologous with that of the gnathostomata.

All previous investigators of the group, from Johannes Müller to Parker, have described only the horny nature of the teeth, and that simply because no one has till now made microscopical sections of them. It must here suffice to point out that the current view is correct only so far as the Petromyzontidæ are concerned. They alone possess only horny teeth. In *Petromyzon marinus*, these are curiously complicated, in that they are represented by three horny cusps or thimble-like bodies lying one upon the other, and each arising in a special groove at the base of the tooth. (Prof. Howes writes me that he has long known of this fact.)

Myxine and Bdellostoma, which retain many more primitive characters than the Petromyzontidæ, possess true teeth in the sense of those of other Vertebrates. These are hidden by the aforenamed horny cones, which are formed above them, and, in fact, each horny tooth in these two genera has a true odontoblastic pulp underlying it. The following is a brief description of the appearance of such a tooth in longitudinal section, as exemplified in Bdellostoma. Outside all is the bright yellow horny layer, formed from a "horn groove" at the base of the tooth. Within this is a stratified epithelium, which extends inwards as far as the true tooth; I am unable, however, to find any modified layer of epidermic cells which might represent the so-called enamel organ of other developing teeth. The true tooth is mainly composed of a very hard conical cellular mass, which is probably calcified (I have not yet tested it chemically). It possesses a true pulp-cavity with blood-vessels, &c., while it is made up of cellular elements, which are arranged in a somewhat radiate fashion. The cells are hard, possessed each of a large nucleus longitudinally striated, especially at the apex of the tooth and near the surface.

The apex of the cone is surmounted by a small cap of bright transparent structureless matter, which is either dentine or enamel; from its appearance, and from the fact that the pulp is very hard and obviously calcified, I am inclined to regard it as an enamel structure. While as yet it is not possible to follow the development of this cap, it appears to me to be a secretion of the pulp-cells; and, should it turn out to be enamel, we shall have striking confirmation of the enderonic origin of that layer, advanced by Huxley more than thirty years ago. I, for one, do not believe his view to be so improbable as is generally supposed.

The teeth of Myxine present essentially the same structure as those of Bdellostoma; they are, however, smaller, weaker, and more degenerate, for the cap of enamel (or dentine) is, in them, reduced almost to nothing—indeed, it can only be found after very careful search, and I think that from some of the teeth it is entirely absent.

With this discovery, true teeth come to be characteristic of all the lowest Vertebrates except the outcast Amphioxus, and thus the gulf separating the latter from the former becomes widened. Some zoologists explain the absence of spinal ganglia in Amphioxus by assuming that they are still within the spinal cord: might one hint that they can now also suppose that the teeth of Amphioxus are still within the gums?

In view of the facts here stated it becomes an interesting question for the palæontologist as to how far the "Conodonts" really are the remains of Myxinoid teeth. Zittel's view that they are really Annelidan teeth seems to me the more probable one (*Handbuch der Palæontologie*, Bd. iii. p. 38).

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[1] Amphioxus is here left entirely out of account. Personally, I do not intend to commit myself in seeking to compare any organs of Amphioxus with those of the higher Vertebrates. I would rather leave Amphioxus alone, but I may at least remark the possibility that the mouth in Amphioxus may turn out to be the homologue of the hypophysis—gut passage in Myxine and Bdellostoma. The fact that no hypophysis has yet been discovered in this animal is only in accordance with other negative comparisons between it and other fishes.

MODELS ILLUSTRATING THE MODIFICATION OF THE ARTERIAL ARCHES IN VERTEBRATES.

HAVING recently, with the help of my assistant, made some simple and inexpensive models illustrating the modifications of the arterial arches in Vertebrates, which I find very useful for purposes of demonstration, I send a short description of them to NATURE. Students, as a rule, find it difficult to understand figures of these structures, and a model, in three dimensions, gives a much more accurate idea of their general relations than any drawing can do.

My models are founded mainly on the figures given by Boas, in his paper "Ueber die Arterienbogen der Wirbelthiere" (*Morphol. Jahrbuch*, Band xiii. Heft 1).

The various vessels are represented by stout brass wires (about $\frac{1}{8}$-inch in diameter), bent to the proper form and soldered together; and each model is made, in the first place, to represent six arches. In the case of the fish, the ventral aorta and lower half of each arch (representing the afferent branchial trunk) is painted blue, to indicate that the blood contained therein is venous; the upper half of each arch (representing the efferent trunk), together with the epibranchials and dorsal aorta, are coloured red, to show that they contain arterial blood. The heart is modelled out of modellers' clay, and fixed on to the ventral aorta before being dried; it shows the typical parts of the fish-heart, and is painted blue.

At present I have only made two other models, representing these structures in air-breathing Vertebrates, the types taken being the frog and the mammal. In these, similar colouring is used, but those parts which disappear in the adult are painted *white*. The various parts of the heart are also coloured red or blue, according to the nature of the blood contained in them.

Thus, in the frog the left auricle is red, the right auricle and sinus venosus blue, and the ventricle purple, to show the mixed character of the blood. The first, second, and fifth arches, the portion of the epibranchial between the third and fourth arches, and the ductus Botalli of the sixth arch, are white; the third arch (carotid and lingual artery), red; the fourth (aortic) arch and dorsal aorta, purple; and the lower part of the sixth (pulmonary), blue. In the mammal, the left side of the heart, the left aortic arch, dorsal aorta, and carotids, are red; the right side of the heart, and the pulmonary artery, blue; and the remaining parts, which disappear in the adult, white.

The paint I have used is Aspinall's oxidized enamel.

As this method of illustrating blood-vessels is also particularly useful for lecture-purposes, I intend, later on, to model whole vascular systems in the same way.

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UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—A small revolution has been effected in the teaching of geometry by the adoption of a regulation allowing any proofs of the propositions in Euclid to be given in the "Little-Go" or previous examination. No proof, however, of any proposition occurring in Euclid will be admitted in which use is made of any proposition which in Euclid's order occurs subsequently.

The estimates for the new plant-house (£2760) and research laboratory (£250) at the Botanical Gardens are accepted, Messrs. Boyd, of Paisley, being engaged for the former, Mr. Sindall for the latter. Sir Joseph Hooker, Mr. Thiselton Dyer, and several skilled horticulturists have inspected the plans, and they meet with general approval. The proposed fern-house, stove, and orchid-house, have a combined area of 2660 square feet, as compared with 2290 square feet, the area of the corresponding present houses.

The apparent boycotting of the Cambridge mechanical workshops by the Museums and Lecture-Rooms Syndicate, and other Cambridge authorities has led to a considerable diminution of work, and consequently to a serious reduction of profit in the workshops, which have also suffered to some extent by the unfortunate rejection of the Engineering Tripos scheme. In a recent discussion Prof. Cayley expressed the opinion that it ought to be as much a matter of course to send University mechanical work to the University workshops as to send University printing work to the Pitt Press. He considered the work done by the workshops compared very favourably with similar work done by contractors. Mr. Lyon, superintendent of the workshops, claimed that, while much of the work done outside for the museums had to be frequently repaired, none of the mechanical workshops' work had required this. They had done the work for the Morphological Laboratory for £1000 less than was estimated. A good deal of testimony was given to the excellence of their work, against which it was stated that the Syndicate thought they could get their work done cheaper and better by a professional builder.

A scheme has been prepared for the future fitting up of the old Botanic Gardens site with University buildings in extension of the museums and lecture-rooms. The most salient points are that the site between the new Chemical Laboratory and the Museum of Human Anatomy is declared sufficient for the new Museum of Geology, and that the next buildings to be taken in hand should be those for Human Anatomy and Physiology. It is also proposed to accommodate the Department of Pathology in the old Chemical Laboratory.

Mr. Wilberforce will deliver a course of lectures on Dynamo-Electric Machines at the Cavendish Laboratory during the Easter term.

Among the Fellows elected at King's College last week were Mr. A. P. Laurie, who obtained a first class in the Natural Sciences Tripos, Part II., June 1884, and Mr. H. W. Richmond, Third Wrangler 1885, and placed in Division I. in the third part of the same Tripos, 1886.

Mr. R. Pendlebury, Fellow of St. John's, has been appointed a University Lecturer in Mathematics for five years.

Open Scholarship examinations in which natural science Scholarships may be awarded will be held at Downing College on May 29, and at Peterhouse in October. The Clothworkers' Exhibition in physical science will be competed for in connection with the Oxford and Cambridge schools examination in July.

SCIENTIFIC SERIALS.

IN the *Journal of Botany* for February, Mr. G. S. Boulger calls attention to the exceedingly loose way in which the term "endosperm" is applied by botanical writers to structures in Angiosperms, in Gymnosperms, and in Vascular Cryptogams which have no real homology with one another.—A very interesting new fern from New Guinea (*Polypodium Annabellæ*) is described and figured by Mr. H. O. Forbes, belonging to the small group in which the fertile portion of the frond is only an extension of the lower barren portion.—In this, and in the number for March, Mr. J. G. Baker continues his synopsis of *Tillandsieæ*, and the editor commences an exceedingly useful alphabetical biographical index of British and Irish botanists no longer living.

American Journal of Science, March.—Asa Gray, by J. D. Dana. The attention of the readers of NATURE has already been directed to this memoir, written by the friend and associate probably most competent to appreciate the life-work of the eminent American botanist.—Calibration of an electrometer, by D. W. Shea. In the various forms of the quadrant electrometer, and in the different methods of setting up the same instrument, the curves of calibration obtained are well known to correspond in a very irregular manner with the curves given by Maxwell's mathematical theory. In this paper are given some observations with an electrometer of the Mascart form, which show variations apparently due to change in the sensibility with variation in the temperature. The accompanying tables exhibit the changes in the form of the curves for various charges of the needle through the range of temperature attainable, at the time, in the room where the electrometer was set up.—On the so-called Northford (Maine) meteorite, by F. C. Robinson. One of the numerous specimens of this "meteorite" contained in various cabinets in Maine, and perhaps elsewhere, has recently been analyzed by Mr. Charles Fish in Mr. Robinson's laboratory. That it is not of meteoric origin seems settled by this analysis, which corresponds closely with some recorded analyses of copper-slag.—History of the changes in the Mount Loa craters; Part 1, Kilauea (continued and concluded), by James D. Dana. The subjects discussed in this paper are: the size of the Kilauea conduit; the ordinary work performed by this crater; the kinds and sources of the vapours concerned; the effect of the expansive force of vapours in their escape from the liquid lavas (projectile action), and within the lavas (vesiculation and its mechanical effect); lastly, work of vapours generated outside of the conduit—fractures, displacements, and other results.—The Taconic system of Emmons, and the use of the name Taconic in geological nomenclature, by Charles D. Walcott. In this first paper on the North American Taconic system, the author deals (1) with the Taconic area in general and the geological work within it; (2) with the geology of the Taconic area as known at the present time. The Taconic area, as here studied, is stated to comprise the Taconic range running north and south nearly along the border-line between the States of New York, Vermont, Massachusetts, and Connecticut, with the country immediately adjacent to the range on the east and west. The strata included within the whole area are grouped under six terranes, identified as Middle Cambrian (1 and 5), Upper Cambrian (2), Calciferous, Chazy, and Trenton limestones (3), and Hudson shales, sandstones, &c. (4 and 6).—On the crystalline form of polianite, by E. S. Dana and S. L. Penfield. The true crystalline form of the anhydrous manganese dioxide, MnO_2, from Platten, Bohemia, to which Breithaupt has given the name of polianite, has been the subject of much discussion. Köchlin's recent contribution to its elucidation has induced the authors to continue their own studies, which establish beyond all doubt the independent position of polianite as a tetragonal crystal isomorphous with cassiterite and the allied species of the RO_2 group.

NEARLY the whole of the number of the *Nuovo Giornale Botanico Italiano* for January is occupied by a monograph by Sig. A. N. Berlese of the genus of Fungi *Pleospora*, of which 104 undoubted species are described, several of them new to science, besides a considerable number of doubtful species. The eight plates, in which the essential characters of nearly all the species are illustrated, as well as monographs of the allied genera *Clathrospora* and *Pyrenophora*, are postponed to the next number.—Prof. A. Beccari also describes three new species of palm from New Guinea.

Rendiconti del Reale Istituto Lombardo, February 9.—On colour-hearing, by Tito Vignoli. A somewhat detailed account is given of this obscure psychological phenomenon, cases being described in which not only sound produced the sensation of colour and colour of sound, but also cases in which sensations of smell and taste were stimulated by sound and colour. Rejecting the explanations hitherto advanced, the author refers the phenomenon to the primæval condition of the brain itself before the various senses became differentiated and localized in this organ. These senses must be regarded as so many forms of the primitive and essential condition of the nerve-tissue in which they became gradually specialized. But although the protoplasmic substance of the brain was thus made the seat of distinct sensations by virtue of incident forces and slow selection, still it has never

ceased to possess the aptitude as a whole for receiving all kinds of impressions from without, and in fact it is this general aptitude that has rendered possible the evolution of the special senses in special centres. Thus the common origin of all the senses would seem to offer the readiest explanation of their occasional confusion even in the human brain itself, the highest development of all. Colour-hearing might in this way be regarded somewhat as a case of reversion or atavism.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 23.—"On Remnants or Vestiges of Amphibian and Reptilian Structures found in the Skulls of Birds." By W. K. Parker, F.R.S.

(1) *Jacobson's Organ.*—This remarkable structure, which must be looked upon as an accessory olfactory organ, is present in certain of the higher Vertebrata, or *Amniota.* It consists of a paired cavity, which early becomes separated off from the proper nasal chamber, and which opens into the mouth by the anterior incisive foramen. It is innervated by branches from the olfactory and trigeminal.

Jacobson's organs are largest in Snakes, Lizards, and Monotremes, and next in order come the Marsupials, Edentates, Insectivores, and the Mammalia generally. Their presence in Man is doubtful, and what has been described as a rudiment of them has probably quite another explanation.[1] They are not known to exist in Chelonians, Crocodiles, and Birds.

In the Snake and Lizard, these structures lie each in a little dish, formed by the vomer of that side, covered in by another vomerine bone—the septomaxillary. They are also protected at the opening of the capsule by a pedate tract of cartilage, derived from the alinasal fold, which, in the Snake, frequently becomes detached from its root. In low Mammalia there are several vomers, and in most of the lower Mammals a pair of small anterior vomers lie on the inside of Jacobson's organ, but the capsule itself is formed by a peculiar fold of cartilage—the recurrent cartilage,—which closes in upon itself, and unites its edges round the organ. As a rule, these "recurrent cartilages" retain their union with the alinasal folds, as in the Lizard; in the Rabbit (Howes) they are distinct, as in the Serpent.

Now in Birds these cartilages not unfrequently appear, but no Jacobson's organ has been found with them. The Birds whose vomerine region comes nearest to that of a low Mammal are the Turnicidæ, or Hemipods, and the great group of the Passerine birds (Coracomorphæ, or Ægithognathæ of Huxley). It is not uncommon for the "ox-faced" vomer of these birds to be formed of two pairs of bony centres, and these become not only fused together, but actually grafted upon the floor of the cartilaginous nasal capsule, in the same manner as is common in the lower kinds of Mammalia.

Remnants of the cartilaginous capsule of Jacobson's organs are found not only in the Hemipods and in the lower Neotropical Passerines (*Homorus, Synallaxis, Anæretes*), but also in some of the highest of the singing-birds—namely, the Wren (*Anorthura troglodytes*)—and also in some of the Woodpeckers (Picidæ), outside the Passerine Order.

In a paper on the "Skull in the Ostrich Tribe" (Phil. Trans., 1886, pl. 10, Fig. 14, *a.i.t.*), the present author figured and described, but did not then fully understand, a peculiar cartilage perched right and left upon the large vomer of the *Rhea.* He, however, has for a long time been satisfied that this is one of the vomerine or Jacobson's cartilages, and this view is strongly corroborated by the recent description of the palate of *Apteryx*, given by T. Jeffery Parker (Proc. Roy. Soc., February 23, 1888). Now if the figure of the transversely-vertical section through these cartilages and the crura of the vomer in the *Apteryx*, be compared with various figures in the present author's "Memoirs on the Mammalian Skull" (Parts I., II., and III., "Phil. Trans."), it will be seen that it so nearly corresponds with sections of the skull of the Pig, the Edentates, and the Insectivores, especially those taken just behind Jacobson's organ, that without explanation it would be impossible to tell which figure belonged to the Bird, and which to the Mammal.

(2) *Parasphenoid.*—This bone forms a large superficial basicranial beam in Ganoidei, Teleostei, Dipnoi, and Amphibia. It corresponds to the subcutaneous part of a dermal scute formed inside the skin of the mouth, developed for support to badly ossified endocranium.

The parasphenoid of the Frog is dagger-shaped, and reaches from near the foramen magnum behind, to the nasal capsule in front, the "guard" of the dagger supporting the auditory capsules. Now in Serpents only the *blade* is present; in Lizards only a very fine thread of bone representing the blade; in some, *e.g.* *Trachydosaurus rugosus* (Cyclodontidæ), even this is wanting. It is not present in those very amphibian forms, the Chelonians; and only a small remnant of the "guard" right and left can be found in Crocodiles, consisting of two "basitemporal" plates, soon covered over by the huge pterygoid.

In all Birds basitemporals are large, as large as in Frogs and Toads; this is equally true of the *Dinornis* and of the smallest Humming-bird. There is a tendency for them to break up into lesser bony parts; thus for a day or two in the chick there are two "basitemporal" and one "rostral" centre; but in several species of the Ranidæ, *e.g.* the Bull-frog, the point of the dagger-shaped bone is separately ossified, and remains distinct.

In the Paradoxical Frog (*Pseudis paradoxa*) there is no "handle" to the dagger; the same form of parasphenoid is common among the water-birds, *e.g. Alca, Uria.* This is an ossification which is the earliest to appear in skulls that take on any kind of ossification; it is also the first bone to appear in an embryo bird, as in the larval Frog.

(3) *Prenasal Rostrum.*—Scarcely any Urodeles, and only a few of the Anura, show any special elongation of the "intertrabecula" or prenasal rostral cartilage; this must have been very long in the Ichthyosauria, as in the Selachii, and as in the embryos of all Birds.

(4) *Palato-pterygoid arch or arcade.*—In the Frog, after metamorphosis, during which the hinge of the jaw becomes shifted far backwards, three regions may be distinguished in the forepart of this arch; thus the suspensorial part or pedicle is the ethmo-palatine, the anterior free spike the pre-palatine, and the hinder part which runs into the pterygoid is the post-palatine.

The anterior part of the pterygo-palatine arcade is distinct from the pterygoid in Urodeles, and the pterygoid in them is an outgrowth of the quadrate which grows forwards towards the palatine, but does not coalesce with it, except in *Ranodon sibiricus.*[1] The "post-palatine" tract of cartilage is developed as a distinct nucleus in the Axolotl (*Siredon*).[2]

The only Reptiles in which the author has discovered any distinct trace of the *endoskeletal* palatine is in the Green Turtle, in which it is very small (see *Challenger* Reports, vol. i. part 5, plate 12, Figs. 9, 9*a*, 9*b*: *e.p.a.*).

This endoskeletal cartilaginous palatine, with its peduncle and fore and hind ray or *crus*, appears in several kinds of birds, in addition to their normal *parosteal* palatine—a mere membrane bone, as in Reptiles and Mammals. This vestige or remnant remains in the adult; it is of no apparent use, and occurs in the Families in the oddest way; sometimes, however, it is present in all the members of some particular Family-group, as for instance in the Musophagidæ or plantain-eaters (*Musophaga, Schizorhis*, and *Corythaix*).[3] It is also found in the Oil Bird (*Steatornis caripensis*) and in the Green Tody (*Todus viridis*), and it is also well developed in *Scythrops* (see Linn. Soc. Trans., ser. 2 (Zool.), vol. i. plate 23, Figs. 3 and 4, *o.u.*).

In that nearly extinct Neotropical type, *Steatornis*, this curious partly ossified remnant has the three crura, all well marked, and their morphological meaning is evident; albeit the whole piece is so small and feeble that it can serve no purpose in the solid palate of that remarkable bird.

To show how unexpectedly this remnant exists, a list of the Birds in which it has been found in a segmented state as a distinct bony element of the face is added below; it often shows itself as a mere process of the ecto-ethmoid, but these cases are not included in the list.

[1] See Gegenbaur, "Ueber das Rudiment einer septalen Nasendrüse beim Menschen," *Morphol. Jahrbuch*, Bd. xi., 1885. At the time when the present paper was read, the author was not aware of Gegenbaur's conclusions with regard to the supposed rudiment of Jacobson's organ in Man.

[1] See Wiedersheim, "Kopfskelet der Urodelen," Leipzig, 1877, Plate 5, Figs. 69, 70.

[2] See W. K. Parker, "On the Skull of the Urodeles" (Phil. Trans., 1877, Plate 24, Figs. 1–3).

[3] See Reinhardt, "Om en hidtil ukjendt Knogle i Hovedskallen hos Turakoerne (*Musophagides*, Sundev)," Copenhagen, 1871, Plate 7.

Motacilla yarrelli, *Budytes rayi*. Motacillidæ.
Todus viridis. Todidæ.
Steatornis caripensis. Steatornidæ.
Schizorhis, *Musophaga*, *Corythaix*. Musophagidæ.
Dicholophus. Dicholophidæ.
Procellaria, *Prion*, *Thalassidroma*, *Diomedea*, &c. Procellaridæ.
Larus, var. spec. Laridæ.
Tachypetes. Tachypetidæ.

Another more partial remnant is seen in the Coracomorphæ or Passerine birds generally, which together make up nearly half the number of known birds.

A distinct nucleus representing the post-palatine region of the Frog's skull reappears in the Crow and the Sparrow, and in all the Passerines, as far as they have been worked out. It lies outside the hinder part of the normal parosteal palatine bone, becomes a solid ear-shaped tract of hyaline cartilage, acquires its own osseous (endosteal) centre, and this, when ossified, coalesces with the normal palatine bone.

These facts, and many others that could be mentioned, make it evident that, in seeking for a clue to the uprise of the Feathered Fowl, we may leave out of immediate consideration all the existing types of Reptilia: ancient Amphibians, or Reptiles just rising out of Amphibian lowliness, are the forms that alone will help us in this search. We do get some light upon the Reptilian relationship of Birds, but it is at best a scattered light; the head of a Bird is like that of the *Ichthyosaurus* in its great facial elongation, the neck- and limb-regions of a Bird are those of a *Plesiosaurus*, whilst the hips and legs are like those of the Ornithoscelida.

But these are not all, or nearly all, the vestigial structures that may be seen in the Bird's skull, to say nothing of the skeleton generally;[1] they are sufficient, however, to justify the assumption that Birds arose, by secular transformation, either from the lowest and most ancient of the true Reptiles, or equally with Reptiles from archaic Amphibia, low in structure, but full of potential excellence, and ready, *pro re nata*, to become Reptile, Bird, or even Mammal, as the case might be.

[1] As regards the skeleton of the manus and pes, the indications of at least five carpals (two of these in some types undergoing further subdivision), three small additional rudiments of digital rays in the manus, five tarsals, and a rudiment of the fifth metatarsal, are all important facts bearing upon this subject.

Physical Society, March 10.—Prof. Reinold, President, in the chair.—Mr. G. L. Addenbrooke exhibited and described a compact form of reflecting galvanometer, lamp, and scale, which he has designed as a portable commercial instrument, and also a modified Post Office Wheatstone's bridge.—Mr. E. C. Rimington read a paper on the measurement of the power supplied to the primary coil of a transformer. The first part of the paper contains a proof of a formula given by Prof. Ayrton at a recent meeting of the Society of Telegraph-Engineers for measuring the power given to a transformer by using a Siemens's wattmeter, and the disadvantages of the method are enumerated. A method is then described in which a high-resistance dynamometer is used. One coil of the dynamometer is placed as a shunt to the primary coil, and the other as a shunt to a known inductionless resistance, R, placed in series with the primary. The time constants of the dynamometer coils are made equal by adding an inductionless resistance to the one having the greatest time constant. Thus arranged the difference of phase between the currents in the dynanometer coils is the same as that between the P.D. and current in the primary of the transformer. The mean power, p_m, is shown to be

$$p_m = \frac{K}{R} \delta (1 + \tan^2\phi_1),$$

where $\frac{K}{R}$ is the constant of the dynamometer for watts, δ the reading of the torsion head, and ϕ_1 the lag angle of the currents in the coils of the dynamometer which can be determined from their time constant and periodic time. The best method of arranging the dynamometer in order that R may be as small as possible is discussed. Prof. Ayrton pointed out that the formula first referred to by the author was given to show *why a watt-meter should not be used*, and that the method suggested by Mr. Rimington was a modification of the well-known electrometer method, but with an additional serious objection, that the periodic time must be known. He also described a direct-reading method of using an electrometer, on ordinary transformer circuits, suggested to him by Mr. Sayers. Mr. Blakesley thought the above formula, given by Mr. Rimington, would only be true where there is no iron in the circuit. He described a method of determining the power by observations on two low-resistance dynamometers, one of which is placed in the primary circuit. Of the other dynamometer, one coil is placed in the primary and the other in the secondary circuit. The power is given by

$$p_m = A\alpha_1 r_1 + r_2 \frac{m}{n}, C\alpha_3$$

where r_1, r_2, m, n are the resistances and numbers of convolutions of the primary and secondary coils, A and C the constants of the dynamometers, and α_1, α_3 their reading. A geometrical construction from which the formula is deduced was given. Mr. Sumpner said all the formulæ at present obtained were founded on the assumptions that the induction coefficients of a transformer under working conditions are constant, but, in a paper to be brought before the Society shortly, he hoped to show these assumptions to be erroneous. In replying, Mr. Rimington said, if the periodic time was not known beforehand, it could easily be determined from the note given out by a telephone placed near the transformer.—On the magnetic circuit in dynamo machines, by Prof. W. E. Ayrton and Prof. J. Perry. An abstract was read by Prof. Perry. The authors have worked out a number of formulæ for dynamo machines, involving the thickness, t, of the armature winding, and α the highest permanent current density per square centimetre of cross section of that winding. One of them is

$$W = \frac{2vNt\alpha}{10^8},$$

where W = highest permanent output in watts, v = circumferential velocity, and N = total induction through the armature. As the winding is thin, $t\alpha^2 = q^2$, a constant. For the best modern machines, which do not get too hot, q has a value of about 288. It is shown that the best permanent output is a maximum when the magnetic resistance of the space occupied by the armature winding is equal to all the other magnetic resistance in the circuit, and the best machines are found to satisfy this condition. From this important result the characteristic of such a dynamo can be drawn with considerable accuracy. For small inductions the air resistance only need be considered, and a line drawn on squared paper connecting N and S′A′, satisfying

$$N = \frac{4\pi S'A'}{10} \div \frac{2(d + t)}{a_2},$$

gives the first part of the characteristic, where S′A′ = ampere-turns, d = clearance, and a_2 = the area of the pole pieces exposed to the armature (increased by a fringe of 0·8 $(d + t)$ all round). From the maximum value of N (viz. $a_1\beta_1$) where a_1 = area of diametral section of iron in armature, and β_1 = maximum induction (17,000 to 18,000), find the value of S′A′ from the formula

$$N = \frac{4\pi S'A'}{10} \div \frac{4t}{a_2},$$

and plot the values of N and S′A′ as the co-ordinates of a point. A curve drawn through this point to touch the line first drawn, at a point corresponding with $N = \frac{1}{2}a_1\beta_1$ will not differ materially from the characteristic of the constructed machine.—A note on the employment of an electro-dynamometer for determining the difference of phase of two harmonic currents of electricity, by Mr. T. H. Blakesley, was taken as read. This is a claim of priority for a method published by the author in the *Electrician* of October 2, 1885, which has recently been described and claimed as the invention of Prof. Ferraris, in a paper communicated to the Royal Academy of Science of Turin. In a book on "Alternating Currents," published at the end of 1885, Mr. Blakesley shows how the method can be used for determining induction coefficients and capacities.

Chemical Society, March 1.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—The origin of colour and the constitution of colouring matters, by Prof. H. E. Armstrong, F.R.S. The majority of compounds, especially those of carbon, are colourless; and in the case of elements

whose compounds are invariably coloured, the greatest diversity of colouring is often noticeable among the several compounds of one and the same element—as in those of chromium or manganese, for example: it is therefore clear that colour is in a high degree conditioned by special forms of intramolecular structure, and consequently that any attempt to determine the "origin of colour" must be based on a knowledge of the structure of coloured matters. For this reason it has become possible only within recent years to discuss the relation between colour and constitution, and, so far, the discussion has been limited to two papers by Graebe and Liebermann (*Ber. deut. chem. Gesellsch.*, 1868, 106) and by Witt (*ibid.*, 1876, 522) respectively. To illustrate the idea on which the argument in the paper is based, the author compares the unsaturated hydrocarbons with the paraffins. In the paraffins, which are singularly inert compounds, and all but colourless even in the infra-red and ultra-violet regions of the spectrum, the carbon atoms are united only by single affinities, and the remaining affinities are engaged by *monad* atoms; the unsaturated hydrocarbons, however, are not only more reactive than the paraffins, but the beginnings of colour are manifest in them in regions above and below the visible spectrum, whilst they are conventionally represented by formulæ in which the carbon atoms appear as united by two or three affinities of each, typified by straight lines or dots. Within recent years, however, the idea has found favour that "affinity has direction," and the author would apply this hypothesis to polyad atoms generally; and in formulating compounds in which such atoms are united by more than single affinites, would represent the polyad atoms as united by curved lines in order to suggest that the affinities are under strain in consequence of their being free to act only in certain directions. In the paper, the author cites a number of cases among inorganic compounds which he thinks afford evidence that the production of colour is dependent on special modes of atomic arrangement, and particularly on such modes of arrangement as involve the existence of a condition of strain in the resulting system, due probably to peculiarities in the affinity relationships of the constituent elements of the system which prevent complete mutual neutralization of the affinities. The occurrence of colour therefore is more frequently than not concomitant with a high degree of reactivity, the coloured compound being usually one of "high potential" or slight stability. Among carbon compounds there is no instance of a hydrocarbon being coloured, giving the term its conventional meaning; and omitting nitro-compounds, there are very few exceptions to the rule that derivatives of hydrocarbons containing only monad radicles are colourless; the exceptions, moreover, are of a very noteworthy character, being either *central* derivatives of anthracene, *i.e.* compounds formed by displacement of the hydrogen atoms of the central nucleus of anthracene—which although not coloured is significantly fluorescent; or the monad radicle contains at its origin a radicle such as CO. Attention is then drawn to the quinones and their derivatives, Fittig's ketone formula being throughout adopted for these compounds. The constitution of the better-known dye-stuffs is then discussed, and the author is led to conclusions which in some cases are different from those hitherto accepted; for example, the azo-dyes are formulated $O\smile C_6H_4\smile N.NHR'$ and $HN\smile C_6H_4\smile N.NHR'$; and rosaniline with its congeners, certain of the phthaleïns, and methylene-blue are also formulated on the quinone type. In the discussion on the paper, in which Profs. Debus, Rücker, and Dewar, Dr. Morley and others took part, Prof. G. C. Foster said that it appeared to him that the real question raised by Dr. Armstrong was whether a definite relation could be traced between chemical composition or chemical structure and the existence and position of absorption-bands in the spectrum of the transmitted radiation. The presence or absence of coloration, as it could be judged of directly by the eye, gave no conclusive answer to the question, for a substance might be as colourless as water, and still exert strong absorption in the ultra-red, or it might have strongly-marked absorption in the ultra-violet. But, more than this, a body might exert selective absorption within the visible spectrum, but if it happened to absorb two complementary colours it would be judged of by the eye as though it were destitute of selective absorption altogether. The subject, therefore, seemed to him to involve a systematic study of absorption-spectra.—Researches on chromorganic salts, Part II., by Mr. E. A. Werner.—Note on benzyldithiourethane, by Dr. A. E. Dixon.

Zoological Society, March 6.—Prof. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of February 1888, and called special attention to some examples of a Finch from New Caledonia (*Erythrura psittacea*), and to five specimens of a Pheasant (*Phasianus principalis*) from Afghan Turkistan. The pheasants had been brought home and presented by Major Peacock, R.E., of the Afghan Frontier Commission, at the request of Sir Peter Lumsden, G.C.B., C.S.I.—The Secretary exhibited (on behalf of Lieut.-Colonel H. M. Drummond Hay) a specimen of the Desert Wheatear (*Saxicola deserti*), lately killed in Scotland.—A paper by Prof. G. B. Howes and Mr. W. Ridewood, on the carpus and tarsus of the Anura, was read. The authors recorded observations made upon thirty-seven genera and sixty species, in all stages of development, representatives of all but three or four less important families. The authors were at variance with previous writers in points which had necessitated a reconsideration of the morphological value of the leading elements of both carpus and tarsus. They had failed to discover, at any stage, a trace of a third proximal element in either fore or hind foot, while they showed that Born was in error in regarding the *naviculare* as the prehallux tarsal. In the hind foot they recorded the discovery of a fourth tarsal, and in the fore foot that of a fifth carpal, which latter in *Xenophrys* was bony. Consequent upon this they regarded the element hitherto held to be the fifth carpal as a postaxial *centrale*; whence it followed that the Anura are, as a group, unique in the possession throughout of a double *centrale carpi*. The authors discussed the various changes undergone by the pollux and prehallux, and the several views concerning the morphological value of the latter. A second part was added, in which the peculiarities of the several families of the Anura were given in order, and the bearings of the structures in question upon classification briefly discussed. The Discoglossidæ were shown to combine most completely the least modified conditions of both fore and hind feet.—Mr. R. Bowdler Sharpe read descriptions of new species of birds, of which specimens had lately been received from the Island of Guadalcanar, Solomon Group, collected by Mr. C. M. Woodford. These were named *Astur holomelas*, *Astur woodfordi*, *Astur shebæ*, *Baza guadalcanarensis*, *Ninox granti*, *Graucalus hololius*, *Edoliisoma erythropygium*, and *Pomarea erythrosticta*.—Mr. W. R. Ogilvie Grant contributed a complete list of the birds obtained by Mr. Woodford on the Islands of Guadalcanar and Rubiana. These were altogether sixty-six in number, the new ones being *Nasiterna aolæ*, *Myzolema sharpii*, *Phlegœnas solomonensis*, *Ardeiralla woodfordi*, and *Nycticorax mandibularis*.

Entomological Society, March 7.—Dr. D. Sharp, President, in the chair.—Mr. J. H. Leech exhibited, and made remarks on, a number of butterflies forming part of the collection made for him last summer by Mr. Pratt, at Kiukiang, Central China. The specimens exhibited included *Papilio Macilentus*, hitherto only recorded from Japan, varieties of *P. Sarpedon*, and a supposed new species of *Papilio*; a series of *Sericinus telamon*; *Charaxes narcæus*, and var. *mandarinus*; *Palæonympha opalina*; new species of *Lethe*, *Apatura*, and *Neptis*; and a series of *Argynnis paphia*, with the var. *valezina* of the female. Mr. Leech stated that all the females of *A. paphia* taken at Kiukiang belonged to the var. *valezina*, the typical form of the female being unknown there. Mr. Poulton expressed his interest in Mr. Leech's statement that *valezina* was the only form of the female of *Argynnis paphia* known at Kiukiang, and said he considered this fact would probably throw a new light on the question of the dimorphism of the species. Mr. Jenner-Weir said he had in the course of some years obtained a series of forms intermediate between the typical female and the variety *valezina*. Mr. H. Goss, Dr. Sharp, and Mr. McLachlan, F.R.S., continued the discussion.—Mr. Champion exhibited, for Mr. J. J. Walker, R.N., about 950 species of Coleoptera, recently collected by the latter near Gibraltar. Mr. McLachlan called attention to the large number of water-beetles included in Mr. Walker's collection.—Mr. Verrall exhibited living specimens of *Aspidomorpha sanctæ-crucis*, from the caves of Elephanta.—Mr. Slater exhibited specimens of a species of weevil which had been doing much damage to maize sent to the Colonial Exhibition.—Mr. W. White read a paper entitled "Experiments upon the colour-relation between the pupæ of *Pieris rapæ*, and their immediate surroundings," which comprised a detailed account of a series of observations carried on at the author's instigation by Mr. G. C. Griffiths. The various experiments were intended to act as a test of the conclusions arrived at by Mr.

Poulton in his paper on the subject in the Transactions of the Royal Society; and to effect this object different and additional influences had been brought to bear on these pupæ, so that an analogy might be drawn between the two sets of results. Mr. Poulton, Lord Walsingham, F.R.S., Mr. Jacoby, Dr. Sharp, and Mr. White took part in the discussion which ensued.

PARIS.

Academy of Sciences, March 12.—M. Janssen in the chair.—Remarks accompanying the presentation of the second edition of his "Traité de Physique Mathématique," by M. H. Resal. To this edition have been added sections on mathematical optics and thermodynamics, enlarging the work from one to two volumes.—On the combination of measures of the same magnitude, by M. J. Bertrand. An attempt is here made to estimate the consequences of rejecting measures assumed to be less accurate as departing furthest from the mean in the doctrine of probabilities.—New theory of M. Lœwy's equatorial *coudé* and equatorials in general, by MM. M. Lœwy and P. Puiseux. An improved method is described for more accurately determining the constants both of bent and straight equatorials, with the most rapid processes for mounting and rectifying these instruments.—On phosphorus and phosphoric acid in vegetation, by MM. Berthelot and G. André. As a general result of their experiments, made especially on *Amaranthus caudatus*, the authors find that, after the normal flowering, the employment of phosphorous, and even to some extent of nitrous, manures seems almost, if not altogether, useless, whereas potassic manures may still be advantageously continued as long as vegetation lasts.—Classification of the Gasteropods, based on the various dispositions of the nervous system, by M. H. de Lacaze-Duthiers. This is a purely synthetic treatise, summing up the long and numerous analytical studies on the nervous system of various mollusks, such as Gadinia, Aplysia, Tethys, and many others described in the *Comptes rendus* and elsewhere. The object is to ascertain what data may be supplied by these different types of nervous systems for a physiological classification of the secondary groups of Gasteropods. Two sub-classes with five orders are proposed for the whole class.—On a general theorem of convergence, by M. J. L. Jensen. The studies undertaken by the author with a view to a generalization of the theory of convergence of a series with positive terms have led to an unexpected simplification of the present theory. It is shown that the criteria of Cauchy, of Duhamel and Raabe, of Bertrand, and others, may henceforth be treated summarily as simple corollaries of one general theorem.—On the measurement of magnetic fields by diamagnetic bodies, by M. P. Joubin. The author's renewed attempts to utilize the magnetization of diamagnetic bodies for measuring the intensity of a magnetic field seem to demonstrate the existence of several states of magnetic equilibrium in diamagnetic bodies. This unexpected result is in accordance with theory according to Duhem's calculations, as well as with the general considerations recently set forth by M. Brillouin.—On the magnetization of diamagnetic bodies, by M. P. Duhem. The grounds are explained which render highly probable the existence of several states of magnetic equilibrium for diamagnetic bodies placed in a given position and subjected to the action of given magnets.—A new eolipyle, by M. Paquelin. The apparatus here described has the advantage of working in any position without the risk of explosion, and consumes not more than 90 grammes of fuel in the hour.—Determination in wave-lengths of the two red rays of potassium, by M. H. Deslandres. This determination, made at the request of M. Lecoq de Boisbaudran, yields for the stronger ray 766·30, for the weaker 769·63, giving a mean 767·965, compared with 588·89 of the D_2 sodium ray, which served for the calculation of the constant.—On the decreasing solubility of the sulphates, by M. A. Etard. The sulphates of iron, cadmium, magnesium, lithium, rubidium, and potassium, as well as anhydrous selenious acid, all present the same phenomenon of decreasing solubility. But that of iron, like the previously described sulphate of copper, changes direction twice, first increasing and remaining constant, then decreasing; the complete series of transformations being accomplished between $-2°$ and $+156°$ C.—Action of roasting on several oxides and salts of manganese, by M. Alex. Gorgeu. The anhydrous protoxides heated briskly leave a red oxide; slowly roasted, so as to avoid incandescence, and then kept at a dull red until the weight of the residuum ceases to change, they yield a sesquioxide; lastly, when heated from 200° to 430° C., the oxidation of the MnO obtained at a high temperature is very slow, and appears not to go beyond the manganite $MnO_2 4MnO$, even after forty or fifty hours. Several other details are given of these interesting experiments.—On the collection of star-fish brought to Europe by the French Scientific Mission to Cape Horn, by M. Edmond Perrier. This collection comprises no less than 553 specimens, referred to 38 distinct species, of which 23 are new. This gives to the southern waters of the American continent a total of 57 species of these organisms.—M. J. Kunstler describes a new Foraminifer from the Arcachon basin.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Geological Evidences of Evolution: A. Heilprin (Philadelphia).—Age of Creation: W. J. Cassidy (Briggs, Toronto).—The Geological History of Plants: Sir J. W. Dawson (K. Paul).—A Treatise on Mine Surveying: B. H. Brough (Griffin).—Old and New Astronomy, Part 1: R. A. Proctor (Longmans).—Rainfall in the East Indian Archipelago, 1886: Dr. Van der Stok (Batavia).—Observations made at the Magnetical and Meteorological Observatory at Batavia, vol. ix. 1886: Dr. Van der Stok (Batavia).—Report on the Crops of the Year 1887 (Washington).—London Geological Field Class Reports, 1887 (Philip).—Morphologisches Jahrbuch, Eine Zeitschrift für Anatomie und Entwickelungsgeschichte, xiii. Band, 3 Heft (Leipzig).—Journal of the Chemical Society, March (Gurney and Jackson)—Journal of the Society of Telegraph-Engineers and Electricians, vol. xvii. No. 70 (Spon).—Notes from the Leyden Museum, October 1887 (Leyden).—Archives Italiennes de Biologie, Tome ix. Fasc. 2 (Turin).—Encyklopædia der Naturwissenschaften, Erste Abthg. 54 Lief., Zoologie, &c.; Zweite Abthg. 46 and 47 Lief., Chemie (Breslau).—Bulletin de l'Académie Royale des Sciences de Belgique, 1888, No. 1 (Bruxelles).

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THURSDAY, MARCH 29, 1888.

ELEMENTARY INSTRUCTION IN PRACTICAL BIOLOGY.

A Course of Elementary Instruction in Practical Biology. By T. H. Huxley, LL.D., F.R.S., assisted by H. N. Martin, M.A., M.D., D.Sc., F.R.S. Revised Edition. Extended and Edited by G. B. Howes, Assistant Professor of Zoology, Normal School of Science and Royal School of Mines; and D. H. Scott, M.A., Ph.D., Assistant Professor of Botany, Normal School of Science and Royal School of Mines. With a Preface by Prof. Huxley, F.R.S. (London: Macmillan and Co., 1888.)

THE appearance of the first edition of "A Course of Elementary Instruction in Practical Biology" in 1875 marked an epoch in biological education. The great effects which the doctrine of evolution had been gradually producing in the general system of biological education were then set forth, and widely extended, by means of a clearly written volume containing an account of thirteen types of the organic kingdom. On the appearance of a greatly extended edition of the work, it may not be out of place to say a few words upon the "type-system" of biological education for which the book in its earlier form has done so much. The immense educational success of the work may perhaps be best judged by the fact that, since its publication, an ever-increasing demand has rendered necessary the production of quite a number of new books following the "type-system," and constructed on an identical plan, but dealing with other forms of life.

The important changes in teaching which have followed these publications are seen in the far smaller amount of systematic and classificatory work which is now imposed upon beginners, and its replacement by the acquisition of a thorough knowledge of well-selected types. Remembering that classifications are no more than a condensed abstract of the opinion of the day upon the relative affinities of organic forms, it is clear that no one of the suggested schemes of arrangement can be regarded as final, except as perhaps expressing in the best way the results of a limited state of knowledge. We know that opinion on the subject of affinity has greatly changed in the past, and as long as new facts are revealed by biological research, so long will opinion continue to change in the future. From its necessarily shifting character, and from the fact that the teacher cannot fairly insist upon the accuracy of its conclusions, classificatory biology is eminently unsuited to the needs of a beginner. And there is also another reason, in that classification, if properly taught, is far too advanced a subject to be made an element in early education. If classification is the concise expression of biological opinion, it should nevertheless represent an opinion arrived at after the consideration of *all* the facts and arguments which bear upon the question. The true and only vindication for any suggested modification of existing schemes of affinity must lie in the decided proofs of a better accordance with existing facts. Whoever suggests a modification is under a great responsibility, for, if the alteration is not an improvement, it will certainly be pernicious in adding to our present state of confusion. It is to be hoped that the whole subject will be treated in a more serious spirit in the future than has been accorded to it in the past.

If, then, classification must be dethroned from the high educational position it has held for so long, and which it still maintains to a considerable extent in botanical teaching, what is to be put in its place? Under the type-system a beginner is set to acquire a thorough knowledge of certain central forms of life, each of which is an example of, and a key to, the understanding of an important organic group. At first the types only represent the very largest groups, such as the sub-kingdoms, so that the amount of implied classification is extremely small. As the student progresses, the number of types increases, and the less important organic groups are represented, so that at the end of his course the advanced student finds himself a master of the solid framework of classification, and then the filling in of the details can be carried on in an intelligent and satisfactory manner. It is at this advanced stage of education that advantage can be gained by means of the celebrated "Hunterian system." The comparative study of long series of homologous structures, considered out of relation to the organisms in which they occur, can only confuse the beginner who is not well acquainted with the organisms themselves. But just as the type system prepares the way for, and in fact culminates in, all that is educationally important in classification, so, when a large number of types has been thoroughly learnt, and the varied relations of organ to organ, and of isolated structure to the whole organism, have been grasped in very many instances, then, and not till then, can great advantage be gained by the Hunterian method. And the *extensive* use of this system will be wisely postponed to a very late period; in fact, until the student is beginning to make use of the training which he has received in the wide fields of biological research. The Hunterian system must always form the backbone of a large part of biological research, although it would be most unwise to make it a fundamental part of biological education. It must, however, be conceded that there are certain systems of structures (such as the osteological and dental systems) which especially lend themselves to this mode of teaching, but on account of this very facility such subjects are liable to assume too great a relative importance in biological training.

One incidental, but by no means necessary or even natural, result of the prevalence of the type-system is to be greatly deplored. This result, which is especially found among students of botany, follows from the habit of rejecting the good as well as the bad points in a disused system. Just as the introduction of section-cutting has led to a too great neglect of dissection and the examination of solid structures, so the prevalence of the type-system seems to threaten the existence of the field naturalist and botanist. Those who follow the old, and, upon the whole, the very foolish system of botanical education which a few years ago was the only system taught, have at least one great advantage: they have a keen and intelligent interest in any country walk, while if they possess a little originality and perseverance, they can contribute something towards the solution of some of the most difficult biological problems. But it is not at all uncommon for the successful student of the newer

system to speak of field botany with utter contempt, as a subject unworthy of notice. This is a very unfortunate thing, for there are many most interesting questions which can only be settled by field-observation; and field-observation is in itself a most important, and at the same time a most enjoyable, side of biological training. The same contrast also holds, although to a less extent, on the zoological side. It is much to be hoped that we may be able to correct this great error which has unfortunately attended a healthy, and, upon the whole, highly beneficial educational reaction. It is to be observed that the excellent general descriptions of the types which form so important a feature of the work are in every way calculated to avert this error.

The most striking thing in the revised form of "Practical Biology" is the reversal of the old arrangement, so that the student is now led to begin with a Vertebrate type, and from this to work his way down to the lowest forms of life, and from these, again, upwards to a type of the flowering-plants. There is little doubt that such a change will be met by conflicting criticisms. I believe, however, that the majority of those who have had the widest experience of biological teaching, and especially those who have instructed students in the first use of the microscope, will heartily agree with Prof. Huxley's defence of the alteration, in the preface to the revised edition. The process by which a student first learns to see with the microscope is almost like the education of a new sense-organ suddenly conferred upon a mature organism. We know that under such circumstances it would be a very long time before the impressions conveyed by the new organ could be harmonized with the well-known experiences resulting from the stimulation of other organs. Accustomed to judge of the shapes of objects by their appearance in three dimensions, the student is suddenly provided with a field of vision in which shapes have to be nearly always inferred from the appearance of solid three-dimensional objects when seen under conditions which prevent them from being examined in more than two dimensions at any one time. For it is a long time before the student can accustom himself, by focussing at successive depths, and by making the most of the limited third dimension of depth which the high powers of the microscope provide, to judge accurately of the forms of objects. And the novel conditions under which a student sees with the microscope effectually prevent him from making the best of the impressions he receives. Thus, if the section of a solid object presented the appearance of a circle 1 inch in diameter, and if two other sections at right angles to each other and to the first section presented the appearance of a rectangular figure 3 feet by 1 inch, nearly everyone would readily infer that the shape was that of a cylinder 3 feet long by 1 inch in diameter. But precisely similar data, when presented in the field of the microscope, do not readily lead the student to any definite conclusions as to the forms of objects, and in reality a long course of discipline is necessary in order to make him form any clear conception of the actual shape of the object at which he is looking. I therefore think that it is expedient to begin the course of biological teaching with organisms which only require the use of a microscope for the investigation of part of their structure, and thus to gradually work downwards to the minutest organisms, in which the whole investigation depends upon high microscopic powers. Thus the gradual training in the use of the microscope will proceed parallel with its gradually increasing necessity.

The addition of the earthworm, the snail, and of Spirogyra is a great improvement upon the former edition of the work. If a choice were necessary, the snail is in many respects a more suitable type than the Anodon. In spite of the greater structural simplicity of the latter form, the anatomical details are more difficult to demonstrate by dissection and more difficult to see when dissected than those of the snail. This objection to the Anodon of course only applies to its selection in preference to the snail in the earlier edition; it is in every way desirable that the Lamellibranchs, as well as the Gastropods, should be represented by a well-known type. These newly added types and the additions to the descriptions of those in the previous edition, and to the practical directions, so increase the size of the volume that it contains almost exactly twice the number of pages present in the earlier form of the work. The practical directions given in the appendix appear to be excellent, and to contain in a very small compass an immense amount of information upon the most recent and approved methods. There are a few slips and indefinite statements which should be modified in succeeding editions, which will doubtless be called for at no distant date.

Thus, on p. 383 we are told that *one or two per cent.* of the sugar is unaccounted for in fermentation; but for the rest it is only loosely stated that the *greater part* is resolved into carbonic anhydride and alcohol and *a small part* into glycerine and succinic acid. On pp. 384 and 386 it would be well to represent the numerical proportions of the formulæ by the same method. On p. 462 it is wrongly stated that the cotyledons become green in the type selected. They are in reality hypogeal. On p. 467 no phosphorus is mentioned in the culture solution in which it is stated that the bean-plant will grow. It should read: "potassium phosphate, iron sulphate," instead of "potassium and iron sulphate." In the note on p. 475, "discolour" is used for "decolorize." On p. 483 the student is advised to procure 2 ounces of microscopic slides and half a gross of cover-slips!

Such slight errors can easily be put right, and they would in most cases be detected by the student in reading the book for the first time. They cannot be considered as seriously detracting from so excellent a book, and one which, in the extreme clearness of its style, is so admirably adapted to the needs of the beginner.

E. B. P.

A TEXT-BOOK OF EMBRYOLOGY.

Lehrbuch der Entwickelungs-geschichte des Menschen und der Wirbelthiere. Von Dr. Oscar Hertwig, o.ö. Professor der Anatomie und vergleichenden Anatomie der Universität, Jena. (Jena: Gustav Fischer, 1886.)

THE brothers Hertwig are highly esteemed as original investigators in the field of embryology wherever that science is cultivated. The completion of a systematic work by one of them on the conventional lines of human embryology is therefore a matter of some moment. An

embryologist who bends himself to the requirements of the stereotyped curriculum of human anatomy as demanded alike by German and English directors of medical education, necessarily abandons more or less the consistent scientific treatment of a branch of human knowledge. There is a conventional embryology of the medical school just as there is a conventional anatomy, histology, and physiology, and as there would be a "medical" chemistry, physics, and biology, and a "medical" alphabet, if some professional men in London were to have their own way. Prof. Hertwig has suffered somewhat by submission to these demands. The second half of his work consists chiefly of a description of the development of the various organs, and would more appropriately find its place in that dullest, but most necessary, of treatises—a text-book of human anatomy. The first part, however, is not open to this objection, and even in dealing in the usual way with the development of the organs of the human body in the second half of his work, Prof. Hertwig has managed to bring in a good deal of that scientific interest which is briefly indicated by Goethe's word "morphology." Nevertheless the detachment of the consideration of the mode of origin of the organs of the human body from that of their adult structure and of the structure and development of the same organs in other animals is, in our opinion, an antiquated and mistaken usage, which we are sorry to find so able an author as Prof. Hertwig constrained to follow.

In his first part Prof. Hertwig adopts a more general and truly scientific treatment, and does not distinctly aim at supplementing the work of the topographical anthropotomist. His first chapter is a description of the sexual products, and his consideration is by no means limited to the ovum and spermatozoon of the human species. A comprehensive, though brief, account of the subject with reference to various recent writers is given, and the classification of animal eggs proposed by Balfour is adopted, viz. alecithal, telolecithal, and centrolecithal.

The maturation of the egg and its fertilization are treated in the second chapter, with special reference to the Echinoderma and other Invertebrata, where it has been possible to study this subject with advantage. A third chapter treats of the process of egg-cleavage—the formation by division of the first embryonic cells; a fourth, of the general principles of development—the latter decidedly brief and undeveloped to a degree which is disappointing. Then we come to a chapter on the development of the two primary germ-layers—or on the gastræa theory, as Prof. Hertwig puts it—in which the apparent differences of development of these two layers in various Vertebrata are considered and reconciled, numerous illustrations being introduced into the text, of which a larger number are taken (with ample acknowledgment both in the text and in the special titles of the cuts) from the "Comparative Embryology" of the late Prof. Balfour than from any other source.

The development of the two (parietal and splanchnic) middle germ-layers (cœlom theory) is the next subject of consideration, and is elucidated by a consideration and figures of the process in Sagitta, Amphioxus, Triton, the Mole, &c. The seventh chapter, on the history of the germ-layer theory, is an able and fair statement of the history of embryological doctrine such as every student should be familiar with, and it brings us to the special Hertwigian doctrine of pseudocœl and mesenchyme. The latter is further placed before the reader in the chapter on the development of connective substance and blood. In dealing with the special subject of this book Prof. Hertwig has no occasion to enter upon the question of the pseudocœl—a theoretical conception which, in our opinion, is unnecessary, and not supported by even plausible evidence. The use of the term "mesenchyme" for those cell-elements of the mesoblast layer which lie below the layers immediately bounding the cœlom, and which give rise to connective-tissue and to blood, is, in our opinion, inadvisable. The distinctness which is implied in the use of this term is not, it seems to us, in accordance with the facts of embryology, and we think that embryological appearances may be more correctly stated without introducing the conception of a distinct "mesenchyme," and without postulating a "pseudocœl" in certain Invertebrata, and by adhering to what we may call the "uniformitarian" system, which seeks to explain "pseu docœl" and "mesenchyme" as a special modification of the normal "cœlom" and "mesoblast" respectively—these modifications arising independently under given mechanical conditions in various developmental histories. At the same time, it must be admitted that the attempt to assign a special importance and genetic persistence to "mesenchyme" on the part of the brothers Hertwig has led them to bring many important embryological facts into clear view. The speculations of His as to parablast and archiblast are finally rejected, and a comparatively harmless, though, it would seem, superfluous, theory replaces it.

In the chapter on the primitive segmentation of the body, we come to closer quarters with the ultimate aim of the treatise, viz. the human embryo; and this is followed by chapters on the "Formation of the External Form," and on the "Egg-membranes of Birds and Reptiles," and on the "Egg-membranes of Mammals." These are well illustrated by some of the best amongst already familiar woodcuts (from Balfour, Kölliker, and Turner), and by a coloured plate. At length, in the last chapter of the first portion of his work, Prof. Hertwig brings us to the human interest which has been the motive of all the previous exposition. Here are discussed the "Human Egg-membranes." The medical student is at last rewarded for his patience in wading through the chapters of a scientific treatise, and has the embryo of Allen Thomson, of Coste, and of Krause made clear to him. An excellent account of the structure of the human placenta, accompanied by many woodcuts and by a coloured plate, is given.

Then follows the second "Abtheilung," with its necessarily uninteresting and disjointed account of the development of organs. Whilst recognizing the value, and, in many features, the originality, of this part of the work, we must insist that even so accomplished a writer as Prof. Oscar Hertwig could only do justice to this subject by treating it as part of a comprehensive work on the morphology of Vertebrata, and this the space at his command has not allowed him to attempt. The student will, however, find clear expositions and the latest information on the development of the organs of Vertebrata, with a special reference to the higher Mammalia or man. As an example of the thoroughness with which

Prof. Hertwig has availed himself of the latest inquiries, we may call attention to two figures of the pineal eye of Chameleon and Hatteria, copied from Prof. Baldwin Spencer's memoir in the *Quart. Journ. Micr. Sci.* of last year. Full justice is done by Prof. Hertwig to Mr. Spencer's researches and their significance. We can cordially recommend this text-book of embryology as presenting a decided advance in scope upon the current German treatises on human embryology, one of its merits being that it embodies, among other good things, the teachings and many of the drawings of our "unvergesslicher" Balfour.

E. R. L.

A TREATISE ON ALGEBRA.

A Treatise on Algebra. By C. Smith. (London: Macmillan, 1888.)

THIS, the latest text-book on elementary algebra, is intended for the higher classes of schools and for the junior students in the Universities. The title of the book "A Treatise on Algebra," together with the fact that in the preface the book is affirmed to be complete in itself, is likely to convey the impression that the work is more extensive and ambitious in its scope and design than is really the case. In regard to the matter treated of, it covers much the same ground as Todhunter's "Algebra," which it greatly resembles; it differs from it chiefly in a different arrangement of the parts of the subject, and in the introduction of elementary notions of "elimination" and "determinants."

As regards rearrangement of the subject-matter, there is one very gratifying novelty: before making any use of infinite series, the author introduces a chapter in which he discusses some of the tests of the convergency of such series. There is no doubt of the soundness of this course, and for this single reason many teachers would be inclined to prefer this book to others of the same nature.

The principal feature of modern elementary algebraical text-books seems to be that they are written without any reference to the light shed upon the relative importance of different parts of the subject by the progress of algebraical research. A comprehensive survey of the existing knowledge of the science should induce an author to lead the schoolmaster, and not to follow him. It is not too much to expect that a book like the one under notice should bear some traces of what is taking place in the development of the science to which it seeks to introduce a student. It is perfectly true that certain fundamental notions must necessarily be presented in much the same detail relatively in every book, independently of the date of production; but beyond this an author may easily be too conservative in his ideas to be able to compile a work which shall be of the greatest advantage to a student who intends subsequently to continue his reading at a University or elsewhere. Even from the narrow point of view of an examination it would be advisable to give some small indications of the directions in which explorations have been recently taking place, for it is well known that problem papers at the Universities and elsewhere frequently contain matter taken from researches quite recently published. The absence of modern ideas in a book gives a teacher but little opportunity of pointing out to promising pupils the roads to the frontiers of the science. This is the more to be deplored just now, when a premium is placed at Cambridge upon originality of thought in connection with examinations for Fellowships.

As an instance of what is meant, it may be observed that the subject of "reversion of series" is omitted altogether, although it has of recent years come into great prominence. As a fact, for the last three years one of the chief points of interest in pure mathematics has been Sylvester's theory of reciprocants, which are simply reversion invariants; that is to say, those functions of the coefficients of a convergent series which remain unaltered after the process of reversion has been carried out. One has a right to expect, for this reason, that a "Treatise on Algebra" published at the present time should make some allusion to the existence of such a process; in the older text-books, such as Young's "Course of Mathematics," and the "Algebra" published in Chambers' series, the subject received a special heading, whilst in more recent works it appears merely as an example. The present time is not happily chosen for its complete banishment.

"Scales of notation" give place here to "systems of numeration"; this is in accordance with the German "Zahlensysteme," and seems to be a more suitable nomenclature.

The definitions throughout the book are very carefully given. One or two are open to criticism, as in the case of "cyclical order"; this is defined in reference to a "cyclical change of letters." In modern mathematics this process is termed a "cyclical substitution of the letters," and is one of the fundamental ideas of the extensive "theory of substitutions." There seems to be no good ground for shirking the word "substitution," which fulfils requirements of simplicity and suggestiveness, and is the word with which the student will afterwards become familiar. It seems a pity that in the chapter on permutations the opportunity is not taken to introduce a few of the leading ideas of this theory.

In defining "symmetrical expressions" the author states that an expression which remains unaltered by the "cyclical change" is *also* considered symmetrical; the modern definition of a symmetrical function is that it is such that it remains unaltered when any substitution is impressed upon the letters. The expression $(b-c)(c-a)(a-b)$, instanced by the author as being also called symmetrical, is in reality a two-valued (sometimes called an alternating) function, falls under a different (the alternating) "group of substitutions," and is not properly called symmetrical.

In the chapter on theory of numbers—a particularly clear one—the idea of congruences is happily introduced with Gauss's notation. One would have liked to see also some of the notions of Sylvester's "constructive theory of the partition of numbers," as the ideas are very simple and useful, and moreover algebraically expressible most elegantly. The partition of numbers is rapidly becoming a most important part of the "theory of numbers," a fact which must soon be recognized by authors of books of the same scope as this one.

Other portions of the book which are well presented are "factors" (including many of the first notions of the "theory of equations"), "imaginary and complex quantities," and "binomial theorem."

One would like to see "piles of shot" relegated to the examples, as in these days of rifled guns and elongated projectiles it seems an anachronism. The book is logical, well printed, and illustrated by the best set of examples that can be found in any book of the same kind.

P. A. MACMAHON.

OUR BOOK SHELF.

My Telescope. By a Quekett Club Man. (London: Roper and Drowley, 1888.)

THIS volume is described by its author as a simple introduction to the glories of the heavens. It is not designed as a guide to the use of a telescope, but simply to give such an account of its teachings as may interest non-astronomical readers. The main features of the various celestial bodies are described, but, for some reason or other, comets are not considered at all. Most of the descriptions are very meagre; thus, nebulæ and star-clusters are disposed of in a page, and that not closely printed; even the sun—"the ruler of our system"—is described in a little over three pages. The scantiness of the information given is the greatest fault of the book.

In the little that the book does contain, many mistakes occur. Thus, the moon is stated to present a marbled or mottled appearance because her surface is unequally refractive (p. 62), and the velocity of light is twice put down as 184,000 miles per second (pp. 46 and 72.)

The illustrations are moderate, and the book has a generally neat appearance. The place it is to occupy in astronomical literature, however, is not very clear, as there are already many cheaper books in existence which contain the same information, and much in addition.

Hand-book of Perspective. By Henry A. James, B.A. Cantab. (London: Chapman and Hall, 1888.)

THIS small book contains the principles of perspective explained in a plain and concise way. The author seems to have taken great trouble to make his meaning as clear as possible, and has spared no pains in getting together a good collection of examples, which are all worked out and accompanied in each case by a diagram.

The examples themselves would form a useful and practical course on the subject, since they are arranged in a progressive order, starting with the projection of a single point, and taking up in turn lines, surfaces, and solids.

Beginners will find this volume very serviceable to them, pictures as well as diagrams being given to illustrate the various positions of planes, lines, &c.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Coral Formations.

IN the last paragraph of my letter which appeared in your issue of the 15th inst. (p. 462), I remarked:—"It is quite reasonable to suppose that the dead coral so dissolved in the formation of lagoons is carried out as material for fresh coral growths."

Mr. T. Mellard Reade, in a letter on the same subject, dated 22nd inst. (p. 488), in criticizing the results published by Mr. Ross's letter of the 15th (p. 462), remarks:—"I believe that at no place on the surface of the globe are such dead shells being supplied at a rate that would even balance this supposed rate of chemical destruction."

Can Mr. Reade give any observations or figures in support of his view of the rate of accumulation of oceanic calcareous deposits?

Laying aside all question as to arithmetical error, and without committing myself to the accuracy of Mr. Ross's figures (or even insisting on my own), as to the amount of dead carbonate of lime dissolved in any given time by sea-water in lagoon formation, but taking it as a fact that it is soluble in a marked degree (as is proved by the experiments made by Mr. Ross and myself), and that coral reefs can only exist in regions under the influence of the great warm tropical ocean currents, then we may expect the waters of coral-bearing regions to contain a greater proportion of lime than is found elsewhere, thus forming the calcareous food for continuous extension of the coral formations.

Of course, a distinction must be drawn between the so-called *dead* and living coral, in thus far that the latter is protected from the solvent action of the sea-water by its vitality, while the former, as referred to in my last letter, is peculiarly susceptible to this influence.

ROBERT IRVINE.

Royston, Edinburgh, March 26.

Professor Rosenbusch's Work on Petrology.

READERS of NATURE interested in the study of petrology will be grateful to Dr. Hatch for his lucid review of Rosenbusch's great work, and those who are not able to profit directly by the German original will be glad of the *résumé* given of the latest classification of the massive rocks according to the views of the greatest living authority on the subject.

A translation of Rosenbusch's book into English is much to be desired. Rich as we are in fragmentary literature on the subject, a leading text-book is still wanting. Dr. Hatch would deserve well of his fellow petrologists if he would give them a translation of the work he reviews so well. There might be room for some cutting down in dimensions, especially in the treatment of the "neo-volcanic" rocks. Rosenbusch himself is conscious that this part of the work is, perhaps, a little overloaded with detail, as he says that with a new structure "the scaffolding is not removed before the house is finished," and possibly a competent translator might consider that a little less scaffolding would still be sufficient.

There will no doubt be more or less difference of opinion among authorities as to the correctness of the views which have governed Rosenbusch in his system of rock-classification, especially on one or two points. But none will deny that this classification, with the immense research and study accompanying and supporting it, fully given to the student in this latest work, are a splendid achievement.

Dr. Hatch does well, especially in the interest of younger workers in petrology, to insist on the purely arbitrary nature of *any* system of classification, so far as the separate "rock-types" are concerned; such types passing more or less gradually into others, on either side of them, in all cases. Rosenbusch himself points this out, but a further emphasis of the warning was well in place.

In working with a large text-book like the one in question, with its minute treatment of small details, the student is apt to neglect this consideration of the passage of one rock into another, or at any rate to devote too little attention to it. Nothing, however, could be a greater safeguard to him in this respect than to make for himself a tabular arrangement of Rosenbusch's system, so that the eye can follow in a moment the relationships of the different rock-types to each other. I think it is a pity that such a map, as it were, does not accompany the book. The attentive study of it would not only much assist the worker in his detailed use of the book, but would also greatly aid the beginner to "keep his head level" and steer clear of the sad pitfall of going in too much for "pigeon-holes."

Perhaps an outline table of this sort, which I inclose, may be of a little use to some of your readers, if only to give a compact view of Rosenbusch's classification and of how he connects the main rock-types in series through the four divisions of plutonic rocks, dyke-rocks, palæo-volcanic rocks, and neo-volcanic rock.

An amplified table on the same model, with the various

leading sub-types under each main type, is what I have tried to indicate. Such a table makes clear at once not only the passage of, say, syenites into trachytes, diorites into andesites, but also that of trachytes into andesites, andesites into basalts, &c., &c.

Plutonic Rocks	Dyke Rocks	Effusive Rocks: Palæo-volcanic	Effusive Rocks: Neo-volcanic
Granites	Muscovite-Granite (Aplite) Granite-Porphyries	Quartz-Porphyries	Liparites Pantellerites
Syenites	Syenite-Porphyries Syenitic Lamprophyres	Quartzless-Porphyries	Trachytes Basic Pantellerites
Elæolite Syenites	Elæolite-Porphyries		Phonolites Leucitophyres
Diorites	Diorite-Porphyrites Dioritic Lamprophyres	Porphyrites	Dacites Andesites
Gabbros and Norites		Augite-Porphyrites and Melaphyres	Basalts
Diabases			
Theralites			Tephrites Basanites
Peridotites		Pikrite-Porphyrites	Limburgites Augitites
			Leucite Rocks
			Nepheline Rocks
			Melilite Rocks

A student who has attempted, with plenty of good sections to work on, to draw a definite line between trachytes and andesites, or augite-andesites and basalts, will probably not easily again fall into the error of believing in hard-and-fast types!

Dr. Hatch has at once indicated two main points which will strike petrologists as open to criticism in Rosenbusch's system: the "dyke rocks," and the subdivision of the effusive rocks into "palæo-volcanic" and "neo-volcanic." With regard to the former it deserves to be pointed out that it has not been found possible to classify any representatives of the diabases and gabbros under the head of dyke rocks at all; and one feels that very considerable force has been used, in some other cases, in order to get the rocks under this division into their proper "pigeon-holes."

Something of the same sort of strain and artificiality is felt in some cases with regard to the two divisions of the effusive rocks, and here again it is, perhaps, the equivalents of the diabases and gabbros which most strongly exemplify this feeling—viz. the augite-porphyrites and melaphyres, and the basalts.

Many passages, in several parts of the book, show how fully conscious Rosenbusch is of the weak places there are in his system, as there must always be weak places in every system in a young and rapidly growing science like microscopic petrology. One does not know which most to admire—the wonderfully wide research and knowledge, and the skill and painstaking care and labour, with which the system of classification has been evolved out of the great amount of material to hand, or the great modesty with which it is presented to us.

It will be a thousand pities if a translation of the book does not before very long find its way into the hands of all English-speaking petrologists. A. B.

Manchester, March 19.

"The Mechanics of Machinery."

ANY errors—or what seem such—in a work of so high character and repute as Prof. Kennedy's "Mechanics of Machinery" may be conveniently noticed in NATURE; and therefore no further reason need be offered for the following remarks.

(1) "If, then, a body is moving with a linear velocity of v feet per second about a centre (permanent or virtual) at radius r feet, it is undergoing a radial acceleration of $\frac{v^2}{r}$ foot-seconds per second, and the centrifugal force corresponding to this acceleration will be $\frac{v^2}{r}$ pounds per unit of mass . . . ." (p. 228).

Surely there is some confusion here between the virtual or instantaneous radius, and the radius of curvature; for it is of course the latter which indicates the radial acceleration.

It has always seemed to me that the common expression that a body is turning "about," or, as Prof. Kennedy sometimes puts it, "round," its virtual centre is very apt to mislead. All that we are entitled to say is that at any given instant one point is at rest; and that all other points are moving in directions at right angles to the lines joining them with that point, and with velocities proportional to those lines. This being true also in the simple case of motion in a circle we are apt to use the same language to express it, with the inevitable suggestion of the *curvature* being in correspondence. It need not be said that the curvature may be zero (as when a circle rolls inside one of twice its diameter); or that it may be *away from* the centre (as when the former circle is more than twice the diameter of the second). The beginner would find it hard to realize the latter case when he has been taught to speak of the body "turning round" its virtual centre. I do not know that any better description can be adopted, but it ought to be introduced with emphatic cautions that it is merely a convenient device of description. In any case it seems to me that the old term "instantaneous centre" is more likely to keep the truth before us than the "virtual centre" preferred by Prof. Kennedy.

(2) In a series of interesting discussions about train resistance, the following passage occurs: "The brake resistance is 1200 pounds, but it has to be overcome through a distance π times as great as that moved through by the train as a whole . . . . ;" this brake resistance having been described above as "the frictional resistance at the periphery of each wheel."

I cannot follow this. Surely if the circumference of the wheel be x feet, by the time the train "as a whole" has run over a rail-length of x feet, the brake has slid over a wheel-circumference of x feet also.[1]

This need hardly have been noticed, but that the examples seem always very carefully chosen, not as fancy problems, but as being in accordance with practical experience. Either, therefore, the common impressions as to the time and distance requisite for stopping must be wide of the mark, or the value assumed, in this and a number of other examples, for the brake resistance, must be about three times too small.

(3) "If, for instance, the brakes had not acted promptly, and had been put very hard on at the end, the velocity and acceleration curves might have been as dotted, when the maximum acceleration occurs almost at the end, a state of affairs very uncomfortable for the passengers" (p. 205).[2]

Is this so certain? I mean, is there any reason to suppose that a sudden change of *acceleration*, not of *velocity*—for of this there is no question with any conceivable brake system—would be felt as disagreeable?

The question must, one would think, have often presented itself to speculative and scientific engineers; but as I do not remember to have seen it discussed one would like to get their opinion on the point. The way it strikes me is this. We cannot practically suppose the acceleration *put on* instantaneously, for the brake needs time to work; but we can get as near as Nature allows to the instantaneous when it is *taken off*. That is, if the brakes are left on to the end, the velocity continues to diminish

[1] In the *Errata* we are told to omit this passage; but as the premises and the conclusion, between which it formed the necessary connection, are left unaltered, the need for it is as great as before. How else, but by such an inference, can the conclusion be reached?

[2] The curve of velocity here is a parabola, or nearly so, representing (on a distance scale) a nearly constant "acceleration," with a constantly decreasing velocity.

down to zero, and at the instant the train comes to rest the constant (negative) acceleration abruptly ceases.

How would this show itself in our feelings? If I were sitting with my back to the engine, leaning against the wall of the carriage, I should feel a slight mutual pressure between my back and the wall. This would remain constant to the end, and then abruptly cease. It would be but slight (for any possible brake acceleration must be but a small percentage of that of gravity) in comparison with that which we feel when we lie down. Would its abrupt termination—there being by supposition no sudden change of velocity—be really unpleasant?

To take a fanciful example. If, as I sit in my chair, gravity were suddenly annihilated (for me): I should note the cessation of pressure, and, in so far as my body is elastic, there would be some change of conformation. The pressure in the blood-vessels would also be changed, &c. But, dealing with even such a large acceleration as this, would the instantaneous change be at all comparable with that of a sudden trifling change of *velocity?*

Anyone who has been in a lift at the moment the cord broke might be able to tell us what all this feels like: but he must be careful to distinguish between the sensations due to the first moment of his passage from those due to the last.

Caius College, Cambridge. J. VENN.

The Definition of Force and Newton's Third Law.

PERHAPS your correspondents now engaged in discussing the value of dynamic terms could extend the range of their controversy a little, and deal with a subject of great importance which no text-books touch.

It seems to me that the definition of force as that which causes or alters motion is not reconcilable with Newton's law which asserts that every force is always opposed by an equal and opposite force.

How can a force opposed by an equal and opposite force perform work, or affect the motion of anything? We have here either a fallacy or an indefiniteness, and the matter is worth clearing up because it incessantly worries students who think.

March 23. NEMO.

Green Colouring-matter of Decaying Wood.

ANYONE who lives in a fairly wooded part of the country must be familiar with the fact that at certain stages of decay fallen branches of trees are often to be observed among the dry forest-litter coloured more or less through their tissue with various shades of green. After an examination of thin sections with the microscope, I am unable to trace this to any saprophytic organism. Chemical analysis, on the other hand, reveals the presence of *iron* as the base of the green colouring-matter (using fairly strong nitric acid as a solvent), which—so far as the evidence at present goes—seems to be some organic salt of iron, the organic acid being probably furnished by the slow decomposition of the woody tissue. In the hope that some further light may be thrown on the origin of the green-colouring matter of many Tertiary green earths, I would ask the favour of being allowed to solicit references to any foreign literature of the subject with which any of the numerous readers of NATURE may be acquainted.

A. IRVING.

Wellington College, Berks, March 17.

THE HITTITES, WITH SPECIAL REFERENCE TO VERY RECENT DISCOVERIES.[1]

I.

OF late much has been said concerning the Hittites, and, as might be expected in relation to such a subject, there have been fanciful hypotheses and wild vagaries, repugnant alike to the scientific method and the scientific spirit. But most persons, one would suppose, who have given serious attention to the subject, must have become convinced that there is a great vacancy in that map of the past which ancient history presents. Mighty kings, dynasties enduring, it may be, through thousands of years, great peoples who had made no slight advance in civilization, have passed away without leaving any chronicle equal even to those which were extant concerning Egypt and Assyria before the decipherment of the hieroglyphics and the cuneiform characters. The recent change in public opinion concerning the Hittites is not due merely to the discovery of monuments and inscriptions in various parts of Asia Minor: a large proportion of these had been known to exist for a considerable time, some for a very long time. It must be referred rather to the recognition of an identity or similarity of character in these monuments and inscriptions. And thus has arisen the idea of an empire stretching from the Euphrates to the Ægean Sea. It is, however, doubtful whether—if we use the word "empire" in such a sense as we employ it when we speak, for example, of the Empire of Russia or the Empire of China—there is any ground for believing that a Hittite Empire ever existed. Most likely there were in Asia Minor many States, or even single cities, which were usually to a great extent independent, and the peoples of which were not, perhaps, altogether homogeneous in race, but which, under pressure of the necessities of war, formed a federation. This view accords with the passages in the First and Second Books of Kings which speak of the "kings of the Hittites" (2 Kings, vii. 6) and of "all the kings of the Hittites" for whom Solomon's merchants brought up out of Egypt chariots and horses (1 Kings, x. 28, 29). The testimony of these passages in relation to the greatness of the Hittite peoples has been till recently but little regarded.

That the Hittites thus spoken of in the Old Testament are to be identified with the Khita of the Egyptian monuments, and with the peoples of the land of Khatti in the Assyrian records, is coming out more and more clearly; and as an especial link joining together the peoples thus designated by the Egyptians and Assyrians may be mentioned the city of Carchemish. Holding the Upper Euphrates, the Hittites stood between the Egyptians and the powerful and warlike peoples of Mesopotamia. On a superficial view this may seem not to be the direct route from Egypt to Mesopotamia; but to lead an army by the apparently more direct way across the Syrian desert would have been difficult or wholly impracticable. Moreover, it would not have been easy for an army to make the passage of the Euphrates towards the mouth of the river. But by the upper course of the Euphrates, at or near the site of Jerablûs, the river could be crossed with comparative ease. On the site of Jerablûs (from which the British Museum obtained a few years ago most of the Hittite monuments now in the collection) stood in all probability the renowned city of Carchemish. This identification, attributed to Mr. Skene, was accepted by the late Mr. George Smith, who visited the place shortly before his death. It was this city of Carchemish (not to go back further in Assyrian or Babylonian history) of which Tiglath-Pileser, about 1100 years before Christ, says, "The city of Kargamis, belonging to the country of the Khatti, I smote in one day. Their fighting-men I slew, their movables, their wealth, and their valuables I carried off." He records further that he pursued the portion of the Hittite army which fled; that he crossed the Euphrates in boats covered with bitumenized skins; and that he returned triumphantly to his city of Ashur. The conflict between the Hittites and Assyrians was, however, destined still to continue for 400 years, during which time, though repeatedly sustaining defeat, the Hittites made again and again a determined resistance. It was the fortune of Sargon to end the conflict by the capture, in 717 B.C., of Carchemish and its king, Pisiris.

Previous Egyptian monarchs had engaged in conflict with the Hittites with more or less conspicuous success; but it was the renowned son of Seti, the great Rameses II., about 1330 B.C., whose war with the Khita, and the great battle fought with them at Kadesh, appear to have

[1] Based on Lectures delivered by Mr. Thomas Tyler at the British Museum in January 1888.

been regarded as the most honourable and glorious. This, at any rate, would seem likely, from the care manifested in transmitting to posterity a record of these achievements. The Egyptian laureate, Pentaur, no doubt with a measure of poetical licence suited to his office, described how the Hittite commander, Khita-sar, summoned to the war all the peoples from the uttermost ends of the seas, countless in number, covering mountains and valleys like grasshoppers; and among this multitude were the people of Carchemish. In order that the "sinews of war" might not be deficient, Khita-sar "had not left silver nor gold with his people; he had taken away all their goods and possessions to give to the people who accompanied him." The details of the conflict show a high degree of military organization on the part of the Hittites; and this is in accordance with the position that they had long attained a considerable measure of civilization. According to the Egyptian records, however, they were defeated, and a great part of their army slain, some perishing in the waters of the Orontes, on the banks of which river the battle was fought. It seems sufficiently clear, however, that the Hittites, and "the miserable king of the hostile Khita," as Pentaur calls him, had proved themselves no contemptible foes, and that, defeated though they may have been, their power was very far from being entirely broken. This may be gathered from the treaty of offensive and defensive alliance which was subsequently ratified between Rameses and the Khita. There was to be continual peace and brotherhood; no hostility was ever to arise. Rameses, moreover, eventually married the daughter of the Hittite chief, and made her his queen.

The great sculpture and painting on the walls of the temple at Abu-Simbel, far up the Nile, which represents the war of Rameses with the Khita, and the battle of Kadesh, gives in a point of detail an interesting piece of evidence tending towards the conclusion that the Khita are to be identified with those who sculptured the monuments now known as Hittite. There are depicted on two at least of the monuments in the British Museum, which were obtained from excavations at Jerablûs or Carchemish, heads of kings or other persons in authority bearing the appendage known as the "pig-tail." We are accustomed

1. 2. 3.

FIG. A.—1. "Pig-tail" from Jerablûs monument in the British Museum. 2. Type of head at Eyuk. 3. Head of Khita warrior at Abu-Simbel (after Rosellini).

to associate the "pig-tail" especially with the Chinese, though they derived this mode of head-dress from the Manchu Tartars at a comparatively recent period. And, *primâ facie*, one is not unnaturally inclined to regard the pig-tail on the Jerablûs monuments as having a connection more or less identical—that is to say, a connection with the Manchu Tartars or with some cognate people. The sculptors of the Jerablûs monuments seem to have done their best to show that the pig-tail is a veritable lock of hair, and not a mere appendage of the tall conical cap. On what has been called the doorway inscription in the British Museum, to show that hair is intended, the "pig-tail" is ribbed or marked across; and there is a similar transverse marking of the hair of kings and other persons on the Assyrian monuments. Turning to the great painting at Abu-Simbel, already alluded to, we find that a certain proportion of the Khita warriors are represented as wearing the pig-tail, though this is not the case even with all the kings and princes. A prince, for example, who has fallen into the water of the Orontes, is destitute of this ornament. And even the great Khita-sar, the commander of the Khita, though he had the conical cap, does not seem to have worn the pig-tail.

The indications are clear that the pig-tailed heads on the Jerablûs monuments represent, in accordance with what has been already said, kings or persons of superior dignity. Other heads with ordinary long hair may be taken to be those of persons of inferior rank—subjects or servants. And, in the Abu-Simbel painting, the pig-tailed riders in the chariots are evidently the superiors of the persons beside them wearing long hair. Generally the soldiers with long hair act as shield-bearers or charioteers, while it is the chief warrior who wears the pig-tail.

On two, also, of the bas-reliefs at Eyuk, a place in Asia Minor not far from the River Halys, the wearing of the pig-tail is clearly represented, though the superiority or predominance of the wearers is not equally apparent. On one bas-relief there are six figures, apparently in marching order, all of which probably bore originally the pig-tail, though the monument is now much decayed. It is not, however, very difficult to make out a remarkable Mongolian type of countenance. This is especially to be seen in the figure of a man ascending steps or a ladder, as represented by Perrot and Guillaume ("Exploration Archéologique de la Galatie," plate 62).

Having regard, however, to the monuments from Jerablûs in the Museum, and to the Egyptian painting at Abu-Simbel, the inference seems pretty clear that the wearers of the pig-tail had gained the predominance in some of the Hittite cities, and that they were of a stock different from that of the general population in those particular cities. With the evidence which we at present have, it would be hazardous to say that this was the case in all the Hittite cities. Indeed some facts already alluded to render such a general conclusion extremely improbable.

The general Hittite population was most likely in great part, or principally, Semitic.[1] It is in accordance with this view that their great deity was Set or Sutech—a name repeated ten times, in connection with different cities, in the catalogue of the gods of the land of Khita, in the treaty with Rameses—and the treaty makes mention also of Astartha, or Ashtoreth, as "of the land of Khita"; and here, again, we have unquestionably a Semitic deity. Moreover, of the worship of Ashtoreth there is other important evidence on the Hittite monuments. There are, besides, names of Hittite cities which are unmistakably Semitic; as Carchemish, which can scarcely be explained otherwise than as meaning "the fortress of Chemosh." Then there is Pitru, or Pethor, as well as Hamath and Kadesh. Looking at these names alone, there would be a strong *a priori* probability that the speech of the inhabitants of these cities was Semitic. No doubt there are many names of Hittite persons and places, mentioned in the Egyptian and Assyrian records, with respect to which we must adopt the opinion of Brugsch that they are at least not purely Semitic.[2] The designation of the leader of the Khita or Hittites, Khita-sar, has, it is true, the word *sar*, which is Semitic for "prince," but the Semitic order is reversed. In a purely Semitic formation we should not expect to find "Khita-prince," or "of-the-Khita prince"; the order would probably be the same as ours, "Prince of the Khita." The presence of those wearers of the pig-tail suggests an explanation of the order of the words in Khita-sar, and of

[1] M. Perrot observes:—"Or les Cappadociens, qu'Hérodote appelle Leuco-Syriens ou Syriens blancs, étaient de race sémitique; c'est un fait attesté tout à la fois par les historiens et par le témoignage des médailles, qui nous montrent encore un idiome sémitique parlé au-delà de l'Halys, de Tarse à Sinope, dans la cours même du quatrième siècle avant notre ère" (Perrot et Guillaume, *op. cit.*, vol. i. p. 335). Mr. Pinches, of the British Museum, tells me that several Cappadocian tablets in the cuneiform character have been discovered. Six of these are in the Museum, and one at least is in part Semitic. The others, together with one in the Bibliothèque Nationale, at Paris, have, with one exception, hitherto resisted the attempts at decipherment which have been made.

[2] "History of Egypt," English translation, vol. ii. p. 5.

various other forms, as the Hittite names Pais and Pisiris, and of names ending with the termination *-beg*, as Sathekh-beg, or Suki-beki. The wearing of the pig-tail agrees with the reversal of the order in Khita-sar, since, in accordance with the Mongolian idiom, the order would be reversed; and, having regard to such names as Genghis, Usbeg, &c., there is no difficulty in accounting for the ending in Pisiris and Sathekh-beg.

We may come, then, rationally to the conclusion that men of a race cognate with the Mongols gained the supremacy in some of the Hittite cities; that this ascendancy had its influence on some proper names, and perhaps on other words, but did not change the language of the entire population. If this had been previously Semitic, it remained such. The wearers of the pig-tail did not require their subjects to arrange their hair in the same fashion; and, similarly, they did not attempt to change their language. If they had made the attempt, it would in all probability have been abortive.

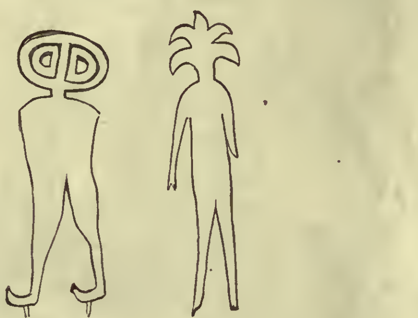
Allusion has been made to the rock-sculptures found at various places through the length and breadth of Asia Minor. Among these, pre-eminence must certainly be assigned to the very remarkable bas-reliefs at Boghaz-Keui, in Cappadocia, a place not far distant from Eyuk, already mentioned, and also near the Halys. Here, where there was, no doubt, originally a chasm or rift in the rocks, closed at one end, the surface of the rocks seems to have been prepared, and a sort of gallery formed. On the two sides of this gallery some sixty or seventy figures have been executed, forming what may be regarded as two processions, which meet on a grand tableau, engraved on the rock at the closed end, in the persons of a male and female figure of much greater height than the rest. Each of these figures seems to be presenting to the other a sort of flower or plant, an arrangement similar to what is to be seen on a seal very lately discovered at Tarsus. The male figure, in the Boghaz-Keui bas-relief, stands on the bended heads of two persons clad in long robes, who in all probability are priests. Each of these figures wears, like that above, a pointed cap, and the curious triangular ornamentation of the skirts of their dresses is very noticeable. The principal male figure has in his hand a sceptre terminating in a ball, and beside him is an animal, said to be a bull, also wearing a pointed cap. If the animal is really a bull, it was probably introduced partly to show by con-

FIG. B.—Central bas-relief at Boghaz-Keui.

trast the relatively gigantic size of the male figure standing beside it. Behind the principal figure is a long procession of some forty other figures, nearly all of which are evidently male, and among them are two winged deities, one of these being apparently the same god that is, or was recently, to be seen on a bas-relief at Jerablûs. Twelve figures in the extreme rear are in the act of running. They have conical caps, but differ from the sceptre-bearing kings, if we may so call them, at the front, in the grand tableau. These hindmost figures we may take to be common soldiers. It is, however, a procession of female figures to the right of a spectator in the rock-gallery to which the principal interest belongs. Each of these, from the more gigantic female figure in the grand tableau to the twentieth in the rear, has on her head a tall cap or crown. This is the so-called "mural crown." In its origin this mural crown probably represented the wall of a city; and the figure bearing it was most likely originally the personification of a city. If, however, this was its origin, it must have become in time diverted from its original use; and, having regard to the male procession, it cannot be regarded as likely that each of the female figures represents a distinct city. Each of these figures has what has been called a *bâton* or stick. But it is very noticeable that this so-called *bâton* is in most, probably in all, cases distinctly curved; a fact which—so it seems to me—probably denotes that it is an unstrung bow. If this is the case, we shall then have a procession of female warriors. Their attire in other respects would be consistent with the idea of their being priestesses, and, if so, the combination of warrior and priestess would precisely accord with one well-known view of the Amazons. It is remarkable, too, that the place where these sculptures are found is not very far from the locality by the River Thermodon and the Black Sea, which the Greeks assigned as the head-quarters of the Amazons. What, then, is the general view to be taken of this remarkable bas-relief? Some have thought that the whole idea is religious, and that at least the two figures meeting in front of the two processions are deities. But how is it, then, that these figures are without wings, seeing that there are winged deities in the male procession? Their mere greatness of size would not show that they are other than persons of kingly and queenly rank. I should not think, however, that the artists who executed this sculpture were commemorating any contemporary event. Probably they were concerned with some notable event in the past, when a king and queen met to ratify an

alliance, or for some other purpose. If the Amazons are, as is commonly thought, merely legendary persons, having no real existence, this sculpture at Boghaz-Keui may yet be looked upon with probability as tending to show that, in what may be called their own country, the story of these female warriors was believed. On the whole, it seems likely, in view of the evidence of this bas-relief, that the story rests on a substantial basis of truth. There is also, I may add, in the Hamath inscriptions, what looks very much indeed like the indication of female warriors armed with club and sword.[1]

Before passing from this Boghaz-Keui sculpture (other interesting bas-reliefs which are there I cannot now discuss), I must refer to the fact that in several instances curious symbols are held in the hands of several of the personages in the processions, or are placed near them. The floral or vegetable symbols held in the hands of the principal personages are surmounted by a remarkable oval figure. This oval object Prof. Sayce regards as a symbol of deity; and the vegetable or other figure beneath he takes for the name of a god. That the name of a god could be indicated by characters such as these seems to me a thing not easily credible; and the inscriptions give very strong reasons for regarding the oval object as a symbol denoting, not deity, but a city.[2] The more probable view seems to be that these figures surmounted by the oval object are the distinctive standards



1. 2.

FIG. C.—1. Standard, with symbol of "city" at Boghaz-Keui. 2. Mandrake after Visconti ("Iconographie Grecque") from manuscript of Dioscorides.

of cities. Unfortunately in the places where these standards occur the sculptures have suffered much from the effects of weather and of time; and the question has been complicated by the differences in the representations given by M. Texier and by MM. Perrot and Guillaume. The representations of the latter, based to a great extent on photographs, are no doubt by far the more accurate. M. Perrot seems to have thought that all these symbols are related to the mandragora or mandrake, a view which I venture to think very improbable. The oval symbol, with its curious marking, is certainly not the fruit of the mandrake, which is round and pendent, not oval and erect. But there is one place in the grand tableau where, I should say, the mandrake is clearly intended.[3] One of these is the symbol borne by the male figure immediately behind what I may call the queen. This figure bears in his hand, as a standard, the mandrake root with the ends turned up into feet. The ancients not only attributed aphrodisiacal and fecundating properties to the mandrake-root, but they also considered that it resembled the body of a man. Pythagoras is said to have spoken of the mandrake as "of human shape." And the difficulty about the feet was easily got over by a little manipulation.[1] There may possibly be some connection between this single male figure with the mandrake standard behind the queen, and what was said by the Greeks as to the relations of the Amazons with the males of a certain city separated from them by a mountain. I should add that the male figure immediately behind the king has the pole of the standard and the oval above, but the intervening figure is gone. It is probably still the standard-pole with the last figure to the reader's left in the central tableau. And possibly, too, at Karabel, the Hittite characters are to be understood as depicted on a standard.

(*To be continued.*)

[1] Under an indication of sex scarcely to be mistaken, is an arm with a hand grasping a club and a sword or dagger.

[2] This was, I believe, the view of Prof. Sayce, before he recognized the Hittite character of the Boghaz-Keui sculptures.

[3] There is evidence also equally clear on the other bas-reliefs at Boghaz-Keui which I do not here discuss.

[1] In the drawing in a manuscript of Dioscorides, of the fifth century, in the library of Vienna, and in Visconti's engraving, the mandrake root is grasped by a female figure. An artist, who is painting the mandrake, is actually accentuating the feet.

TIMBER, AND SOME OF ITS DISEASES.[2]

VI.

If we turn our attention for a moment to the illustrations in the first article, it will be remembered that our typical log of timber was clothed in a sort of jacket termed the cortex, the outer parts of which constitute what is generally known as the bark. This cortical covering is separated from the wood proper by the cambium, and I pointed out (p. 184) that the cells produced by divisions on the outside of the cambium cylinder are employed to add to the cortex.

Now this cortical jacket is a very complicated structure, since it not only consists of numerous elements, differing in different trees, but it also undergoes some very curious changes as the plant grows up into a tree. It is beyond the purpose of these articles to enter in detail into these anatomical matters, however; and I must refer the reader to special text-books for them, simply contenting myself here with general truths which will serve to render clearer certain statements which are to follow.

It is possible to make two generalizations, which apply not only to the illustration (Fig. 20) here selected, but also to most of our timber-trees. In the first place, the cortical jacket, taken as a whole, consists not of rigid lignified elements such as the tracheids and fibres of the wood, but of thin-walled, soft, elastic elements of various kinds, which are easily compressed or displaced, and for the most part easily killed or injured—I say for the most part easily injured, because, as we shall see immediately, a reservation must be made in favour of the outermost tissue, or cork and bark proper, which is by no means so easily destroyed, and acts as a protection to the rest.

The second generalization is, that since the cambium adds new elements to the cortex on the inside of the latter, and since the cambium cylinder as a whole is travelling radially outwards—*i.e.* further from the pith—each year, as follows from its mode of adding the new annual rings of wood on to the exterior of the older ones, it is clear that the cortical jacket as a whole must suffer distension from within, and tend to become too small for the enlarging cylinder of rigid wood and growing cambium combined. Indeed, it is not difficult to see that, unless certain provisions are made for keeping up the continuity of the cortical tissues, they must give way under the pressure from within. As we shall see, such a catastrophe is in part prevented by a very peculiar and efficient process.

Before we can understand this, however, we must take a glance at the structural characters of the whole of this jacket (Fig. 20). While the branch or stem is still young, it may be conveniently considered as consisting of three chief parts.

(1) On the outside is a thin layer of flat, tabular cork-cells (Fig. 20, *Co*), which increase in number by the activity

[2] Continued from p. 279.

of certain layers of cells along a plane parallel to the surface of the stem or branch. These cells (*C.Ca*) behave very much like the proper cambium, only the cells divided off from them do not undergo the profound changes suffered by those which are to become elements of the wood and inner cortex. The cells formed on the outside of the line *C.Ca* in fact simply become cork-cells; while those formed on the inside of the line *C.Ca* become living cells (*Cl*) very like those I am now going to describe.

(2) Inside this cork-forming layer is a mass of soft, thin-walled, "juicy" cells, *pa*, which are all living, and most of which contain granules of chlorophyll, and thus give the green colour to the young cortex—a colour which becomes toned down to various shades of olive, gray, brown, &c., as the layers of cork increase with the age of the part. It is because the corky layers are becoming thicker that the twig passes from green to gray or brown as it grows older. Now these green living cells of the cortex are very important for our purpose, because, since they contain much food-material and soft juicy contents of just the kind to nourish a parasitic fungus, we shall find that, whenever they are exposed by injury, &c., they constitute an important place of weakness—nay, more, various fungi are adapted in most peculiar ways to get at them. Since these cells are for the most part living, and capable of dividing, also, we have to consider the part they play in increasing the extent of the cortex.

(3) The third of the partly natural, partly arbitrary portions into which we are dividing the cortical jacket is found between the green, succulent cells (*pa*) of the cortex proper (which we have just been considering), and the proper cambium, *Ca*, and it may be regarded as

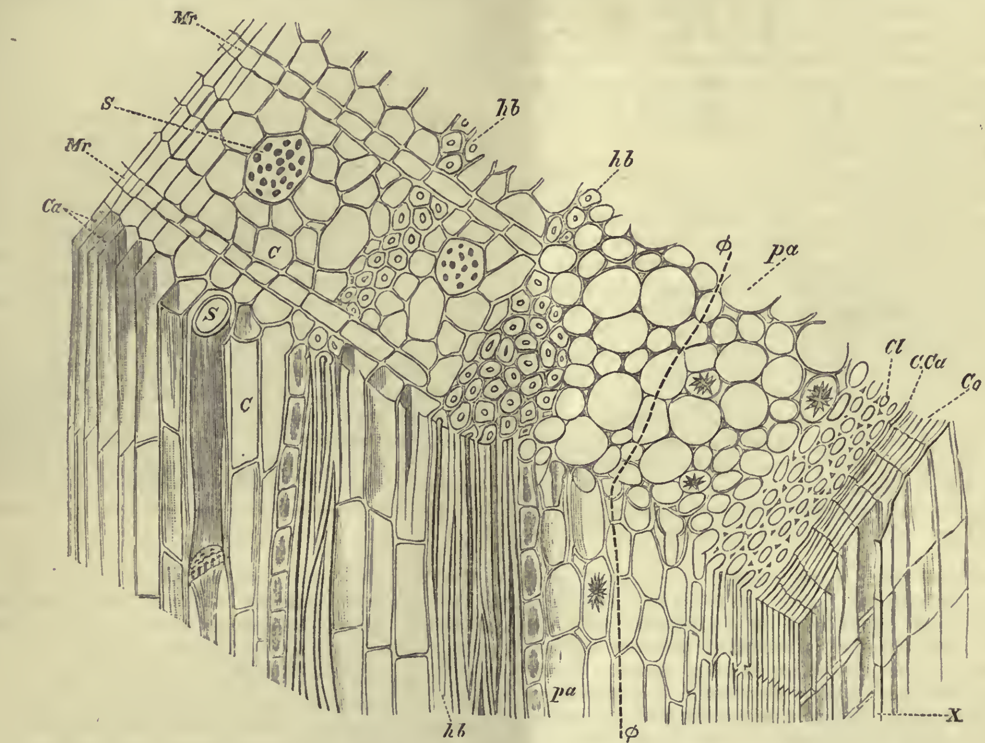


FIG. 20.—A piece of the cambium and cortical jacket of a young oak, at the end of the first year. It may be regarded as consisting of three parts, in addition to the cambium (*Ca*). Beginning from the outside, we have: (1) cork-cells (*X*), formed from the cork-cambium (*C.Ca*): the cells developed on the inside of the latter (*Cl*) are termed collenchyma, and go to add to the cortex. (2) The cortex proper, consisting of parenchyma-cells (*pa*), some of which contain crystals. (3) The inner or secondary cortex (termed *phloem* or bast), developed chiefly by the activity of the cambium (*Ca*): this phloem consists of hard bast fibres (*hb*), sieve-tubes (*S*), and cells (*c*), and is added to internally by the cambium (*Ca*) each year. It is also traversed by medullary-rays (*Mr*), which are continuations of those in the wood. The dotted line (φ) in the cortical parenchyma indicates where the new cork-cambium will be developed: when this is formed, all the tissues (e.g. *pa*, *Cl*), lying on the outside of the new cork will die, and constitute (together with the cork) the true *bark*.

entirely formed directly from the cambium-cells. These latter, developed in smaller numbers on the outside, towards the cortex, than on the inside, towards the wood, undergo somewhat similar changes in shape to those which go to add to the wood, but they show the important differences that their walls remain unlignified, and for the most part very thin and yielding, and retain their living contents. For the rest, we may neglect details and refer to the illustration for further particulars. The tissue in question is marked by *S*, *c*, *hb* in the figure, and is called *phloem* or bast.

A word or two as to the functions of the cortex, though the subject properly demands much longer discussion. It may be looked upon as especially the part through which the valuable substances formed in the leaves are passing in various directions to be used where they are wanted. When we reflect that these substances are the foods from which everything in the tree—new cambium, new roots, buds, flowers, and fruit, &c.—are to be constructed, it becomes clear that if any enemy settles in the cortex and robs it of these substances, it reduces not only the general powers of the tree, but also—and this is the point which especially interests us now—its timber-producing capacity. In the same way, anything which cuts or injures the continuity of the cortical layers results in diverting the nutritive substances into other channels. A very large class of phenomena can be explained if these points are understood, which would be mysterious, or at least obscure, otherwise.

Having now sketched the condition of this cortical jacket when the branch or stem is still young, it will be easy to see broadly what occurs as it thickens with age.

In the first place, it is clear that the continuous sheet of cork (*Co*) must first be extended, and finally ruptured, by the pressure exerted from within: it is true, this layer is very elastic and extensible, and impervious to water or nearly so—in fact it is a thin layer or skin, with properties like those of a bottle cork—but even it must give way as the cylinder goes on expanding, and it cracks and peels off. This would expose the delicate tissues below, if it were not for the fact that another layer of cork has by this time begun to form below the one which is ruptured: a cork-forming layer arises along the line ϕ, and busily produces another sheet of this protective tissue in a plane more or less exactly parallel with the one which is becoming cracked. This new cork-forming tissue behaves as before: the outer cells become cork, the inner ones add to the green succulent parenchyma-cells (*pa*). As years go on, and this layer in its turn splits and peels, others are formed further inwards, and if it is remembered that a layer of cork is particularly impervious to water and air, it is easy to understand that each successive sheet of cork cuts off all the tissues on its exterior from participation in the life processes of the plant: consequently we have a gradually increasing *bark* proper, formed of the accumulated cork-layers and other dead tissues.

A great number of interesting points, important in their proper connections, must be passed over here. Some of these refer to the anatomy of the various "barks"—the word "bark" being commonly used in commerce to mean the whole of the cortical jacket—the places of origin of the cork-layer, and the way in which the true bark peels off: those further interested here may compare the plane, the birch, the Scotch pine, and the elm, for instance, with the oak. Other facts have reference to the chemical and other substances found in the cells of the cortex, and which make "barks" of value commercially. I need only quote the alkaloids in Cinchona, the fibres in the Malvaceæ, the tannin in the oaks, the colouring-matter in *Garcinia* (gamboge), the gutta-percha from *Isonandra*, the ethereal oil of cinnamon, as a few examples in this connection, since our immediate subject does not admit of a detailed treatment of these extremely interesting matters.

The above brief account may suffice to give a general idea of what the cortical jacket covering our timber is, and how it comes about that in the normal case the thickening of the cylinder is rendered possible without exposing the cambium and other delicate tissues: it may also serve to show why bark is so various in composition and other characters. But it is also clear that this jacket of coherent bark, bound together by the elastic sheets of cork, must exert considerable pressure as it reacts on the softer, living, succulent parts of the cortex, trapped as they are between the rigid wood cylinder and the bark; and it is easy to convince ourselves that such is the case. By simply cutting a longitudinal slit through the cortex, down to near the cambium, but taking care not to injure the latter, the following results may be obtained. First, the bark gapes, the raw edges of the wound separating and exposing the tissues below; next, in course of time the raw edges are seen to be healed over with cork—produced by the conversion of the outer cells into cork-cells. As time passes, provided no external interference occurs, the now rounded and somewhat swollen cork-covered edges of the wound will be found closing up again; and sooner or later, depending chiefly on the extent of the wound and the vigour of the tree, the growing lips of the wound will come together and unite completely.

But examination will show that although such a slit-wound is so easily healed over, it has had an effect on the wood. Supposing it has required three years to heal over, it will be found that the new annual rings of wood are a little thicker just below the slit; this is simply because the slit had released the pressure on the cambium. The converse has also been proved to be true—*i.e.* by increasing the pressure on the cambium by means of iron bands, the annual rings below the bands are thinner and denser than elsewhere.

But we have also seen that the cambium is not the only living tissue below the bark: the cortical parenchyma (*pa*), and the cells (*c*) of the inner cortex (technically the phloem) are all living and capable of growth and division, as was described above. The release from pressure affects them also; in fact, the "callus," or cushion of tissue which starts from the lips of the wound and closes it over, simply consists of the rapidly growing and dividing cells of this cortex, *i.e.* the release from pressure enables them to more than catch up the enlarging layer of cortex around the wound.

An elegant and simple instance of this accelerated growth of the cortex and cambium when released from the pressure of other tissues is exhibited in the healing over of the cut ends of a branch, a subject to be dealt with later on; and the whole practice of propagation by slips or cuttings, the renewal of the "bark" of Cinchonas, and other economic processes, depend on these matters.

In anticipation of some points to be explained only if these phenomena are understood, I may simply remark here that, obviously, if some parasite attacks the growing lips of the "callus" as it is trying to cover up the wound, or if the cambium is injured below, the pathological disturbances thus introduced will modify the result: the importance of this will appear when we come to examine certain disturbances which depend upon the attacks of Fungi which settle on these wounds before they are properly healed over. In concluding this brief sketch of a large subject, it may be noted that, generally speaking, what has been stated of branches, &c., is also true of roots; and it is easy to see how the nibbling or gnawing of small animals, the pecking of birds, abrasions, and numerous other things, are so many causes of such wounds in the forest. H. MARSHALL WARD.

(*To be continued.*)

NOTES.

ON Friday, the 23rd inst., Sir Henry Roscoe drew attention in the House of Commons to the Woolwich regulations, the mischievous nature of which we have repeatedly exposed. Sir Henry Roscoe was cordially supported by Sir Lyon Playfair; and Mr. Stanhope, we are glad to say, dealt with the subject in a fair and conciliatory spirit. He promised to discuss the matter with men of science, and the result, we may hope, will be that new regulations will soon be drawn up, securing that scientific candidates shall not be placed at a disadvantage as compared with students of language and literature.

IN replying to Sir Henry Roscoe, Mr. Stanhope made a statement to the effect that the regulations now in force for Sandhurst, which were issued in 1884, were recommended by upwards of fifty head masters. We have before us the Report of the Head Masters' Conference for 1883, in which the recommendations of their Committee are printed, and these recommendations differ in certain important respects from the regulations actually adopted by the War Office. The head masters appear, from their suggestions, to have desired to retain a more important place for Latin and Greek than those subjects occupy in the War Office regulations; and they also attached a higher value to higher mathematics. On the other hand, they placed modern languages and experimental science on a lower but relatively less unequal footing than the War Office has done. Although, therefore, in consequence of the position of classics, the actual value of science was, on the whole, perhaps not better under the head masters' suggestions than under the regulations issued by the War Office, it is hardly correct to speak of the head masters as having recommended a scheme of which the predominating fault is, as

we think, the undue difference in the marks allotted to modern languages as compared with the experimental sciences. Moreover, even if Mr. Stanhope were right, it would not follow that the head masters contemplated the application of their suggestions in all respects to the scientific branches of the Army, since the needs of those branches are so obviously different in certain matters. We quite recognize the necessity pointed out by the Secretary of State for War, that the system of the Royal Military Academy (Woolwich) and that of the Royal Military College (Sandhurst) should be as much as possible assimilated to each other. But for that very reason the needs of the Woolwich cadets in the case of science should have been more carefully considered in the framing of the Sandhurst regulations. No one will contend that scientific capacity is a bad thing for a Sandhurst cadet, and since it is admittedly of direct and very great importance to secure scientific capacity on the part of Woolwich cadets, it appears reasonable that, in the case of science subjects, the needs of the Woolwich system should be chiefly regarded, if in this respect the two systems must be made similar.

TEN Fellows of the Royal Society have died in less than four months—a large number when we take into account that the annual death-rate is barely fifteen. Six out of the ten gave an average age of seventy-nine.

THE annual general meeting of the Chemical Society was held yesterday. Mr. Crookes, F.R.S., the President, read a report on the state of the Society, and an address on elements and meta-elements. The medal founded, for triennial award, by Dr. Longstaff, was conferred on Dr. W. H. Perkin, F.R.S. The President, in presenting the medal, expressed the pleasure he felt in thus testifying, in the name of the Society, to the value of Dr. Perkin's interesting and important researches on the magnetic rotary polarization of compounds in relation to their chemical constitution. Mr. Crookes also took the opportunity of congratulating Dr. Longstaff on the fact that although on the eve of his eighty-ninth birthday he is still hale and hearty.

THE half-yearly general meeting of the Scottish Meteorological Society was held yesterday in the hall of the Royal Scottish Society of Arts, Edinburgh. In their Report the Council state that, in addition to the routine work of the Office, the Secretary's time has been occupied with the preparation of the Report on the Ben Nevis observations from the opening of the Observatory in November 1883 to the end of December 1887, and in seeing these observations through the press. The work, which is to appear as an extra volume of the Transactions of the Royal Society of Edinburgh, is in a state of forwardness, the whole of the observations proper of the Observatory being now through the press. The Council also note that the physical and biological researches have been conducted on the *Medusa* during the winter months with characteristic vigour, and with a success so great as to point to the solution of the questions raised by the herring and salmon fisheries, while at the same time an entirely new light has been cast on the circulation of the water in our fresh and salt water lochs, and generally on the problem of oceanic circulation.

AN influential Committee has been formed at Edinburgh for the purpose of collecting subscriptions for a memorial of the late Prof. Kelland. An appeal has been issued to his former students and others, and it ought to meet with a prompt and generous response. Prof. Kelland, as the Committee remind those whom they have addressed, was not only an excellent and most successful teacher of mathematics, loved and honoured by the students of forty sessions at the University of Edinburgh, but one of the most effective recent promoters of the cause of education in Scotland. The precise nature of the memorial will to some extent depend upon the amount subscribed; but it will be essentially a foundation bearing the name of Kelland, such as a Scholarship, or even a special Lectureship, in connection with the Chair of Mathematics at the University of Edinburgh.

THE Irish Exhibition, which is to be held at Olympia, Kensington, from June 4 next to October 27, ought to be one of very great interest. The intention is that the English public shall have an opportunity of obtaining a clear view of the predominant industries of Ireland. It is also proposed to exhibit some of her historical and antiquarian treasures. The profits are to be given in aid of Irish technical and commercial schools.

THE eighth German Geographentag, which was to have been held at Berlin in April, has been postponed. The Committee, in announcing this decision, explain that the festivities which might, as usual, be connected with the meeting would not be in accordance with the feeling excited in the capital by the death of the Emperor William.

THE *Standard* of Wednesday, March 21, printed some extracts from the letters of Cowper, the poet, which may serve to show that the climate of England has not deteriorated, bad as it has lately been. In a letter dated March 19, 1788, Cowper writes to his friend Bagot:—"The spring is come, but not that spring which our poets have celebrated. So I judge, at least, by the extreme severity of the season—sunless skies and freezing blasts, surpassing all that we experienced in the depth of winter. How do you dispose of yourself in this howling month of March? As for me, I walk daily, be the weather what it may, take bark, and write verses. By the aid of such means as these I combat the north-east wind with some measure of success, and look forward—with the hope of enjoying it—to the warmth of summer." On May 6 he says to Lady Hesketh:—"I am just recovered from a violent cold, attended by a cough. I escaped these tortures all the winter; but whose constitution or what skin can possibly be proof against our vernal breezes in England? Mine never were nor will be." Yet only three weeks afterwards (May 27) he exclaims to the same correspondent:—"How does this hot weather suit thee, my dear, in London? As for me, with all my colonnades and bowers, I am quite oppressed by it." It would be interesting if some one could provide particulars as to the weather of any of the former '88 years.

THE *American Meteorological Journal* for February devotes an article to the works of Prof. William Ferrel, now seventy-one years of age. His first meteorological papers were published in the form of essays in 1856, and were reprinted and extended, under the title of "Motions of Fluids and Solids on the Earth's Surface," as one of the professional papers of the Signal Service. In this paper the explanation of the trade winds is altogether different from that usually given. His latest contribution—"Recent Advances in Meteorology" (see NATURE, vol. xxxvi. p. 255)—is the best summary of the principles and results of meteorology in existence. Ferrel's views received considerable attention in France soon after publication, but in this country and in America they have only attracted notice more recently. His mathematical papers on the motions of the ocean are not less important than those on the motions of the atmosphere. The article is accompanied by a good portrait of Prof. Ferrel.

WE have received from Dr. Van der Stok the rainfall observations made in the East Indian Archipelago during the year 1886. Observations are now taken at 102 stations, several additions having been recently made, including an important station on the Key Islands, in longitude 132° 45′ E.; the district now represented extends over 37° of longitude. The data published include the monthly and yearly values for the year 1886, and the means for a number of years, the number of days of rainfall, and the greatest falls in twenty-four hours. The value of the work

would be much enhanced by the addition of charts showing the distribution in space and time, and by some discussion of the results. It has been shown by M. Woeikof that the rainfall of stations close together differs materially. For instance, at Batavia the proportion of rainfall in the wettest and driest months is 8 to 1, while at Buitenzorg, only 25 miles distant, the proportion is only 2 to 1. The work contains a short account of the position of every station.

THE Central Physical Observatory of St. Petersburg has published a memoir on the rainfall of the Russian Empire (506 pp. 4to) with an atlas. The data used in the calculations are brought down to the year 1882, and include observations taken at 450 places, embracing altogether 3112 years. The tables contain individual monthly and yearly values, and the means for the whole period, rainfall frequency, and maximum falls in twenty-four hours. The influences of the form of gauge and exposure on the amounts of rainfall, and of the method of reckoning days of rain, are fully discussed. The amount taken as representing a rainy day is ·004 inch, while in England it is ·01 inch. It is shown that the difference in the methods of counting rainy days materially interferes with the calculations based upon rainfall frequency.

A SERIES of highly interesting experiments upon the vapour-density of ferric chloride have lately been completed by Drs. Grünewald and Victor Meyer. The chlorides of aluminium and indium have already been shown by Nilson and Pettersson, and by V. and C. Meyer respectively, to possess the molecular formulæ $AlCl_3$ and $InCl_3$; it therefore became most important to determine, if possible, whether the molecule of the corresponding chloride of iron possessed, as has been so generally supposed, the constitution Fe_2Cl_6, or $FeCl_3$. The pure ferric chloride for use in the experiments was obtained by gently heating fine iron wire in a stream of dry chlorine gas and re-subliming the product, thus obtaining the salt in beautiful hexagonal plates, exhibiting a fine green colour by reflected, and a purple tint by transmitted light. The first determination was carried out in a bath of vapour of boiling sulphur (448° C.). At this temperature, the lowest at which vapour-density estimations are possible, the volatilization is very slow, but occurs without the slightest decomposition. And yet the vapour-density obtained was considerably lower than that required by the formula Fe_2Cl_6, showing that at no temperature does ferric chloride possess the molecular formula Fe_2Cl_6, but must of necessity consist of molecules corresponding to the simpler formula $FeCl_3$. On repeating the determinations at higher temperatures in baths of phosphorus pentasulphide (518°) and stannous chloride (606°), and in a platinum apparatus heated in a Perrot furnace to temperatures of 750°, 1050°, and 1300°, the numbers obtained gradually approached the vapour-density of $FeCl_3$, the only unfortunate circumstance being that decomposition into ferrous chloride and chlorine occurred as the temperature was increased. However, on repeating the observations in an atmosphere of chlorine, results almost identical with the former ones were obtained; hence there can be no doubt that the true formula of ferric chloride is not Fe_2Cl_6, but $FeCl_3$. It follows from this as a matter of course that the former view as to the tetrad nature of iron must be laid aside. It will be of great interest, in view of this somewhat unexpected result, to learn the results of the determinations of the vapour-density of the lower chloride of iron, which, we understand from Prof. Meyer, are being undertaken by Profs. Nilson and Pettersson.

A THIRD edition of "Practical Amateur Photography," by Mr. C. C. Vevers, has just been issued. This little manual is intended to serve as a text-book for the beginner and a handy work of reference for the advanced photographer, and care has been taken to make it eminently practical.

AN interesting book on "Tank Angling in India," by Mr. H. Sullivan Thomas, has been published at Madras and in London (Hamilton, Adams, and Co.). Anglers in India will find in the little work—in which there are some fairly good illustrations—an immense amount of information about paste-baiting, live-bait picketing, live-bait with a float, worm and prawn fishing, localities suitable for tank-fishing, stocking ponds, the mainspring of fish-life, and names, description, and habitat of fish.

IN No. 124 of the Proceedings of the Royal Society of Edinburgh (session 1886–87) many valuable papers are printed. Among the contents we may note: the sense of smell, being Part III. of Prof. Haycraft's treatise on "The Objective Cause of Sensation"; on transition resistance at the surface of platinum electrodes, and the action of condensed gaseous films, by Mr. W. Peddie; researches on the problematical organs of the Invertebrata, by Dr. A. B. Griffiths; the salinity and temperature of the Moray Firth, and the Firths of Inverness, Cromarty, and Dornoch, by Dr. H. R. Mill; on the minute oscillations of a uniform flexible chain hung by one end, and on the functions arising in the course of the inquiry, by Dr. E. Sang; notes on the biological tests employed in determining the purity of water, by Dr. A. W. Hare; glories, halos, and coronæ seen from Ben Nevis Observatory, extracts from log-book, by Mr. R. T. Omond; on glories, by Prof. Tait; rectilineal motion of viscous fluid between two parallel planes, by Sir W. Thomson; and the thermal windrose at the Ben Nevis Observatory, by Mr. A. Rankine.

AT a recent meeting of the Wellington (New Zealand) Philosophical Society a paper was read by Mr. E. Tregear on "The Origin of Fire" according to Polynesian folk-lore. Mr. Tregear read from Sir George Grey's work the Maori legend of the procuring of fire from the old fire-goddess Mahuika by the hero Maui who had the power of becoming a bird at will, and compared this with the Samoa version in which the fire-deity is a male person, from whom Maui procures fire, having vanquished him in a personal encounter. In the legend of another of the islands the place of Mahuika as fire-deity is taken by the great Polynesian god Tangaroa. From the fact that in these legends the path by which fire was reached was always downwards into the centre of the earth, Mr. Tregear suggests that it was probable that the ancestors of the Polynesians had experience of natural fire drawn from volcanic sources, but to Maui is due the discovery in Polynesia of fire by friction. With regard to Maui himself there is great difficulty in the parent-names. The assumption by him at will of the form of the dove or of the hawk is consistent with the belief in the ancient world of the various shapes assumed by deities when desirous of accomplishing their purposes. The "seed of fire," an expression used in traditions for the inflammable nature of certain kinds of timber, was a common idiom in ancient Continental nations. Fire-worship continued to have its devotees in Europe until comparatively recent times; and the sacred fire was always "new fire" which had not previously been used for any purpose, being kindled by friction. A legend is preserved in Eastern Polynesia of the descent of the Maori people from a race whose name is the same as that of the fire-kindling instrument used in India, and it is remarkable that the deity who forges the thunderbolts in India is probably identical in name with the thunder-god of the Maoris.

DR. GEORGE M. DAWSON has contributed to the Transactions of the Royal Society of Canada (vol. v. section 2, 1887) a valuable series of notes and observations on the Kwakiool people of Vancouver Island. Referring to the question as to the best means of doing good service to the Kwakiool, Dr. Dawson says it is primarily essential to establish among them

industries which will remove the temptation now felt to drift to the larger settlements and towns. The Kwakiool, with other Indians of the coast, already cultivate in a desultory manner small crops of potatoes, on such minute patches of open ground (generally the sites of old villages) as are to be found along the shore. Their bent, however, is not that of an agricultural people, and the densely-wooded character of their country calls for labour, herculean in proportion to the unsystematic efforts of these people, before it can be cleared and reclaimed for agriculture on any large scale. They are excellent boatmen and fishermen in their own way, and Dr. Dawson has no doubt that under favourable conditions they would readily learn to build boats, make nets, and cure fish in such a manner that the product would be marketable. To effect these objects the most essential step, in Dr. Dawson's opinion, is the establishment of industrial schools, where the younger people may be separated from their old associations and instructed in various callings appropriate to their condition and surroundings.

We have received the Report of the Rugby School Natural History Society for the year 1887. This is the twenty-first issue. The editors point out that owing to various causes a perhaps unusually large number of the meetings were taken up with lectures. Among the contents are two exceptionally interesting papers: one on "Specialization," by Mr. E. Solly, and one on "Natural History in Southern Germany," by Mr. E. E. Austen.

Mr. E. Stanford has issued a pamphlet, by Mr. F. A. Velschow, of Copenhagen, on "The Natural Law of Relation between Rainfall and Vegetable Life, and its Application to Australia." The object of the paper is to show why, in the author's opinion, the regularity of the downpour of rain "depends directly on one particular quality appertaining to vegetable life." He also undertakes to prove that "vapour rarefies the atmosphere instead of increasing its specific gravity, as is now supposed."

The last Calendar of the Imperial University of Japan, to which we have already briefly referred, shows in a very striking manner how the Japanese are beginning to rely upon themselves for instruction in matters relating to higher education. The following figures will show to what extent the Japanese now avail themselves of European assistance in their University:—(1) Law Department: 19 professors, assistants, and lecturers, of whom 5 are foreigners; (2) Department of Medicine: 53 professors and assistants, of whom 2 are foreigners; (3) Engineering Department: 33 professors, assistants, and lecturers, of whom 4 are foreigners; (4) Literature Department: 19 professors and lecturers, of whom 6 are foreigners; (5) Department of Science: 25 professors and assistants, of whom 2 are foreigners. The academical and other qualifications of the Japanese professors and lecturers appear in most cases to be all that could be expected from men holding their positions. The distribution of the foreign professors and lecturers in the University is, we believe, as follows: eight British, eight Germans, two Frenchmen, and one American. Not many years ago a large majority of the *employés* of the University of Tokio, as it was then called, were Americans; and of the present number of foreigners the majority appear to be employed in teaching foreign systems of law and foreign languages. We notice the appointment for the first time of a Professor of Sanitary Engineering, wherein the Japanese University is in advance of nine-tenths of the educational institutions of the West. It is also curious to notice that the Professor of Japanese Philology and Literature is an Englishman.

Ellis's "Irish Education Directory and Scholastic Guide" for 1888 has just been issued. The publication of the volume has been delayed in consequence of the many important changes made in the regulations of the medical licensing bodies in Ireland. During the past year the work has been carefully revised, and it contains full information as to the Irish Universities and professional schools, and the institutions of Ireland for promoting intermediate and primary education. There are also complete alphabetical lists of Irish colleges and schools, and copious alphabetical and classified indices.

The examination papers set in 1887 in connection with the Royal University of Ireland have been published in a separate volume as a supplement to the University Calendar for the year 1888.

According to *Allen's Indian Mail*, the principle of payment by results in the primary schools, has not been altogether successful in India. Last year it proved a failure in Kachar, and this year it is having its final trial in Assam. The teachers in charge of certain selected schools were offered their choice of fixed salaries or payment by results, and if they did not work the latter system successfully they were to revert to the former. The rules for payment by results have recently been revised, larger rewards being offered, and the scheme has been opened to all primary schools.

On Monday a deputation representing Islington, Hackney, and Stoke Newington, waited upon Mr. Anstie, Charity Commissioner, to confer with him about a proposed scheme of Technical Institutes for the North of London. Mr. Anstie was reminded that the Commissioners had suggested to a previous deputation that St. Pancras, Islington, Hackney, and Stoke Newington should combine in order to formulate an educational scheme which would benefit the North of London. Since that time three of the parishes—Hackney, Islington, and Stoke Newington—had met, but St. Pancras had declined to join them, and they now desired to know what assistance they could get out of the City Parochial Funds for their scheme. The deputation said it would be difficult to raise money, but, supposing that they raised £60,000 in Islington and Hackney, could the Charity Commissioners promise them one-half of that amount? Mr. Anstie replied that the Charity Commissioners' proposal to South London was to contribute pound for pound, to be applied rather to permanent endowments than to pay any preliminary expenses. The Commissioners were very anxious in any scheme that was proposed that provision should be made for children between the ages of 13 and 16 to continue their instruction, and they particularly wished to benefit the poorer classes. In fact, they were bound to do so under the provisions of the Act. If St. Pancras stood outside, then the Commissioners would have to treat with the three parishes alone. He urged them very strongly to use every effort to get as much money as possible. The Commissioners would look most favourably upon a scheme which contained in it the promise of the largest contributions.

During the approaching summer a new branch of the London Geological Field Class will make a detailed study of the Chalk formation under the direction of Prof. H. G. Seeley, F.R.S. The other branch under the same direction will follow the course of former years by investigating the principal geological features in the neighbourhood of London. Full particulars can be had by intending students on application to Messrs. G. Philip and Son, 32 Fleet Street, and from many booksellers in the suburbs.

The additions to the Zoological Society's Gardens during the past week include a Shining Parrakeet (*Pyrrhulopsis splendens*) from the Fiji Islands; two Banded Grass Finches (*Poëphila cincta*); two Bicheno's Finches (*Estrelda bichenovii*) from Queensland; two Mandarin Ducks (*Æx galericulata*) from China, purchased; a white-fronted Lemur (*Lemur albifrons* born in the Gardens.

OUR ASTRONOMICAL COLUMN.

The Pulkowa Catalogue of 3542 Stars for 1855.—Dr. Backlund has given in the "Mélanges Mathématiques et Astronomiques" of the St. Petersburg Academy, tome vi., pp. 563–99, a comparison of the star places of the Pulkowa Catalogue of 3542 stars for 1855, with those of the other Pulkowa star catalogues, including the unpublished one for 1875 by Herr Romberg, as well as with the catalogues of Becker, Boss, and Resphigi. This catalogue, which forms part of vol. viii. of the Pulkowa observations, contains the mean places of 3542 stars observed with the meridian-circle of that Observatory during the years 1840–69. It includes all the Bradley stars north of S. Decl. 15°, with the exception of the Pulkowa fundamental stars, which have not been included because the catalogue-places depend on their positions as determined with the transit-instrument and vertical circle. And as the definitive positions of the present catalogue depend on the two catalogues of fundamental stars for 1845 and 1865, it appears that the system of the catalogue for the epoch 1855 will be practically identical with that of the mean of the two catalogues above mentioned. Dr. Backlund's comparisons give a very favourable view of the accuracy with which the relative positions of the stars have been determined, the probable error of an R.A. in the 1855 catalogue being ± 0″·034 for a star south of N. Decl. 30°, and of a declination ± 0″·30; but they also show that the problem of the determination of absolute positions is in a far less satisfactory state. A further comparison of the Pulkowa 1855 catalogue has been made by Herr Seyboth (*Astr. Nach.* 2808), the catalogue with which it has been compared being that of the Cape Observatory for 1880. The agreement appears to be as close as could be expected considering the unfavourable position at either Observatory of several of the stars used; the Pulkowa star places south of the equator show, however, some discordance from those obtained at the Cape as also from those of Boss's standard catalogue. From a comparison of the fundamental stars only, Herr Seyboth finds for the probable error of the Cape places ± 0·021s. in R.A., and ± 0·38s. in Decl.

The Constant of Precession and the Proper Motion of the Solar System.—The completion of Auwers' re-reduction of Bradley's observations, and of the definitive catalogue based thereupon, taken together with the catalogue of 3542 stars and the two fundamental catalogues referred to in the preceding note, has furnished M. L. Struve with the means for a very important investigation, which he has recently published in vol. xxxv. of the *Mémoires* of the Imperial Academy of St. Petersburg. The epoch for Bradley's catalogue being 1755, and the mean date of the three catalogues just mentioned, 1855, M. Struve had at his disposal the places of all Bradley's stars north of S. Decl. 15° for two epochs a century apart. The differences of these places would be due to a combination of three causes: the actual proper motions of the stars, the movement of the solar system, and the error in the constant of precession employed; and they therefore furnish the means of determining both the true value of the precession constant and the direction and rapidity of the motion of translation of the solar system. In his discussion of his materials, M. Struve has followed the method adoped by Sir G. Airy in his treatment of the same problem (Memoirs R.A.S., vol. xxviii.), and determined all his unknowns at the same time. As after excluding all those stars which rest upon but one observation of Bradley, together with a few others omitted for special reasons—seven for their large proper motion,—there still remained 2509 stars, with 2181 proper motions in R.A., and 2345 in Decl., without some method of grouping, the equations of condition would have been too numerous for manipulation. M. Struve has therefore marked off seven zones, each 15° in breadth, and divided these by lines of right ascension into 120 spherical trapezia of nearly equal areas. Each group has been weighted according to the number of the stars it contains, and also according to their magnitudes, so as to reduce the influence of the brighter stars. The solution of the equations of condition by the method of least squares shows very nearly the same correction to the adopted constant of precession,—that of Prof. O. Struve, of 1841, viz. 50″·3798,—from the proper motions in right ascension as from those in declination, the resulting value for the constant being 50″·3514, not so small a value as Nyrén's, 50″·3269, but smaller than those of Bessel, Dreyer, and Bolte. The equating of X, Y, and Z to zero in the normal equations gives a result substantially the same, and proves that the constant can be considered as independent of the motion of the solar system.

For the speed of translation of the solar system, M. Struve finds $q = + 4''\cdot3642$; for the co-ordinates of the apex of the motion, A = 273° 21′, and D = + 27° 19′. Comparing the various determinations which have been made by other astronomers, he is disposed to adopt as a mean position of the apex—A = 266°·7, D = + 31°·0. For q, the displacement of the sun in 100 years as seen from an average star of the sixth magnitude, a number of investigators, O. Struve, Dunkin, Gyldén, and Mädler, have found values not greatly differing from 5″; but others, Ubaghs, Airy, Rancken, and Bischof, obtain very different results, varying from $q = 1''\cdot45$ by Ubaghs, to $q = 49''\cdot5$ by Bischof. Taking 0″·011 as the mean parallax of a star of the sixth magnitude, $q = 5''$ would represent an annual motion of about five radii of the earth's orbit, or a velocity of a little over 15 miles per second. It is clear, however, that we are yet far from being in a position to regard these estimates of velocity as more than provisional.

The chief difficulty in these investigations lies in our ignorance of the actual distances of the stars from us, and even of their relative distances. M. Struve has assumed the following mean values of ρ—the distance of the star from the sun—for each order of magnitude, the sixth magnitude being taken as unity:—

m.		m.	
1	0·13	5	0·70
2	0·23	6	1·00
3	0·36	7	1·49
4	0·51	8	2·25

and adopting 8″ for the secular proper motion of a sixth magnitude star, the scale represents to some extent the proper motions actually observed. M. Struve has also discussed the question of the rotation of the entire sidereal system in the plane of the Milky Way, but his results do not afford any support to the hypothesis, and it has been neglected in the general investigation. At the end of the memoir M. Struve has given the means for the calculation of the general and planetary precessions, together with their secular variations. An appendix furnishes a list of those Bradley stars for which the Pulkowa catalogues gave proper motions sensibly different from those deduced by Auwers from the Greenwich and Berlin observations.

Comet 1888*a* (Sawerthal). — Dr. B. Mattheissen gives (*Astr. Nach.* No. 2830) the following ephemeris from Finlay's elements for this object:—

For Berlin midnight.

1888.	R.A. h. m. s.	Decl. ° ′	Log r.	Log Δ.	Brightness.
April 3	22 13 45	7 35·9 N.	9·8821	0·0728	0·83
5	20 12	9 40·7			
7	26 36	11 39·3	9·9035	0·0934	0·68
9	32 55	13 31·7			
11	39 8	15 18·4	9·9267	0·1134	0·56
13	45 17	16 59·5			
15	51 21	18 35·7	9·9507	0·1328	4·46
17	22 57 19	20 6·9			
19	23 3 11	21 33·5 N.	9·9749	0·1513	0·38

The brightness on February 18 has been taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 APRIL 1–7.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on April 1

Sun rises, 5h. 36m.; souths, 12h. 3m. 45·5s.; sets, 18h. 32m.: right asc. on meridian, 0h. 44·9m.; decl. 4° 49′ N. Sidereal Time at Sunset, 7h. 14m.

Moon (at Last Quarter April 3, 13h.) rises, 23h. 28m.*; souths, 4h. 2m.; sets, 8h. 30m.: right asc. on meridian, 16h. 41·5m.; decl. 17° 59′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	5 0	10 24	15 48	23 5·3	7 48 S.
Venus.....	5 0	10 28	15 56	23 9·1	6 54 S.
Mars	19 34*	0 59	6 24	13 38·1	7 32 S.
Jupiter....	23 25*	3 38	7 51	16 18·1	20 23 S.
Saturn....	11 26	19 25	3 24*	8 7·6	20 49 N.
Uranus ...	18 42*	0 18	5 54	12 57·7	5 25 S.
Neptune..	7 22	15 3	22 44	3 44·9	18 8 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

April.	h.	
1	0	Mercury at greatest distance from the Sun.
2	21	Venus at greatest distance from the Sun.
4	13	Uranus in opposition to the Sun.

Saturn, April 1.—Outer major axis of outer ring = 42″·8; outer minor axis of outer ring = 15″·5: southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′			h. m.	
U Cephei	0 52·4	81 16 N.	Apr. 2,		4 43	*m*
			,, 7,		4 22	*m*
R Lyncis	6 52·1	55 29 N.	,, 2,			*M*
R Canis Minoris	7 2·5	10 12 N.	,, 5,			*m*
R Canis Majoris	7 14·5	16 12 S.	,, 3,		21 15	*m*
S Canis Minoris	7 26·6	8 33 N.	,, 5,			*m*
R Virginis	12 32·8	7 36 N.	,, 7,			*M*
δ Libræ	14 55·0	8 4 S.	,, 3,		23 22	*m*
U Ophiuchi	17 10·9	1 20 N.	,, 1,		1 35	*m*
			,, 6,		2 11	*m*
X Sagittarii	17 40·5	27 47 S.	,, 1,		4 0	*M*
T Herculis	18 4·9	31 0 N.	,, 1,			*m*
β Lyræ	18 46·0	33 14 N.	,, 6,		0 0	m_2
R Lyræ	18 51·9	43 48 N.	,, 2,			*m*
η Aquilæ	19 46·8	0 43 N.	,, 2,		3 0	*M*
T Capricorni	21 15·8	15 38 S.	,, 4,			*M*
δ Cephei	22 25·0	57 51 N.	,, 3,		1 0	*M*
R Lacertæ	22 38·3	41 47 N.	,, 7,			*M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near ν Virginis	175	7 N.	Bright; slow.
,, ε Delphini	305	12 N.	Bright; slow.

THE ROYAL METEOROLOGICAL SOCIETY'S EXHIBITION.

FOR several years past the Royal Meteorological Society has organized an Exhibition of Meteorological Instruments in connection with its ordinary meeting in March. The first Exhibition, which was held in 1880, was of a general character; the subsequent ones, however, have been devoted to the following special subjects, viz. hygrometers, anemometers, travellers' instruments, thermometers, sunshine recorders and radiation instruments, barometers, and marine meteorological instruments.

The subject selected for this year's Exhibition was atmospheric electricity, including new meteorological instruments. The Exhibition was held in the rooms of the Institution of Civil Engineers, 25 Great George Street, Westminster, from March 20 to 23, and was of a most interesting character. The catalogue embraced 155 exhibits, which were arranged under the following heads:—Electrometers, lightning conductors, lightning protectors for telegraph purposes, objects damaged by lightning, &c.; alleged thunderbolts, new instruments, photographs of flashes of lightning, and photographs, drawings, &c.

The Astronomer-Royal exhibited all the apparatus for atmospheric electricity which was formerly in use at the Royal Observatory, Greenwich. These instruments were mounted so as to show the manner in which they were actually arranged for observation.

The Kew Committee also exhibited a number of electrometers which were employed by Sir Francis Ronalds at the Kew Observatory from 1843 to 1851. These are fully described in the Report of the British Association for 1844. Several forms of Thomson's portable electrometer were also shown. The electricity in this instrument is collected by means of a burning fuse at the extremity of a vertical wire. Prof. F. Exner, of Vienna, sent his portable apparatus for the determination of the normal potential in the open air and while travelling.

Numerous patterns of lightning conductors were exhibited by Messrs. J. W. Gray and Son, Messrs. R. Anderson and Co., Messrs. John Davis and Son, and also by the Lightning Rod Conference. Models of churches, houses, chimney-shafts, &c. showed the systems adopted for securing protection from damage by lightning. Messrs. Siemens, Brothers, and Co., also exhibited their apparatus for testing the efficiency of lightning conductors.

The Postal Telegraph Department showed a number of lightning protectors which are used for protecting telegraph instruments.

The Exhibition contained many objects damaged by lightning, including lightning conductors, telegraph instruments and line wire, and portions of trees struck by lightning. The most interesting exhibit, however, was that showing the clothes of a man torn off his body by lightning on June 8, 1878, while standing under a tree. These comprise a flannel jacket, flannel under-vest, trousers, stockings, garters, boots, and watch; also a portion of the bark from the tree.

A valuable collection of meteorites was also shown, the specimens being from various parts of the world, and one showing the Widmanstätten figures. A number of alleged "thunderbolts" were also exhibited. These were of an amusing character; the specimens being in reality nothing more than a large nodule of sandstone, a cannon-ball, a piece of coal, clinkers, &c. Mr. Symons, at the meeting of the Society, showed that these were really of a terrestrial, and not a celestial, nature.

One of the special features of the Exhibition was the very interesting collection of more than fifty photographs of flashes of lightning which have been collected from all parts of the world. These show that lightning does not take the zigzag path as depicted by artists and painters. The lightning really takes a very sinuous and sometimes erratic path. Some of the photographs had been enlarged specially for the Exhibition, and showed up to great advantage. In some cases the photographs showed the lightning to be not merely a line of light, but to have a perceptible breadth, somewhat resembling a piece of tape waved in the air. A large number of the photographs were taken in London during the great thunderstorm of August 17, 1887. One of the photographs taken by Mr. E. S. Shepherd shows the remarkable phenomenon of a *dark* flash.

Mr. Symons exhibited three diagrams of lightning made by Mr. James Nasmyth, F.R.S., in 1856, showing (1) Nature's lightning; (2) painter's lightning; and (3) forked lightning. On comparing these drawings with the photographs of lightning, it is at once apparent what a keen eye Mr. Nasmyth must have had, for the agreement is exceedingly close.

Several new meteorological instruments were exhibited. Mr. W. H. Dines showed a maximum wind pressure anemometer. This has a circular plate, which is always kept face to the wind, at the back of which is a vessel containing shot. The pressure of the wind forces back the plate, and allows shot to fall from the higher to the lower part of the vessel. As soon as the weight of the shot in the lower vessel is equal to the whole pressure on the plate, the plate resumes its normal position, and the opening through which the shot falls is closed. The weight of the shot in the lower vessel gives the maximum pressure since the instrument was last read. Mr. G. H. Larkins showed some rain-band spectroscopes with Tripe's arrangement. The improvements in this instrument over the ordinary form of direct-vision spectroscope are: (1) that it gives uniform light and dispersion, and also better definition of the lines; (2) that as the slit is of uniform width, observations made with this form of instrument are comparable with each other; and (3) that the focussing tube can be fixed by a revolving clamp and kept ready for use.

Dr. Marcet exhibited Prof. Colladon's instrument for illustrating the formation of waterspouts. This consists of a large glass vessel, at the bottom of which has been scattered some dust somewhat heavier than the water. The motion given to a handle turns a wheel which imparts to the water a circular motion. The dust is then drawn up from the bottom in a column, and looks exactly like a waterspout or a sand pillar.

Mr. J. B. Jordan showed one of his new pattern photographic sunshine recorders (in which he now obtains a straight record instead of a curved one), and also that devised by Dr. J. Maurer, of the Swiss Central Observatory, Zurich.

Mr. G. M. Whipple exhibited his repeating cloud camera, which has been designed for obtaining a series of four photographs of the same cloud at short intervals of time, in order to show rapid changes of form.

Numerous photographs of damage by lightning were shown, as well as several records of atmospheric electricity taken at the Greenwich and Kew Observatories during thunderstorms and snowstorms. Messrs. Norman May and Co., exhibited two beautiful photographs taken from the top of the Worcestershire Beacon (1390 feet above sea-level), about 700 feet above the general level of the fog which covered the whole of the surrounding country, on January 12 last. Above the fog there was bright sunshine.

Mr. R. Abercromby and Mons. C. Moussette each exhibited some very fine photographs of clouds; and Mr. J. S. Dyason showed a number of sketches of skies in colour.

WILLIAM MARRIOTT.

THE BOTANICAL DEPARTMENT, NORTHERN INDIA.

UP to the year 1874–75, the Botanical Gardens at Saharanpur and the Botanical Officer in charge of them were Imperial, *i.e.* were under the control of the Supreme Government. In 1875, under the scheme of decentralization by which the independent powers of local Governments were considerably increased, the charge of the Saharanpur (Botanical) Institute became provincial, and passed under the authority of the Lieutenant-Governor of the North-Western Provinces and Oudh.

In 1887 the subject of reorganizing the Botanical Survey in India was taken up in connection with the memorandum, dated February 10, 1885, by Mr. Thiselton Dyer, Director of the Royal Gardens, Kew, and after consultation with the Government of the North-Western Provinces and Oudh and the Superintendents of the Botanical Gardens, Howrah and Saharanpur, the Government of India determined that the most important step which it was desirable to take in order to bring the hitherto unexplored regions of India under botanical survey was to expand the circle, for the botanical investigation of which the Saharanpur Officer was responsible, so as to bring the greater part of Upper India within the sphere of his duties. In order to effect this object it became necessary to restore the Saharanpur Botanist to his former position as an Imperial officer. A further reason for this change was found in the necessity for maintaining, at the disposal of the Government of India, the services of a Botanical Officer with a specially trained staff for the purpose of accompanying expeditions in the neighbourhood of or beyond our north-western frontier. These duties have now been attached to Mr. Duthie, the officer who holds the Saharanpur appointment.

The transfer took effect from April 1, 1887. But the Gardens, with the assistant, Mr. Gollan, who was brought out in 1879, were not placed under Imperial control, but still remained provincial. The Imperial Botanist is therefore now divorced from the Curatorship of the Gardens, which has passed, under the general control of the Director of the Provincial Department of Agriculture, into the hands of the former assistant. The Botanical Officer retains his occupation of the house and Botanical Museum (with the Herbarium), but has no longer any connection with the practical management of the surrounding gardens.

The Botanical Officer retains under his control the whole of the native staff connected with the Museum and its Herbarium, as well as the native artist (now drawing Rs. 100 a month), who was trained at the Bombay School of Art specially for the Saharanpur Institution.

THE NEW SIBERIAN ISLANDS.[1]

THE Expedition of MM. Bunge and Toll, who have explored the lower Yana and the islands of New Siberia during the last two years, was sent out by the Russian Academy of Sciences. Hedenstrom's description of the masses of petrified wood which is found on these islands, and the information gathered from the hunters as to the richness of the archipelago in remains of Quaternary mammals, were the chief inducements for sending out the Expedition.[2] The Expedition consisted of Dr. Bunge, who had just terminated his two years' stay at the Sagastyr Polar station at the mouth of the Lena; Baron Toll; two Cossacks, four Yakuts, and two Tunguses. After having explored the region at the mouth of the Yana during the summer of 1885,[3] and spent the winter at the Kazatchie settlement, twenty miles to the south of Ust-Yansk, the Expedition started in the spring for the New Siberian Islands, and for better exploring them divided into two parties. Dr. Bunge undertook the exploration of the southern islands of the archipelago, and especially of the small Lyakhoff Island, while Baron Toll explored the northern islands (Kotelnyi, Thaddeus, and New Siberia), usually called the Anjou Islands.

Owing to some misunderstanding Dr. Bunge did not find his reindeer on the small Lyakhoff Island, which was his chief station; and, until June 14, he was compelled to limit his explorations to a few excursions only. He saw large flocks of ducks coming from the north—that is, from what is an open sea on our maps, while several species of *Larus* and *Totanus* came from the south. As a rule, few birds cross to Little Lyakhoff Island in their migrations; only geese come by the end of June, and as they moult on the shores of the small lakes and ponds of the island, they are killed in great numbers by the hunters.

The winter lasts on the Little Lyakhoff Island until the first part of June, and returns again in October. On October 16 the frost was already $-37°$ C., but even during the summer 10° C. over zero is considered a very hot temperature; and in July there were thirteen days with snow, fifteen with fog, four with rain, and one snowstorm.

And yet organic life develops with astonishing rapidity. The first flowers were seen on June 11, and Dr. Bunge's collection of phanerogams numbers seventy species; but all plants are dwarfs, hardly reaching a few inches, while the soil, even in the best situated places, thaws only to the depth of 16 inches. The water of the small ponds is so much warmed by the rays of the sun, that temperatures of from 10° to 16° C. were observed, and therefore worms and Crustacea rapidly develop in the ponds. The insects are few; even the mosquitoes do not plague men and cattle as they do on the continent; still, two butterflies were caught. As to mammals, herds of reindeer come every year from the continent to the islands, but in smaller numbers than formerly; they are followed by wolves. The snow fox is very common, but the common fox and hare are exceedingly rare visitors to the islands. The Polar bear has become of late very rare, and hunters attribute this to the fact that the ice has remained unbroken for several yeas past. They affirm that the ice around the coasts has not moved since the year when Nordenskiöld sailed through in the *Vega*, and Dr. Bunge doubts whether it will soon be possible to repeat the same journey.

The chief interest of the island is in its masses of fossil bones buried in the frozen soil. Bones of the mammoth, rhinoceros, *Bos moschiferus*, two other species of *Bos*, several species of *Cervus*, very many bones of *Equus*, and several others, were found, and brought in by Dr. Bunge. The rocks of which the island is built are granite and sedimentary rocks without fossils.

Baron Toll's expedition was much richer in results. It appears from his surveys that the Kotelnyi Island extends much farther east than is shown on our maps, and that it is connected with Thaddeus Island by a sandy beach. It would be most interesting to know how far this circumstance is due to the upheaval of the islands, which is sure to go on like the upheaval of all the northern coast of Siberia. But the most important discovery is, that the masses of fossil wood which were found on Thaddeus Island proved to be Tertiary, and not Quaternary, as has hitherto been supposed. They belong to layers of Tertiary coal, and fossil *Sequoiæ* were found amidst them. We have thus a new proof that the great Tertiary continent which possessed a warm climate, well known from Oswald Heer's description, included not only Greenland, Spitzbergen, and Novaya Zemlya, but also the New Siberian Archipelago, more than 90° of longitude to the east of Novaya Zemlya. Geology must explain the existence of this warm climate beyond the 75th degree of latitude, at a period so closely preceding that of the glaciation of the northern hemisphere.

Finally, Baron Toll, after having made rich zoological, botanical, and palæontological collections—Silurian, Devonian, and Triassic deposits being found on the Kotelnyi Island,—reached the northern extremity of the island under the 76th degree of latitude, and thence he saw the land which was seen eighty years ago by Sannikoff, and has since periodically appeared on, and disappeared from, our maps. It exists, and it is situated nearly a hundred miles (150 versts) due north, off the northern extremity of the Kotelnyi Island.

If we take into account the facts that there are serious reasons for admitting the existence of a land to the north of Novaya Zemlya,[1] and that the existence of Sannikoff's Land is now again

[1] See the Preliminary Report by A. Bunge in the *Izvestia* of the Russian Geographical Society, vol. xxiii. 1885, 5th fascicule.

[2] See Dr. Schrenck's "Zur Vorgeschichte der von der Akademie ausgerüsteten Expedition," in *Beiträge zur Kenntiss des Russischen Reichs*, 3te Folge, 1886.

[3] The account of the explorations in the Yana region has appeared in the *Beiträge zur Kenntniss des Russischen Reichs*, 1886.

[1] See the "Report of the Commission for an Arctic Expedition" in the *Izvestia* of the Russian Geographical Society, 1871.

confirmed, we must recognize that the discovery of Franz Joseph's Land was but a first step towards the discovery of the Arctic archipelagoes which undoubtedly exist under and within the 80th degree of latitude. P. A. K.

EARTHQUAKES IN THE LEVANT.[1]

THE Island of Zante, in the Ionian Group, to the north-west of the Gulf of Arcadia, is a centre from which no fewer than seven submarine cables radiate, and Mr. Forster has taken advantage of his position as manager of the station to make some interesting observations of the connection between interruptions of the cables and the occurrence of earthquakes, which are more frequent, he says, in the Levant than in any other part of the world, except Japan. The notes of what he has himself observed are valuable and suggestive, but unfortunately he has made them a peg on which to hang a theory that the "true and only reason for seismic disturbances" is that landslips and subsidences occur in ocean beds. Soundings of the bed of the Mediterranean, made chiefly in the interest of cable-laying, have brought to light extraordinarily rapid variations of depth. In one case, Mr. Forster tells us, the repairing ship of his company found a difference of 1500 feet between the bow and stern soundings. "We know of mushroom-shaped mountain ranges, abrupt and precipitous table-lands, immense marginal shelves and overhanging cliffs. . . . We know by soundings that many of these tottering masses are hanging over precipices from 3000 to 5000 feet in height, and that the erosion of the water at the base of the inverted cone-shaped rocks eventually causes them to slide over." Mr. Forster admits that a secondary cause of earthquakes may be "explosions owing to the filtration of water through the crevices and chasms that a denudation of so large an extent must necessarily cause." He has done good service in drawing attention to a cause of earthquakes which seismologists may have been disposed to under-rate, but he overstates his case outrageously in making this explanation cover every example. "I am prepared, of course," he says, "to encounter a torrent of objections to the acceptation of my theory as the sole cause of seismic disturbances" (*sic*), and this is well.

By way of supporting the theory he has written a long, rambling, inconsequent pamphlet, the manner and matter of which we may illustrate by quoting a paragraph that is neither better nor worse than its neighbours :—

"If, therefore, we are to believe that the process of cooling our planet, which began so many thousands of centuries ago, is gradually and surely condensing the nucleus of liquid or solid fire in its centre, it is reasonable to assume that the bed of the Mediterranean, by virtue of its more recent formation, should be more subject to the effects of the contraction of the upper crust than other parts which have gone through these periods already ; because it is evident that the contraction which originally commenced began equally so over the whole surface of the earth's sub-crust, and was, through some unknown reason, abruptly suspended in certain parts which only subsided and very suddenly when the cooling action was once more renewed."

Or this, which the author himself puts in italics :—

". . . *The severity of the concussion is always precisely proportionate to the bulk of the falling mass, the depth of its fall, and the nature of the matter constituting it, and on to which it falls.* . . ."

We commend the following to the attention of electricians, as coming from one of their own number :—

"By 'freeing' a cable, it is understood that the end opposite to the testing-station is detached from the apparatus and left free in the air ; and, provided the cable be electrically perfect, no deflection of the magnetic needle will appear at the testing-office when the cable is joined up through a delicate galvanometer to 'earth.' If, however, the cable thus insulated is lying near to or in the direct radius of a volcano, or near to any hot springs, the increased temperature would cause a thermopile to be set up, and by induction through the insulating material its presence would be plainly manifest."

The fact that in certain earthquakes the author made this test and found no current, is used as an argument to show the "absolutely local nature" of what in another place he calls the "*centri*" of the shocks.

What is of real value are his positive observations of certain cases where the rupture of a cable or the production of a bad fault in it took place at the moment an earthquake occurred, and of cases where, when the cable came to be repaired, the contour of the ocean bed was found to have undergone a distinct change, the cable being, in more than one instance, actually buried below the new surface. The great earthquake which destroyed Filiatra in August 1886, shook the telegraph office at Zante so sharply that the clerk rushed out. On returning to his Morse instrument, he found, by the paper band which was still running, that a message coming from Candia had been interrupted at the time of the shock, and tests taken immediately after showed a dead break 23 miles from Zante. Other cables, following a more northerly course, were not disturbed. In another instance a cable was broken at once in two places, 2 miles apart, apparently by a subsidence of the ground between. Once, when the Zante-Trepito cable was broken by an earthquake 7 miles from Zante, the repairing ship discovered "that the break had occurred in a depth of about 2000 feet of water, where about 1400 feet originally existed, and it was impossible to haul in the broken end, firmly jammed down by the mass which had fallen over and upon it."

If Mr. Forster had contented himself with telling the story of facts like these, which have come within his own observation, the seismologist would have felt nothing but gratitude. But these grains of wheat are only reached after wading through an intolerable deal of wordy chaff. The gist of the pamphlet is to be found in the last fifteen or twenty pages, and we advise the reader who wishes to save his patience to go to them at once. We conclude by quoting an interesting passage where Mr. Forster describes two natural phenomena of Cephalonia, about which one would like to know more :—

"Not far from Lizuri, which is on the western side of the Bay of Argostoli, is a moving rock which, unchanged by the roughest or the calmest sea, rocks to and fro with the regularity of a pendulum. It is separated from a fixed mass of rock against which it opens and shuts in its perpetual motion ; at one time it will jam a knife placed in the crevice, from which, in a few seconds, extraction is impossible, whilst the next moment you can easily insert your bent hand when its maximum aperture has been reached. This phenomenon has been carefully examined by many scientific men ; divers have been sent below to ascertain if it be the result of a detached rock from a neighbouring cliff having fallen on to another, and thus becoming very finely balanced, as all logan stones usually are. However, it was not only shown to be a perfectly solid rock, but it does not require the motion of the water to sway it, as so often we find it erroneously stated, the motive power for swaying it being furnished from an absolutely inexplicable cause. Nearly opposite to this rocking stone the other remarkable phenomenon is to be found, consisting of a body of water, equal in bulk to about half a million gallons per day, running in from the sea at four points on the coast somewhat rapidly for a certain distance until it gradually becomes sucked into the earth and disappears. By conducting the water into an artificial canal for a few yards, and by collecting the four points of supply into one, enough motive power is obtained to drive two mills. The stream, after being thus utilized, is allowed to follow its own course, and is lost among the rocks. . . . [It] has no visible outlet."

[1] "Seismology: a Paper on Earthquakes in General ; together with a New Theory of their Origin, developed by the Introduction of Submarine Telegraphy." By W. G. Forster, Manager and Electrician to the Eastern Telegraph Company, Zante. Dedicated, by Special Authority, to His Majesty George I., King of the Hellenes. Pp. 68. (London : Waterlow and Sons, 1887.)

THE MINERAL CONCRETION OF THE TEAK TREE.[1]

AT the last meeting of the Nilgiri Natural History Society Mr. Lawson showed a specimen of a whitish mineral substance found in a teak tree growing in the Government Plantation at Nilambúr. This peculiar secretion is not altogether unknown to officers in the Forest Department, and its composition has on more than one occasion been investigated by chemists.

In 1870 the fact of calcareous masses occurring in timber was brought to the notice of the Asiatic Society of Bengal by Mr. R. V. Stoney, who stated (*vide* P.A.S.B., May 1870, p. 135) that many trees in Orissa had pieces of limestone or calcareous tufa

[1] A Paper read by David Hooper at a meeting of the Nilgiri Natural History Society, Ootacamund, November 7, 1887.

in their fissures, but principally Asan (*Terminalia tomentosa*, W. and A.), Swarm (*Zizyphus rugosa*, Lam.), Sissu (*Dalbergia sissu*, Roxb.), and Abnus (*Diospyros melanoxylon*, Roxb.).

In 1880, Mr. V. Ball, in making a geological survey in the Central Provinces, met with this concretion, and thus alludes to it in his "Jungle Life in India":—"Some white marks on the cut stumps of an Asan tree caught my eye, and these on examination proved to be sections or laminæ of calcareous matter which alternated with the ordinary rings of woody growth. The rocks about were gneisses and schists, and I could discover nothing in the soil to account for the peculiarity. In some cases irregularly shaped pieces seven inches long by two inches thick were met in the trunks at a height of about six feet from the ground. By the natives the lime is burnt and used for chewing with *pan*. On examination it was found there was no structure in these masses which would justify a conclusion that they had been formed by insects. Some included portions of decayed wood seemed to be cemented together by the lime."

Major-General Morgan, late Deputy Conservator of Forests, Madras, speaks of it in the following terms in his "Forestry of Southern India":—"It is a curious fact that in the Wynaad, though there is no free lime in the soil, yet Teak (*Tectona grandis*) and Blackwood (*Dalbergia latifolia*), if wounded near the ground, contrive to absorb large quantities of lime. It may be seen, incrusting the tree on the surface as far as four feet in height, from three inches to a foot in width, and two or three inches in thickness. The lime is so hard that it destroys circular saws, and the Carumburs use it for chewing with betel."

Abel, in 1854, thus described it: "The wood of teak, which grows in the South of India and other tropical countries, frequently exhibits cracks and cavities of considerable extent lined with a white crystalline deposit consisting chiefly of hydrocalcic orthophosphate, $CaHPO_4,H_2O$, with about 11·4 per cent. ammonio-magnesium phosphate" (Chem. Soc. Q.J. xv. 91.)

This white deposit in the wood of teak has also been examined by Thoms, who found it to consist of monocalcic orthophosphate, $CaHPO_4$ ("Landw. Versuchs. St." xxii. 68, xxiii. 413). More recently still Prof. Judd has found in teak a specimen of crystalline apatite, a well-known mineral containing a large proportion of calcium phosphate.

"The formation of this deposit indicates that the wood itself must contain a considerable quantity of phosphoric acid, and the analysis shows this is really the case, as the ash of teak wood is composed as follows:—

CaO	MgO	FeO	K_2O	Na_2O	SiO_2	SO_3	P_2O_5	CO_2	Cl
31·35	9·74	0·80	1·47	0·04	24·98	2·22	29·69	0·01	0·01

The percentages of carbon and hydrogen are higher than in most woods, and this together with the richness in calcium phosphate and silica may perhaps account for the great hardness of teak" (Watts' "Dict. Chemistry," 3rd Supp. p. 1894).

The sample from Nilambúr was in the form of a rounded flattened cake about ten inches in diameter and two or three inches in thickness; dirty white in colour, with a rough gritty surface. A sample was made for analysis by breaking off portions from different parts of the cake and reducing the whole to a fine powder. The powder examined under the microscope was mainly in an amorphous condition similar to prepared chalk, with a dark-coloured gummy matter, and a small quantity of crystalline quartz sand. The following is the composition:—

Calcium carbonate	70·05
Tricalcic orthophosphate	2·89
Quartz sand	9·76
Organic matter	14·30
Moisture	3·00
	100·00

The analysis shows that the principal compound is calcium carbonate, and the concretion approaches nearer the chalk or limestone formation than that of the apatite or phosphatic found by other investigators. An examination of deposits from other trees might show greater differences than these, but it seems enough has been done to prove that the calcium element forms the base.

The sand, probably blown up as dust and made to adhere by the organic matter, is a mechanical ingredient. The deposit contained no salts of sodium or calcium soluble in water, nor any ammoniacal compounds; this would stand to reason, as the heavy rain to which this district is subjected would scarcely leave anything soluble on the trees.

A sample of the soil from the Teak Plantations, the same as that in which the ipecacuanha is being cultivated, has also been examined. It is a light reddish brown sandy loam with quartz. In a dry state it contains 79 per cent. of silica and silicates, about 5 per cent. of organic matter, the same of iron oxide and alumina, and 0·217 per cent. lime as oxide.

The scanty amount of lime present in the soil, and the large amount found in the tree, show what an enormous quantity must have been taken up by the sap. I have shown elsewhere that a full-sized cinchona tree contains about ten ounces of lime (as slaked lime), not concentrated by abnormal development in one place, but distributed in all its parts. A teak tree from its size and ash contents would have a much larger supply than a cinchona, and yet, it seems, is able to excrete it in some abundance. In what manner this takes place is not easy to determine. The calcium enters the plant in a soluble form as sulphate. The calcium unites with oxalic and other acids and is precipitated, while the sulphuric acid parts with its sulphur to form organic compounds. A wound in the tree is liable to render these processes abnormal by causing the vegetable acids to ferment by exposure to the air and to yield carbonic acid as one of the products, and this meeting with the calcium in the ascending sap exuding from the wound might convert it into an insoluble calcium carbonate which would harden in the cavity of the tree and form the deposit.

THE NEW YORK AGRICULTURAL STATION.[1]

THE special report of the Director, Dr. E. Lewis Sturtevant, extends over the first fifty-seven of a volume of 480 pages, and within their limits are to be found the general conclusions arrived at during the past year. The remainder of the *brochure* consists of the detailed reports of the horticulturist, the botanist, and the chemist. After an analysis of the rainfall and temperature of 1887, which appear in general climatic conditions to have borne a great resemblance to what we ourselves experienced, the Director calls special attention to the importance of soil moisture, and surface cultivation as a means of conserving it. He shows the vast importance of checking evaporation from the surface by preserving a finely pulverized condition of the top soil. This he calls a "soil mulch," and states that "it protects the capillary outlets from surface exposure."

"The extent of the conservation of water through the prevention of evaporation by cultivation, as measured by the lysimeters in 1885 from May to September inclusive, with a rainfall of 14·42 inches, as between bare soil and cultivated soil, was about 1·4 inch, and as between cultivated land and sod-land about 2·5 inches. The rational direction, therefore, to the farmer for carrying out intercultural tillages must be to use an implement as a means to an end, *i.e.* the maintaining of a mulch of loose soil upon the field. . . . The intercultural tillage should be applied whenever the upper soil has regained, through the effect of rains, its connection with the lower soil, and the capillary tubes become extended to the surface. Following the same line of argument, the evil effects of weeds are attributed to their appropriation and transpiration of moisture from the soil rather than to their robbing the plant of food constituents. This conclusion will be brought home to anyone who notices the dry condition of the soil in near proximity to tree roots."

The remarks upon feeding cattle with a view to milk and to beef production are interesting, but the system of experimenting upon single animals is not to be commended. The conclusion forced upon the Director, that "individuality is sufficient to mask the influence of food," is patent to anyone, and should demonstrate the absolute need of carrying out any feeding experiments upon a large number of cattle simultaneously. Average results may then be expected upon which practice may be based.

The Director pours a flood of cold water upon the system of plot experiments in the field. Under the head of "Conclusions" he says:—"These field trials indicate the utter unreliability of field experiments, and should convince the public of the lack of certainty which attends all general conclusions gained by this process. I trust the time may arrive when this plat work, instead of being forced upon experiment stations, will be condemned." Certainly

[1] Sixth Annual Report of the Board of Control of the New York Agricultural Experiment Station (Geneva, Ontario Co.) for the year 1887.

after obtaining no increase by the application of 1400 pounds of a fertilizer, over what had been obtained from a dressing of 400 pounds per acre, there might appear some cause for complaint. We cannot, however, indorse Dr. Sturtevant's opinions. Plot experiments may be made fairly representative of larger areas, and upon them various treatments of soil and crop may be compared. No doubt great care should be taken in carrying out such experiments; but surely a series of plots of $\frac{1}{4}$, $\frac{1}{10}$, or $\frac{1}{20}$ acre each might be and are made to teach most useful lessons. Dr. Sturtevant might well pause to consider that if such experiments are of no value the value of other experiments might be doubted, and the public, to whom he appeals, might think fit to rescind a grant amounting to £4000 a year for the purpose of carrying out his researches. The volume abounds in tables of analyses of fodder and grain crops. A large portion (200 pages) of the middle is occupied by a descriptive catalogue of varieties of beet, carrot, radish, turnip, onion, celery, spinach, squash, tomato, &c., mostly very wearisome, and savouring more of the catalogue of the seedsman than of the results of scientific work.

Downton. JOHN WRIGHTSON.

SCIENTIFIC SERIALS.

Bulletin de l'Académie Royale de Belgique, January.—Researches on the influence of magnetism and temperature on the electric resistance of bismuth and its alloys with lead and tin, by Ed. van Aubel. These protracted experiments have been undertaken in order to determine the variations of electric resistance due, not only to magnetism and heat, but also to molecular structure, with a view to discovering the causes of the disturbances and completing our knowledge of the phenomena first observed by Hall. In the present paper, a first contribution to the study of the subject, the author deals mainly with the diminution of the electric resistance of bismuth and its alloys under increased temperature. He shows that the anomaly cannot be due to the presence in the metal of foreign elements such as arsenic, tin, lead, or iron. The state of greater or less tension of the bismuth itself also seems to have no influence. But the study of some bismuth wire obtained by the soldering of the filings of this metal under a pressure of several thousand atmospheres constantly exhibits a considerable increase of resistance when the temperature is raised.—Experimental researches on the vision of the Arthropods, third part, by Felix Plateau. This part deals with the vision of caterpillars, and with the *rôle* of the frontal ocelli in the perfect insects. The very numerous experiments here described and carried out under the most varied conditions, tend to the general conclusion that in insects possessing both compound and simple eyes (ocelli), the former are of some service, while the latter are quite useless, and should consequently be grouped in the category of rudimentary or atrophied organs. In the case of caterpillars the vision is defective, not extending distinctly beyond one centimetre, and is supplemented by the antennæ and the fine hairs covering the body. Perfect insects when completely blinded almost invariably fly in a straight line vertically, which, against the opinion of Forel, is attributed to the more intense light of the higher regions, to which the whole surface of the body is susceptible. The primitive "dermatoptic sensation" is revived, and acts in a feeble way as a substitute for the later developed ocular vision of which the animal has been deprived.—On the molecular work of the organic liquids, by P. De Heen. It is shown that the author's formula of 1882, that for the organic fluids belonging to one and the same homologous series the molecular work is fairly constant, has been mainly confirmed by subsequent research.—This number of the *Bulletin* also contains a valuable paper by Louis Henry on the volatility of the carbon compounds, the result of several years' research.

Rivista Scientifico-Industriale, February 29.—On the peronospora of the grape-vine, by Prof. G. Cuboni. The two phases of this disease are fully described for the first time, and the disease itself is carefully distinguished from black-rot and other analogous forms of blight with which it is often confounded. A mixture of sulphur with 3 or 4 per cent. of the sulphate of copper is proposed as the best remedy if applied at an early stage.—Prof. E. Canestrini concludes his experiments on some effects produced by induction sparks. In one instance the leaves of some perennial plants were found to be covered with dark spots similar to those frequently observed on plants struck by lightning. But the results of these researches, like others of a similar kind, have obviously no more than a relative value, depending as they do on the intensity of the induced currents.

Journal of the Russian Chemical and Physical Society, vol. xix. No. 8.—Isomery in the series C_nH_{2n-2}, by A. Favorsky.—On the laws presiding at reactions of direct addition, by J. Kabloukoff.—Short notes by MM. E. Sokoloff, Joukovsky, and Gorboff.—Experimental researches into the oscillations of electrical force in electrolytes, by A. Sokoloff; it is the third of a series of elaborate papers on the subject, especially with regard to the capacities of voltameters.—On the measuring of specific heat by the method of mixtures at a constant temperature, by N. Hesehus.—End of a full bibliography of all books and articles printed in Russia on chemistry and chemical technology during the year 1886.

Journal of the Russian Chemical and Physical Society, vol. xix. No. 9.—On the speed of formation of acetic ethers of monatomic alcohols, by N. Menshutkin, being a first paper of a new series of researches where the compound influence of the surrounding medium in which the reaction is going on has to be studied.—Notes by MM. Matweieff and Spiridonoff.—On the empirical formula of cholic acid, by P. Latchinoff, being an answer to the criticisms against the new formula ($C_{25}H_{42}O_5$) proposed by the author.—On the gelatinous state of albuminoid bodies, by W. Mikhailoff, being the first of a series of papers intended to summarize elaborate researches on the subject, in accordance with the principles laid down by Lieberkühn and his followers.—On the number of parameters which determine the displacement of a kinematic chain, by P. Somoff. Taking up the view of Reuleaux, who recorded each mechanism as a kinematic chain, the author shows the necessity of considering the degree of freedom left to each part of the chain in its displacements in various mechanisms.—On the dependence of the colour of bodies on the angle of incidence of the rays of light, by W. Rosenberg.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 15.—"Report of the Observations of the Total Solar Eclipse of August 29, 1886, made at Grenville, in the Island of Grenada." By H. H. Turner, M.A., B.Sc., Fellow of Trinity College, Cambridge. Communicated by the Astronomer-Royal.

The first part of the paper gives details of the general arrangements made for observation—the selection of a site, the erection of the instruments, and a hut to cover them; and refers to the unfavourable conditions under which the observations were made. The second part gives the results of the observations. These were of two kinds.

(1) Before and after totality the order of appearance and disappearance of a number of bright lines in the spectrum of the chromosphere and inner corona was watched. The lines selected were those observed by Mr. Lockyer in the Egyptian eclipse of 1882, and the observations were undertaken with a view to the confirmation of his results.

The lines are denoted for convenience by small letters as follows:—

	λ.		λ.		λ.
a	4870·4	*e*	4917·9	*h*	4932·5
b	4871·2	*f*	4919·6	*i*	4933·4
c	4890·0	*g*	4923·1	*k*	4956·5
d	4890·4			*l*	4970·0

With this nomenclature, a table given by Mr. Lockyer in a short account of his results (Roy. Soc. Proc., vol. xxxiv., 1883, pp. 291, &c.) shows that lines *g* and *l* are seen by Tacchini in prominences, while *a*, *b*, *c*, *d*, *e*, *f*, and *k* are seen in spots.

Mr. Lockyer saw *g* and *i* 7 minutes before totality,
and in addition *k* and *l* 3 ,, ,,
and all the lines 2 ,, ,,

In my own observations I saw *g* 3 minutes before totality,
and in addition *i* 40 seconds ,,

while the moment of appearance of all the lines was indistinguishable from the commencement of totality.

After totality clouds obscured the sun for a short time; but on their clearing the visibility of *g* and *k* was noted; *i* could not be seen.

The three lines *g*, *i*, and *k* were extremely short, and did not

appear to extend beyond the chromosphere before and after totality.

The unfavourable conditions under which the observations were made as compared with Mr. Lockyer's—with a low sun and through passing clouds, and an atmosphere charged with moisture which doubtless diminished the light in this region of the spectrum considerably—perhaps account in some measure for the striking difference in vividness of the phenomena. The solar activity was also much nearer minimum in 1886 than in 1882. As far as they go, however, the observations are confirmatory of Mr. Lockyer's, except in the visibility of the line *k* after totality. This line was not noted before totality, and it is possible that the observation may be spurious, although the evidence for it is as good as that for all the observations, which were found to be generally of a difficult character. The instrument used was a 6-inch refractor by Simms, with a grating spectroscope; the grating being $1\frac{1}{2}$ inch square, ruled with 17,000 lines to the inch. The second order of spectrum was used.

(2) During totality I was directed to look for currents in the corona. I can only report a negative result. The structure of the corona appeared in a 4-inch refractor, with a power of 80, to be radial to the limb throughout, and no striking differences in special localities were noticed.

Appended to the paper are two drawings which do not attempt to give more than the distances to which the coronal rays extended in various directions. One was made by Mr. St. George with an opera-glass, and the other by Lieut. Smith with the naked eye; but in the latter case the observer's eyes had been specially covered fifteen minutes before totality, and the brighter portions of the corona were screened from him by a disk of angular diameter three times that of the moon. He consequently traced the rays much further than Mr. St. George, though, allowing for this difference in conditions, the drawings are fairly accordant.

"On the Ultra-Violet Spectra of the Elements. Part III. Cobalt and Nickel." By Profs. Liveing and Dewar.

The authors compare the results obtained by the Rutherfurd grating which they used in measuring the wave-lengths of the iron lines with those obtained wlth the larger Rowland's grating used for measuring the wave-lengths recorded in this paper, and find them closely concordant. They next compare the measures of wave-lengths of the cadmium lines obtained by them by means of a plane Rowland's grating and a goniometer with an 18-inch graduated circle with those obtained by Bell with a large concave grating of 20 feet focal length. The result of the comparison is that the plane grating gives measures which agree very closely with those given by the concave grating, while the former gives more light and is better for complicated spectra, such as those described in this paper, because the overlapping spectra of different orders are not all in focus together as they are when a concave grating is used.

The authors give a list of 580 ultra-violet lines of cobalt and 480 lines of nickel. They find a certain general resemblance of the two spectra, but no such exact correspondence as the close chemical relationship of the two metals would render probable. They point out that the coincidences of lines of the two metals are hardly, if at all, more in number than would have been the case if the distribution of the lines had been fortuitous. They give a map of each spectrum to the same scale as Ångström's normal solar spectrum.

Linnean Society, March 15.—W. Carruthers, F.R.S., President, in the chair.—On a ballot being taken, the following were elected Fellows of the Society: Messrs. J. W. Taylor, W. Gardiner, and David Sharp. The following were admitted Fellows of the Society: Messrs. A. G. Renshaw and A. E. Shipley.—Mr. J. Harting exhibited the frontal portion of the skull of a red-deer stag, which, although an adult animal, had never possessed horns, and made some remarks on the occasional occurrence of this abnormality. The stag in question was one which had been shot some years ago by the late Emperor of Germany in the Royal forest of Göhrde, in Hanover. A discussion followed in which the President, Mr. Seebohm, and Dr. Hamilton took part.—The first paper of the evening was then read by Mr. George Massee, entitled "A Monograph of the *Thelophoreæ*," and drawings of several of these Fungi were exhibited. The paper was criticized by Mr. A. W. Bennett and Prof. Marshall Ward.—In the absence of the author, a paper by Mr. E. A. Batters, describing three new marine Algæ, was then read by the Botanical Secretary, Mr. B. Daydon Jackson, who exhibited the drawings made to illustrate the paper. After some critical remarks from the President, Mr. Harting pointed out the indirect influence of the Gulf Stream in causing a deposition of northern sea-weeds upon the north-east portion of the English coast, where some of the species described had been found.

Zoological Society, March 20.—Mr. Henry Seebohm in the chair.—Mr. G. A. Boulenger read a note on the classification of the Ranidæ, in which, after speaking of the difficulty hitherto experienced in dividing this large group satisfactorily, he called attention to Peters's discovery that in certain forms a small additional phalanx is present between the ultimate and what is normally the penultimate phalanx. The author therefore proposed to separate the family Ranidæ into two groups, according to the presence or absence of this peculiar digital structure.—Mr. G. B. Sowerby gave the description of sixteen new species of shells, amongst which were two species of the genus *Lima* from Hong Kong and Japan; a remarkable species of the rare genus *Malletia* from the Bay of Bengal; a very distinct species of *Cypræa* from Japan; and one of the largest species yet known of the genus *Columbella*.—Mr. F. E. Beddard read some notes on a freshwater Annelid, of which he had obtained specimens from a tank in the Society's Gardens. Mr. Beddard referred these specimens to a new species of the genus *Æolosoma*, which he proposed to called *Æ. headleyi*.—Prof. Newton, F.R.S., communicated (on behalf of Mr. Scott Barchard Wilson) the description of *Chloridops*, a new generic form of Fringillidæ, based on a specimen obtained on the west coast of the Island of Hawaii, Sandwich Group, which he proposed to name *Chloridops koua*. Unfortunately the single example yet obtained was of the female sex.

Geological Society, March 14.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—On the gneissic rocks off the Lizard, by Howard Fox, with notes on specimens by J. J. H. Teall. The rocks may be classed under three heads: (1) the coarse gneisses or Mên Hyr type, (2) the light-banded granulitic gneisses or Wiltshire type, and (3) the transition micaceous rocks of "Labham Reefs," type intermediate between (2) and the mainland schists. The first are seen in Mulvin, Taylor's Rock, Man-of-war Rocks, the Stags, Mên Par, Clidgas, Mên Hyr, and Vasiler; the second in Sanspareil, the Quadrant and adjoining reefs, Labham Rocks, &c.; and the third in the Labham Reefs. The inclination of the divisional planes appeared conformable with that of the rocks of the mainland. The gneisses and granulites of several of the islands are traversed by numerous dykes of prophyritic basic rock, seen in Taylor's Rock, Man-of-war Rocks, Sanspareil, Quadrant Rock and Shoals, and Clidgas. These dykes have been disturbed by movements subsequent to their intrusion. They sometimes strike across the foliation-planes of the gneiss, and send veins into the latter rock; at other times the strike is parallel to that of the foliation-planes; the two modes of occurrence are occasionally observable in different portions of the course of the same dyke, *e.g.* in one traversing that part of the Man-of-war group known as the Spire. This dyke is also noticeable from the fact that it appears to be traversed by veins of gneiss. The dykes vary in width from 18 inches to several feet. In his notes on the specimens, Mr. Teall arranges the rocks in four groups. Prof. Bonney spoke in high terms of the value of the work done, as it was in a region accessible with difficulty, which time did not permit him to explore when working at the rocks of the mainland.—The Monian system, by the Rev. J. F. Blake. The reading of this paper was followed by a discussion in which the President, Dr. Hicks, Prof. Hull, and Prof. Bonney took part.

Royal Meteorological Society, March 21.—Dr. W. Marcet, F.R.S., President, in the chair.—The President delivered an address on atmospheric electricity. He first alluded to Franklin's experiments in America in 1752, who succeeded in obtaining the electricity of a storm-cloud by conducting it along the string of a kite sent into the cloud. De Romas, in Europe, repeated the experiment, and, having placed a wire within the twine his kite was attached to, obtained sparks of 9 or 10 feet in length. The characters of the two kinds of electricities were next described—the vitreous or positive, which was produced by rubbing glass, and the resinous or negative, obtained by rubbing sealing-wax or another resinous substance; and it was shown, by bringing suspended balls of pith within the in-

fluence of these electricities, that electricities of different kinds attract each other, and those of the same kind repel each other. De Saussure's and Volta's electroscopes were next described, pith-balls being used in the former and blades of straw in the latter for testing the pressure of electricity. With the object of measuring the force of electricity, Sir W. Thomson's electrometer was mentioned, in which the electricity is collected from the air by means of an insulated cistern letting out water drop by drop, each drop becoming covered with electricity from the atmosphere, which runs into the cistern where it is stored up, and made to act upon that portion of the instrument which records its degree or amount. The atmosphere is always more or less electrical, or, in other words, possessed of electrical tension, and this is nearly always positive; while the earth exhibits electrical characters of a negative kind. The effects of atmospheric electricity were classed by Dr. Marcet under three heads: (1) lightning in thunderstorms; (2) the formation of hail; (3) the formation of the aurora borealis and australis. He explained how clouds acquired their electrical activity by remarking that clouds forming in a blue sky, by a local condensation of moisture, became charged with positive electricity from the atmosphere; while heavy dark clouds rising from below nearer to the earth were filled with terrestrial negative electricity, and the two systems of clouds attracting each other would discharge their electricity, giving rise to flashes of lightning. In some cases a storm-cloud charged with positive electricity would approach the earth, attracting the terrestrial negative electricity, and when within a certain distance shoot out lightning which would apparently strike the earth; but it would just as well have struck the cloud, only there was nothing in the cloud to sustain any damage, while on the earth there were many objects lightning would destroy, to say nothing of its effects upon animal life. Thunder is the noise produced by the air rushing in to fill up the vacuum made by the heat of the lightning flash. There may be sheet lightning, zig-zag or forked lightning, or globular lightning. The latter is particularly interesting from its assuming a spherical form. Illustrations were given of objects struck by lightning, the most remarkable being, perhaps, the clothes of a working man which were torn into shreds, while the man himself was not seriously injured. Dr. Marcet next proceeded to show a flash of lightning, which he produced by throwing on a white screen the image of an electric spark 2 or 3 inches in length, enlarged by means of the lens of an optical lantern; forked lightning, 6 or 8 feet in length, with its irregular zig-zag course, was most clearly demonstrated. After alluding to the protecting power of lightning conductors and their construction, Dr. Marcet explained the formation of hail and waterspouts, and exhibited an instrument by Prof. Colladon, of Geneva, for showing the formation of waterspouts. He concluded his address with a few remarks on the aurora borealis and australis, the formation of which was illustrated by de la Rive's experiment, which consisted of successive discharges of electric sparks through a partial vacuum while under the influence of a powerful magnet; electric sheets of light were seen assuming the form of bands and possessed of a certain rotating motion.—Mr. G. J. Symons, F.R.S., read a short commuication on the non-existence of thunderbolts, and briefly described the history of several so-called thunderbolts, the specimens obtained being of an amusing character, thus clearly showing that they were of a terrestrial and not a celestial character.

Edinburgh.

Royal Society, February 20.—Sir W. Thomson, President, in the chair.—A preliminary note on the duration of impact, by Prof. Tait, was communicated. The results already obtained were got by means of a roughly made apparatus designed for the purpose of testing the method used. When a wooden block of 10 lbs. mass fell through a height of 18½ inches on a rounded lump of gutta-percha, the time of impact was found to be 0·001 sec., and the coefficient of restitution was 0·26.—A paper on a bathymetrical survey of the chief Perthshire lochs was read by Mr. J. S. Grant Wilson of H.M. Geological Survey. Lochs Rannoch, Tummel, Earn, and Tay, were specially dealt with. In the discussion which followed, Sir W. Thomson remarked that he did not consider that the ice had much to do with the formation of rock basins. Where it found a rock basin already in existence it might increase its dimensions.—Mr. H. M. Cadell, H.M. Geological Survey for Scotland, read a paper, of which an abstract appeared in our last issue (p. 488), on experimental researches in mountain building.—Mr. Peach communicated a paper by Dr. Ernst Stecher on contact phenomena of some Scottish olivine diabases.

March 5.—Mr. J. Murray, Vice-President, in the chair.—Mr. W. E. Hoyle communicated a paper by Mr. D. McAlpine on observations on the movements of the entire detached animal, and of detached ciliated parts of bivalve mollusks, viz. gills, mantle-lobes, labial palps, and foot.—The Chairman communicated a report on the fishes which he had obtained in deep water on the north-west coast of Scotland. The report was drawn up by Dr. A. Günther, F.R.S., Keeper of the Zoological Department, British Museum.—Prof. Haycraft read a paper by Dr. Carlier and himself on the morphological changes which take place in blood during coagulation.—A paper by Prof. Tait on the mean free path, and the number of collisions per particle per second in a group of equal spheres, was communicated. In this paper Prof. Tait cited De Morgan's definition of the term "mean," and pointed out the difference between the mean free path, properly so called, and the quantity to which that name is usually applied.—A preliminary note by the same author on the compressibility of glass at different temperatures was also read. The glass experimented upon was ordinary lead glass. At 8° C. the compressibility per atmosphere is 0·0000027, and increases by 0·00000002 per degree Centigrade of rise of temperature.

March 19.—Sir W. Thomson, President, in the chair.—Mr. George Seton read a paper on illegitimacy in the parish of Marnoch.—Dr. G. Sims Woodhead communicated some notes on the use of the mercuric salts as antiseptic surgical lotions.—In a paper on the effect of differential mass-motion on the permeability of gas, Prof. Tait gave the calculations which he promised in his reply to Prof. Boltzman published in the *Philosophical Magazine*.—The President read the second part of a paper by Mr. J. J. Coleman, on a new diffusimeter, and other apparatus for liquid diffusion, and discussed the determination of diffusivity in absolute measure from Mr. Coleman's experiments.—Sir W. Thomson also read an extract from a letter of the late William Froude to himself, dealing with the soaring of birds. Mr. Froude showed that in all cases soaring is dependent on the existence of upward air currents. In the case of a complete calm at sea, the upward current is produced by displacement as a wave passes underneath.—Mr. W. Peddie read a preliminary note on new determinations of the electric resistance of liquids by a method based upon Joule's law, and which therefore avoids any error which might be caused by transition-resistance or polarization.—Mr. C. A. Stevenson gave a notice of the recent earthquake in Scotland, with observations on those since 1882.

Paris.

Academy of Sciences, March 19.—M. Janssen in the chair.—On certain points connected with the theory of accidental errors, by M. Faye. It is argued that the arithmetical mean does not necessarily and in all cases give the most probable result. The law of error can be regarded only as a simple approximation to the truth, although so far valuable that it may be freely applied to all sorts of observations and measurements, provided they be exempt from systematic error. The danger lies in excluding all extremes which might have the effect of enabling the observer to draw any conclusion he pleases, or which squares best with some preconceived view. The same subject is discussed in a paper by M. J. Bertrand on the probable value of the smallest errors in a series of observations.—On a point in the theory of the moon, by M. F. Tisserand. The object of these remarks is to determine in Delaunay's theory of the moon the full scope and application of the theorem of the invariability of the great axis of the lunar orbit. The demonstration here induced from Delaunay's method is extremely simple, and leads to some further interesting inductions.—New theory of the equatorial *coudé*, by MM. Lœwy and P. Puiseux. The paper deals with the corrective terms depending on the inner glass and the axis of declination. In a future paper will be given the terms depending on the outer glass, together with the complete formulas of reduction.—On the absorption of saline substances by plants, by MM. Berthelot and G. André. The experiments here described are mainly confined to the sulphate of potassa, and will in future be extended to other substances with a view to elucidate the obscure processes by which plants derive their mineral elements from the soil. The solution of the

soil whence the roots derive the sulphate is shown to remain always richer than the solution penetrating into the vessels of the roots. Thus is confirmed the general law of this class of phenomena, and some important inductions are drawn from it in connection with the formation of the nitrates in certain plants.—On the relations of atmospheric nitrogen with vegetable humus, by M. Th. Schlœsing. Having previously studied the relations of vegetable earth with ammonia, carbonic acid, and oxygen, the author here extends his researches to its relations with atmospheric nitrogen.—On the actinometric observations made at Montpellier during the year 1887, by M. A. Crova. From these observations it appears that the calorific intensity, measured at noon, steadily increased from the beginning of winter nearly to the end of spring, attaining its monthly maximum (1·35 calorie) in May, and its absolute maximum (1·54 cal.) on the 24th of that month. Then it declined rapidly, its mean value during the summer being inferior to the means of other seasons. It rose, fell, and again rose in autumn, during which period it acquired a new maximum of 1·26. These observations confirm the general laws induced from the records of previous years, showing that, although the epochs of maxima and minima may often be exceptionally displaced, the maxima of radiation occur normally in spring, the minima in summer. Tables follow giving the mean annual intensities and other meteorological data for the five years 1883–87.—On the unification of the calendar, by M. Tondini. Reference is made to the recent steps taken by Italy for the purpose of promoting the universal adoption of the Gregorian calendar, which, thanks to the action of England, has already replaced the Chinese system in Japan under almost insurmountable difficulties.—Remarks accompanying the presentation of a model of the Fumat safety-lamp, adapted for use in mines subject to fire-damp, by M. Daubrée. This lamp, which has been for some time in use in the Grand Combe mines, has successfully withstood the severe tests to which it has been subjected by MM. Mallard and Le Châtelier, and seems to answer all the purposes of such an appliance quite as well as any other hitherto devised.—The meridian of Laghwat, by M. L. Bassot. The geodetic junction of Spain with Algeria, completed in 1879, has now been extended to Laghwat, 180 miles south of Algiers, and on the verge of the Sahara. This carries the great meridian for the west of Europe across 28° of latitude, from the northernmost point of Great Britain through France and Spain to about 33° N. latitude, on the confines of the desert. To the triangulation have been attached the astronomic stations Gelt-es-Stel and Laghwat itself, the latitude and longitude and an azimuth for each of these places having been accurately determined.—On the passage of the electric current through sulphur, by M. E. Duter. Sulphur, a very bad conductor at the normal temperature, is here shown to acquire a considerable degree of conductibility when raised to the boiling-point.—On the radiograph, by M. Louis Olivier. The instrument described under this name has been invented by the author as a self-acting recording photometer. At each revolution of the drum it closes an electric circuit, and thus automatically shuts off the luminous action at any desired moment. While serving in a general way as a registering apparatus for light, it is capable of venous applications in photography, meteorology, and physics.—On the hydrate of sulphurated hydrogen, by MM. de Forcrand and Villard. Having already made known the composition of this substance, $HS + 12HO$, and measured its tension of dissociation between $+0·5$ and 29° C., the authors here resume its study for the purpose of more accurately determining this tension at or about the temperature of 0°. This is found to be about equal to atmospheric pressure.—Experimental researches on chronic intoxication by alcohol, by MM. A. Mairet and Combemale. Having previously described the influence of chronic alcoholic intoxication on the nervous and muscular systems, the authors here study its effects on the heart, the respiratory and digestive organs, and the bodily temperature.

BERLIN.

Physical Society, March 2.—Prof. du Bois Reymond, President, in the chair.—Dr. Gumlich spoke on Newton's rings as seen by transmitted light. The speaker had calculated and experimentally verified the formulæ for the rings as seen by transmitted light, in the same way as many years ago Prof. Wangerin had treated the rings when seen by reflected light, and subsequently verified the results of his calculations experimentally in conjunction with Prof. Sohncke. The outcome of Dr. Gumlich's calculation was the same as that of Prof. Wangerin for the rings seen by reflected light. When the light is incident at right angles, the rings are circles lying in one plane: when the angle of incidence is less than a right angle, the rings lie on a surface of extremely complicated shape, which is characterized by a primary ordinate, and an oblique ordinate at right angles to the former, which do not coincide at the point of intersection. By means of an apparatus which the speaker showed and carefully described, he had experimentally tested the accuracy of his calculations, and found them fully confirmed. When the light is incident obliquely, the rings are no longer circles as they are when it falls on to and passes through the medium at right angles, but are now ellipses whose axes bear a ratio to each other which is dependent on the angle of incidence of the light. It was not found possible to obtain any definite results as regards the width of the rings, since this is very largely affected by temperature.—Dr. Sprung reported on a work which had been sent in by Dr. Müller-Erzbach treating of the determination of mean temperature by means of the weight of water which is vaporized. A bulb blown on the end of a glass tube is half filled with water and introduced into a wide-necked flask whose bottom is covered with sulphuric acid. Assuming the truth of Dalton's law of tensions, Dr. Müller has arrived at a formula by means of which the mean temperature of a space can be determined from the mass of water which is vaporized in a given time.—At the end of the meeting Prof. Lampe discussed a reply which had been recently made to a criticism of his on a piece of work done last year by Dr. Häussler, and showed how devoid of foundation the reply was.

BOOKS, PAMPHLETS, and SERIALS RECEIVED

Geography of the British Isles: A. Geikie (Macmillan).—Pubblicazione del Real Osservatorio di Palermo, vol. iii. (Palermo).—Glen Desseray: Principal Shairp (Macmillan).—Die Wechselbeziehungen zwischen Pflanzen und Ameisen im Tropischen Amerika: A. F. W. Schimper (Fischer).—Encyclopædia Britannica, vol. xxiii. (Black).—Disease, its Prevention and Cure: C. G. Godfrey (Grevel).—Untersuchungen über Heterogenese, iii., Dr. A. P. Fokker (Groningen).—Journal of Morphology, vol. i. No. 2 (Ginn, Boston).

CONTENTS.

THURSDAY, APRIL 5, 1888.

THE FORESTRY SCHOOL AT COOPER'S HILL.

THE Forestry School at Cooper's Hill is intended in the first place for the education of a certain number annually of young officers for the Indian Forest Department. The arrangements are, nevertheless, of such a kind that private students are admitted to the forestry course, in as far as space is available, and on condition that they conform to the rules.

It is in many ways advantageous that the Forestry School is attached to the Royal Indian Engineering College at Cooper's Hill. Although the course for forest students is necessarily different from that designed for engineering students, there are several subjects to be studied in common, and consequently the present arrangements admit of the forest students obtaining their training in surveying, descriptive engineering, and mathematics, for instance, in the excellent courses provided by the well-known Professors in the Engineering College.

The Forestry School itself consists of a block of buildings attached to the Royal Indian Engineering College, on the brow of Cooper's Hill, near Staines, and looking north over Runnymede and the Valley of the Thames. It is within a convenient distance from London, the traveller arriving at Egham (the nearest station on the London and South-Western Railway) in from forty-five to sixty minutes from Waterloo. Windsor Great Park is within a mile of the beautiful and spacious grounds in which the College stands, and the fine trees of all kinds to be met with in the neighbourhood give to the situation much that is desirable for a centre for the teaching of forest botany, and several parts can be made use of to a certain extent for illustrating subjects in forestry proper.

The building of the Forest School itself consists of large and small class-rooms, a museum, and the well-designed and appointed botanical laboratory. In this block the students pursue their main studies—botany, forestry, and entomology. Their other studies—engineering, surveying, mathematics, geometrical and freehand drawing, physics, geology, and one or two other subjects to be referred to presently—are pursued under the direction of the various Professors in the class-rooms and laboratories of the Royal Indian Engineering College, to which the Forestry School is attached.

The forest museum is a convenient, well-lighted room, rapidly filling with useful collections of specimens illustrating the chief departments of forestry. Among the most valuable and conspicuous objects in this splendid collection may be mentioned the series of European and Indian timbers, which are so disposed that the student has ready access to them, while the Professors are able to refer to them in lecturing, and thus to make the teaching, in the best sense of the word, practical. Then there is a remarkably complete and interesting collection of implements used in forestry, and there are models of timber-slides, apparatus for catching timber, and other forest works, also so disposed that every student can handle and examine them and learn their uses with facility. Another valuable feature in this museum is the series of economic products of Indian plants. This is of course not complete, but the greatest credit is due to all concerned for bringing together for such useful purposes so many instructive specimens of fibres, seeds, barks, fruits, food-materials, &c., from the chief representative Indian plants; and when it is remembered that the Forestry School is so young, in this country (it was started in September 1885), it is the more praiseworthy that the authorities have made such good use of their opportunities and time. The collections must no doubt receive numerous additions as time passes, for it is well known that a museum takes many years to bring within measurable distance of completeness, but the Cooper's Hill museum is already fairly filled, the nucleus of the collections having been derived from the late Indo-Colonial Exhibition, and from the Royal Gardens, Kew. It would require too much space to enumerate the remaining interesting features of these instructive series of forest objects: specimens of timber showing the changes due to abnormal growths, the healing of wounds, the various injuries produced by unsuitable environment or by the attacks of insects and other living organisms, and last, but by no means least, a unique collection showing the ravages of those fungi which injure timber-trees, collected by Prof. Robert Hartig, of Munich, and presented to the School, and a collection of the more injurious forest insects, presented by Herr Oberforstrath Judeich, of Tharand. There is also a small herbarium, of a particularly interesting character, containing an excellent series of Conifers and other trees.

The botanical laboratory has just been completed, and is, without doubt, one of the best designed small laboratories, for its purpose, that we have seen. It consists of an oblong room running east and west, and lighted from the north and east by windows arranged conveniently for work with the microscope. There are also tables and apparatus for experimental demonstrations in vegetable physiology; provision will exist for cultivating seedlings and plants at constant temperatures, for measuring growth, and for exhibiting the influence of light, gravitation, &c., on the growth of plants; and arrangements for showing the quantities of water given off from transpiring leaves, for developing plants in water-cultures, &c. The students are supplied with microscopes, reagents, and accessories, and are taught to familiarize themselves thoroughly with all modern appliances bearing practically on their studies.

The above-mentioned block of buildings also includes one small and one larger lecture-room, which are provided with necessary teaching appliances. The series of botanical diagrams especially are remarkably good, and in fact many of them are unique, being the private property of the Professor of Botany, and drawn and coloured by himself. Another feature which must not be overlooked is the projected botanic garden. This will consist of a series of seed-beds, &c., illustrating the raising of forest trees, and of beds of plants chosen from the most important natural families, in order that the students may familiarize themselves on the spot with their chief characteristics. This botanic garden is now in process of being laid out, and it will be ready for the use of students in a short time.

The courses of studies followed by the forest students are admirably adapted to the wants of practical men

whose lives will be largely spent in the creating, planting, preserving, and using of forest and other trees. Obviously, such a course must comprise several branches of teaching, the one thing common to all being that they bear upon the practical needs of the future forester. That the same training applies to a planter or estate-manager needs no remark, and portions of the course would be suited for others engaged in work in woodlands, and in the colonies, &c. The full course, as at present set forth in the syllabus of studies, is as follows.

The student begins work in September, and attends lectures regularly during two academical years. In engineering, he is taught the principles of road-making, and the building of forest bridges and other structures; he is also instructed in the practice and theory of surveying under the care of the Professor of Surveying. In his first year he studies for two terms under the Instructor in Geometrical Drawing, and in his second year receives lessons in the keeping of accounts. To these subjects may be added freehand drawing, and a modern language. In addition to these more technical subjects, the student attends certain short courses in mathematics and in applied mathematics, under the Professors of these sciences; he also studies physics—in lectures, as well as in the laboratory—entomology, and geology. A short course on organic chemistry is now being commenced.

The rest of his work consists in the special training as a forester, and it may safely be stated that there is no other centre in the Empire where so thorough and excellently designed a curriculum for a forester or planter can be obtained. The two subjects of forestry and botany are under the care of separate Professors. Dr. Schlich lectures on forestry, dividing his subject as follows:—In the first year he deals with the various soils, climates, and the regulating effects of forests on these; sylviculture, artificial and natural woods; the tending, thinning, pruning, &c.; the protection of forests against man and other animals, and especially insects, and against injurious plants, climatic influences, &c. During the second year the student is instructed in the utilization of forests; the technical qualities of woods; the felling, shaping, transportation, &c., of timber; the utilization of minor forest produce; the preservation of wood; sawmills; charcoal, &c. He then passes to the study of working plans, and especially the arrangement of cuttings; surveying and mapping forests; measurement and determination of ages of trees and forests; and the methods of regulating the yield of forests. The final course of lectures is on forest law. In addition to the lectures, the students also make occasional excursions, under the direction of Dr. Schlich; the neighbourhood of Windsor Forest facilitating this important object, and enabling the Professor of Forestry to make his teaching thoroughly practical.

In botany, under the management of Prof. Marshall Ward, the students are instructed by means of lectures, and practical work in the laboratory and in the fields and woods of the neighbourhood. The course in botany is designed to train foresters, not technical botanists: its aim is throughout practical, and directed to teaching the students exact and thorough knowledge of the life-phenomena of the trees and plants which it will be their duty to rear, and take care of, and utilize in the future. Commencing with a short course of thoroughly practical instruction in the elementary biology of plants selected as illustrative types of the vegetable kingdom, the young student is taught the use of the microscope and how to apply it practically in examining the tissues of plants. He is then instructed in the organography and anatomy of plants, learning (not only in lectures, but also in the laboratory and in the field) what the organs of plants *are*, and what they *do*; so that roots, stems, leaves, buds, bulbs, tubers, tendrils, thorns, &c., become to him not mere abstractions, but objects on which his attention will be continually fixed as active parts of plants. The study of cells and their contents, of epidermis and stomata, of vascular bundles and other tissues—of wood, bark, cambium, and so forth—is carried on thoroughly, not only that the forester may know the principles by which to classify and recognize timbers and forest products, and learn their uses, but also that he may understand what these various parts of the plants do in nature: how heart-wood is formed, how the timber grows and may be improved, how wounds may be healed over, how the roots take up substances from the soil, and how the plant makes use of them, and so forth. The student concludes his first year's study in botany (in the early summer) by familiarizing himself with the names and systematic position of the plants in the neighbouring fields and woods, especial attention being paid to the important trees and shrubs, and their relations to the forest flora of India.

During his second year, the student is instructed in the physiology of plants—how they feed, respire, and chemically change substances in their interior; how they grow, and are affected by light, gravitation, temperature, moisture, &c.; how they are reproduced, hybridized, and so on; the effects of various agents in the production of wood, in influencing the fertility, and so forth. The course is completed by the study of the diseases of plants, and especially of timbers, and how their effects may be minimized or healed.

As special features of the greatest importance, it should be mentioned that the senior students pay periodic visits to the magnificent gardens, museums, and plant-houses at Kew, under the direction of Prof. Marshall Ward, in order that their knowledge of the important economic plants and their products shall be real. They see the plants growing, learn to familiarize themselves with their peculiarities and habits and uses, and are thus not strangers to them when they land in India. Secondly, the young foresters are taken abroad, and taught what life in the forest really is. At the completion of their first year's studies, they accompany the Professor of Forestry to Scotland, or to the New Forest, or to the Forest of Dean, as may be decided for the year; and at the end of their second year they are taken to the Continent for three or four months' practical work in Germany and France, to examine the systems pursued in the large and more systematically managed forests of those countries, and thus to study the art of forestry in practice under conditions more resembling those met with in the huge and valuable forests of India.

During the summer of 1887, for instance, the young officers who are now in India were taken to Bavaria, under the direction of Dr. Schlich, accompanied by Prof. Marshall Ward and Mr. Gamble. They visited the

magnificent museum and laboratories of the Forestry School at Munich, the Forest of Freising, the willow nurseries and plantations at Oberberghausen, the spruce forests at Hohenaschau, and the timber depot at Traunstein. They then proceeded to the Austrian forests of the Salzkammergut; and later to the Forest School and school forests at Nancy, the cork oaks and pine forests in the Esterel, and the *Pinus maritima* forests on the west coast of France, used for the preparation of turpentine as well as for timber.

With this practical tour, the training of the young forester in Europe stops, and he departs for India to assume the new duties and large responsibilities of his life as a forest officer under the Imperial Government.

THE BALTIC AMBER COAST IN PREHISTORIC TIMES.

Die prähistorischen Denkmäler der Provinz Westpreussen und der angrenzenden Gebiete. Von Dr. A. Lissauer. (Leipzig: Engelmann, 1887.)

THE prehistoric antiquities of that part of the Baltic coast that lies about the mouth of the Vistula have something more than a local interest. The old Prussian shore—the land of the Æstii of Tacitus and Cassiodorus, of the Estas of King Alfred—had already in very early times a European importance in its connection with the widely ramifying amber commerce of antiquity, of which this was in historic times the richest field of production. The present work by Dr. Lissauer, the President and founder of the Anthropological Section of the Natural History Society of Danzig, is peculiarly welcome as giving in a thoroughly scientific form a summary of the results of the archæological discoveries made in recent years relating to the prehistoric period in the province of West Prussia and its border districts. The author has divided the work into several sections, corresponding to the Neolithic, Hallstatt, and the successive Iron Age periods, and has accompanied each with an excellent synoptic list of the various individual finds.

Of the earlier Palæolithic Age there are, of course, no remains in this Baltic tract, which was still covered with an ice-sheet at a time when primæval man had already begun to tenant the caves of Cracow. As the ice retreated there was formed the great glacier stream at present represented by the Vistula, but which then prolonged its course to the west, and, joining with the Elbe, poured its waters into the German Ocean. The physical event which in this region dominates all the succeeding history is the breaking through of the Vistula at Fordon, near Bromberg, and the formation of the new channel by which it poured itself henceforth into the Gulf of Danzig; and this, geologically speaking, was a comparatively recent consummation. The author has reproduced an elaborate calculation of Jentzsch, based on the formation of the delta and the average amount of sediment conveyed by the waters of the Vistula, according to which the breaking through of that river to the north must have taken place approximately about 3000 B.C. That the Neolithic immigration into the Old Prussian land from the south must have taken place at an early period is seen from the local distribution of these remains, which tends to show that the ice and snow still lingered on the higher parts of the country. On the other hand, from the fact that Neolithic settlements are peculiarly abundant in the old bed of the Vistula itself, Dr. Lissauer concludes that this immigration did not take place till well after the date when the river had taken its new course. Here, too, as elsewhere, we find the same entire revolution in the character of the Neolithic fauna as contrasted with the Palæolithic group of the Polish caves for example. Not a single representative remains. No reindeer bones even have been discovered on the Neolithic sites of the lands of the Lower Vistula, though the remains both of aurochs and of bison have been found.

Among the most interesting and characteristic objects that appear in association with the Neolithic deposits of the Lower Vistula are certain rude representations of human and animal figures cut out of amber. These remarkable productions, perforated as if for suspension, and engraved with fine lines, are more frequent to the east than to the west of the Vistula mouth; but one of the most striking, a figure of a boar, ranked by Virchow amongst the best relics of the plastic art that have reached us from the Stone Age, was found in the neighbourhood of Danzig. These amber men and animals have been the object of a special study by Dr. Tischler, of Königsberg, whose researches into the prehistoric remains of East Prussia are the complement to those of Dr. Lissauer in the Western Province. In his admirable papers on the Stone Age in East Prussia, Dr. Tischler has shown that these figures are characteristic of an extensive East Baltic region; they have been found in the same shapes and with the same perforations, but cut out of bone and stalagmite instead of amber, in the Polish cave of Pod-kochanka; and, what is still more remarkable, bone figures of analogous character have been discovered amidst the remains of a Neolithic station, described by the Russian explorer Inostranzeff, on the shores of the Lake of Ladoga. From these and other parallels, Dr. Tischler has been able to establish the existence of a distinct East Baltic Stone Province extending from the Oder to the Lake of Ladoga, and in all probability to the Onega shores, and including not only the provinces of East and West Prussia but the greater part of Poland. The relation of these northern "idols" to the clay figures of men and animals found in the Swiss lake-dwellings, in the pile settlements of Laibach, and some of the prehistoric sites of Hungary and Transylvania, where one has been found of alabaster; and the relation again of these latter to the "Pallas" of Dr. Schliemann's Trojan excavations, or the rude "Carian" and Cypriote figures, suggest wide and far-reaching inquiries on which it is impossible here to embark.

Of the Bronze Age, pure and simple, there are very scanty remains in these East Baltic coast-lands; though there are sufficient examples, both of Hungarian and West Baltic forms, to show that before the close of the period in Central and North-Western Europe its arts were already taking root in this region. Dr. Lissauer's remarks on what he terms the "so-called Bronze Age," but which, in the greater part of our Continent at least, represents a very well defined stage of culture, reflect an attitude of mind not yet wholly extinct amongst German scholars. How far the Hallstatt culture can in this district be regarded as the immediate successor of that of Neolithic times is a

question, however, of comparatively secondary importance. The main fact with which we have to deal is that it is only in the transitional age that takes its name from the great Salzkammergut Cemetery, and when iron was already coming into use, that we have the evidence of intimate and extended relations between the Amber Coast of the Baltic and the lands to the south and south-east. The importance of this fact in its bearing on the early course of the amber trade does not seem to me to be clearly brought out by Dr. Lissauer. Montelius, however, has conclusively shown that throughout the earlier and purer Bronze Age in Central and North-Western Europe the source of the amber supply was not the East Baltic, but the coast of Jutland and the mouths of the Elbe and Weser, where, as Müllenhoff has demonstrated on purely literary grounds, lay the Amber Islands of Pytheas. The main course of this early commerce, as indicated by the connection of the Bronze Age forms discovered, was up the course of the Elbe; and the first appearance of an intrusive southern culture at the Vistula mouth in Hallstatt times shows that it was not till this comparatively recent period that the Baltic amber route was opened up. But, when once this, then probably as now, far more prolific field was known, southern commerce showed more and more a tendency to follow this route, to the final desertion of the older line to the north-west. Among the most characteristic evidences of the trade relations thus established between the Old Prussian Amber Coast and the Mediterranean may be cited the discovery of a "cordoned" bronze bucket of the class common to Northern and Southern Italy, and of which large finds have come to light in Southern Hungary,—a class of objects which there seems no longer any warrant for qualifying, as Dr. Lissauer does, as "Etruscan," but which, as Helbig has shown, may very well represent an old Chalcidian fabric. A whole succession of finds of Greek coins further mark in somewhat later times the continued intercourse with the south. Dr. Lissauer apparently accepts the much-disputed Schubin find of Archaic coins of Athens and Erchomenos, and though the inclusion in this sixth century hoard of two later pieces of Athens and Miletus, and a modern Siamese coin, render the circumstances of the find open to grave suspicion, the later series of discoveries of coins of Thasos, Macedon, &c., extending from Hungary to Gothland, throws a retrospective light on the probable direction followed by one branch of this Baltic commerce. It appears equally clear, however, both from archæological and historic sources, that another line crossed the Julian Alps to the head of the Adriatic, finding in all probability its southern continuation by the East Adriatic coasting route. This, it will be remembered, was the route followed by those who, in Herodotus's account, conveyed the mysterious gifts of the Hyperboreans to the Delian shrine of the Sun-god—a mission which seems to have an inseparable connection with the "Sun-stone" Islands of Eridanos's mouth and the Phaethontid maidens.

Among the most interesting and characteristic features of the Hallstatt period in West Prussia are the "face-urns," or cinerary vases with human features rudely modelled on their neck; and Dr. Lissauer is probably on the right track when he compares them with the early vases of the same kind discovered by Fräulein von Torma in the Valley of the Maros in Transylvania. That they have any relation with the "face-urns" of Etruria seems out of the question, especially since the appearance of the monograph of Prof. Milani, tracing the evolution of the developed Tuscan type from an earlier class of cinerary vases with funeral masks attached to them. But the parallels from the Maros Valley may be more plausibly regarded as supplying an intermediate link in space and time between the face-urns of the Baltic coast and those of prehistoric Troy. In other respects the ceramic forms that occur in West Prussia and its borderlands during this period, such as the "twin" and painted vases, show strong southern and south-eastern affinities; while the occurrence amongst the ornaments of *Cypræa moneta* and *Cypræa annulus* from the Red Sea and Indian Ocean point to still more extensive eastern relations. Cowry ornaments, it may be worth observing, are of frequent occurrence in the prehistoric cemeteries of the Caucasian region, and there is here perhaps an indication of old Pontic communications by the Dniester or Dnieper Valleys—lines of intercourse which Dr. Lissauer does not seem to have kept sufficiently in view.

The Hallstatt culture on the Old Prussian shore is in its turn cut short by that to which we in England give the name of "Late Celtic," but which on the Continent passes by the name of La Tène from the Swiss station of that name. The Roman taste for amber ornaments subsequently gave a great impulse to the commercial intercourse between south and north *viâ* the Pannonian frontier station of Carnuntum, and we have abundant evidence of the progress of Roman provincial arts on the Lower Vistula. The finds of Roman coins become more and more frequent, and culminate in the reign of Severus, after which time they as suddenly fall off. There can be little doubt, as Dr. Lissauer has suggested, that this sudden break in the commercial relations with the south is due to the great migration of the Gothic tribes, who had before this time established themselves in this part of the Baltic coast, to their new seats on the shores of the Black Sea and Trajan's Dacia. Into the depopulated lands west of the Vistula the new tide of Slavonic settlement now poured, while the older branch of the Litu-Slavic race, the Æstii or "Old Prussians," still held their own on the Amber Coast to the east of the river-mouth, as we know from the offerings made by them to King Theodoric. The last section of Dr. Lissauer's work is directed to this Wendish period of East Baltic history, to the "Burgwall" and the "Bergwall," the pile-dwellings, the characteristic pottery and ornaments of the primitive Slavonic race, and to the monuments of their rising commerce with Byzantium and the Arabian East. To a somewhat later date, perhaps, may be assigned the curious stone figures included by Dr. Lissauer in an appendix to his Neolithic section, and as to the date and origin of which he refrains from conjecture. There can, however, as the author himself admits, be no reasonable doubt that they belong to the same category of monuments as the well-known *Kamienne baby* or "stone wives" of the Russian steppes. They extend, in fact, in an unbroken zone through Poland and Lithuania to the steppes of the Dnieper and the Sea of Azoff, and find their analogies in Central Asia and in the rude stone figures on the Siberian

kurgans. As to the ethnic character of the people who spread them over this vast Scythian region, we have the direct testimony of the traveller Rubruquis, who, when visiting the Polovtzi or Kumans—the scourge of mediæval Russia—actually witnessed their erection over the grave-mounds or kurgans of that race. Their Turko-Tataric origin is indeed entirely borne out by their physiognomy, which, as I have myself had occasion for observing in various parts of Southern Russia, is of an unmistakably Mongolian cast, and their dress and accoutrements thoroughly bear out this identification, the head-gear in some instances being identical with that still worn by some Tekke-Turkomans. Individual divergences of type in some of the western examples may of course show that these Mongolian images were imitated by Wendish or Old Prussian, Polish, or Lithuanian hands. Two things, however, may be regarded as certain: that the stone figures of the steppes are of Turko-Tataric origin, and that the date of their Baltic reproductions is considerably later than Neolithic times.

ARTHUR J. EVANS.

VOLTAIC ELECTRICITY.

Voltaic Electricity. By T. P. Treglohan, Head Master, St. James's Science and Art Schools, Keyham, Devonport. (London: Longmans, Green, and Co., 1888.)

ONE occasionally hears of the evil effects of cram and bad teaching which the system of examination and payment by results so extensively made use of by the Science and Art Department is supposed to encourage. If such books as the above are in general use by teachers or candidates, it cannot be denied that the evil is very serious.

There is little of reasoning or explanation anywhere; but, instead, there are strings of statements which would, if they were accurate, consist of ready-made answers for such questions as may be set for the first stage or elementary course of voltaic electricity. At the end of the book will be found the elementary questions in voltaic electricity for the last twenty years, with numbers attached showing the pages where the answers may be found.

The book professes to be largely experimental, and the student is urged to make the apparatus and to try the experiments described. A few extracts will show how utterly misleading it is in this respect.

If the tongue is placed between a penny and a half-crown, "a feeble spark is seen as contact is made between the two metals."

"The missing Zn" (owing to the action of a voltaic cell) "is found in the cell, either in the liquid or at the bottom, as a grayish-coloured deposit."

"This" (the bichromate) "was a strong cell, and was tolerably constant; but, after a short time, was weakened in consequence of crystals of chrome alum forming in the liquid. To prevent this crystallization, the liquid must be frequently disturbed, either by lifting the plates out of it, or by some other means."

After speaking of the Daniell, Bunsen, and Grove, the author describes the Leclanché as "another very constant cell."

To show that zinc and carbon have a greater E.M.F. than zinc and copper in a cell, a condenser and two electroscopes are recommended to be used. In the figure the plates are shown separated and connected each to the zinc or copper and to one electroscope, of which the leaves are widely divergent. The student is not told that the connections must not be so made, nor is any practicable method of making the experiment described.

"In brine the positive and negative elements have the same relative order as in dilute acids; but in ammonia the relation is reversed, and those that were negative in the former case will be positive in the latter."

"It is found that the wire attached to the Cu, C, or Pt has free statical electricity apparent at its terminal, which repels the glass rod rubbed with silk, and that attached to the Zn free statical electricity, which repels the sealing-wax rubbed with flannel."

This extraordinary statement appears five times in a few pages.

Three or four Grove's cells are "necessary" to electrolyze acidulated water; the hydrogen gas collected in one of the tubes of a voltameter explodes "with a tolerably loud report." When a solution of common salt is electrolyzed, "the sodium of the salt and the hydrogen of the water" (appear) "where the current leaves the cell."

"Another simple experiment is to send the current through a solution of iodide of potassium. A brown substance—iodine—is seen at the anode, and the metal potassium at the cathode."

It is doubtful what some passages mean, as for instance, the paragraph:

"If any number of plates be used together, the E.M.F. of such a cell would be the result of the difference of potential of the two plates which are furthest apart in the electromotive series."

Frequently, the language is more than careless; thus, after speaking of sulphuric acid and sulphate of copper, the author says *other* binary compounds; and, after describing the action of a solenoid, he says coils and helices *also* exhibit magnetic properties.

Those expressions of doubtful meaning—intensity and quantity—are freely used, as is the word potential, which fortunately has not its meaning explained. The names of some of the units are met with for the first time in the sentence: "Current strength is calculated in amperes, electromotive force in volts, and resistan*ces* in ohms." Not a word of explanation is given.

NATURAL HISTORY OF VICTORIA.

Prodromus of the Zoology of Victoria. Decades 1–15. By Prof. F. McCoy. (Melbourne, 1878–87.)

JUST ten years ago, Prof. Frederick McCoy decided, under instructions from the Victorian Government of the day, to commence the publication of a series of short descriptions, accompanied by coloured figures, of the indigenous members of the different classes of the animal kingdom. These were to be published in parts containing ten plates in each, which have appeared with commendable regularity to the present time. As the fauna of Victoria was not as well known as its flora, it was a necessary preliminary, in order to effectually carry out such a scheme, to have a large number of drawings made, as opportunity arose, from the living or

quite recent examples of many species of reptiles, fish, and the lower animals, the true characters of which, in many cases, were but imperfectly known, from their having been described from often badly-preserved specimens.

The value of such a work will be readily granted, and the energy of the Victorian Government will be duly appreciated by those of us in the mother country who know the difficulty there would be in our obtaining Government sanction for the publication of like descriptions of the animal inhabitants of these islands.

Of all the forms described and figured in these decades, the originals are preserved in the National Museum at Victoria. The first volume was completed with the tenth decade in 1885, and it forms a large octavo volume of 100 plates and over 200 pages, with a classified index. Since then, Decades 11 to 15 have been published, bringing the date to last year.

On this important work, which we fear is not so well known in this country as it ought to be, we venture to make the following remarks. Of the century of plates forming Vol. I., fifty-four are illustrative of vertebrate forms, and forty-six of invertebrate ones. Of these latter no less than twenty-eight are exclusively of Polyzoa, which seems to us a somewhat unfair treatment of the other groups. We cannot object to it on the score of the advancement of science, but we think we justly may, so far as the usefulness and interest of these decades are for the public. Another criticism, and we have done: the references to where the species have been described are for the most part useless. For example, to the species figured on the 100th plate, *Goniocidaris tubaria* (Lam.), where we find "*Cidarites tubaria* (Lamk.), Anim. sans Vert.," there is not another word added, and this reference is not only defective but erroneous. This is a subject that ought to be attended to: we do not demand a full and detailed synonymy, but would, in such a publication, be content with just such information as would enable a student to see where the generic and the specific names adopted by the author were to be found first described; and to give this, few would be better qualified than Prof. McCoy.

With scarcely an exception, the plates have been exceedingly well executed; those on insects by A. Bartholomew demand a special word of praise, and the same artist has also done full justice to the fishes and the Mollusca, the plates representing the "tuberculated argonaut" being nearly perfect. Another artist whose work we may allude to is Dr. Wild, well known in connection with the *Challenger* Expedition; among the drawings executed by him, that of the Australian fur seal, a group with the adult male, female, and a cub, is worthy of praise.

The descriptive details vary, as might be expected, in interest; sometimes we have most interesting and full accounts of the life-history of the species, as notably in the cases of the fur seals just referred to, of the case moths (Metura), the bell frog, the great cicada, and others too numerous to mention; and were our space unlimited we would gladly show how all-sided is the information to be gained from these decades. The following will serve as an example. A common moth, first described from New South Wales by Lewin as *Phalænoides glycine*, from the larvæ feeding on the leaves of a leguminous plant (*Glycine bimaculata*), is equally common in the colony o Victoria, but there the larvæ fed on *Gnaphalium luteo-album*, a common weed. Since the planting of vineyards this moth has increased in enormous numbers, and the larvæ have completely abandoned their original food, and now devour only the leaves of the grape vine, on which the moth multiplies beyond measure. It is a puzzle how the female moth was guided to deposit her eggs on a plant of so different a character from that which she had been accustomed to, and which must have been to her unknown. The injury done to the vineyards of Victoria by this insect is enormous, and would seem, in spite of many remedies, to be increasing. Insectivorous birds will not eat the marauding larvæ; and children, who might keep down the plague by hand picking, must, by law, attend their schools.

We hope to again notice these decades on the completion of the second volume. In the meanwhile we have said enough to call our readers' attention to the value and interest of the information which they contain; and we congratulate Prof. McCoy and the Victorian Government on their publication. E. P. W.

OUR BOOK SHELF.

Technological Dictionary. In 3 vols. English-German-French, French-German-English, German-French-English. Third Edition. (London: Trübner and Co., 1888.)

THE inventions and discoveries of the present century have introduced a very considerable number of new words into the various languages of the world, but more especially into the European languages. As these words do not occur in ordinary dictionaries, special dictionaries embodying them are necessary to a great number of persons. Thus, to facilitate communication in commercial transactions between one country and another, and to enable students of science and technology to profitably consult works written in languages other than their own, they are indispensable. As regards the three principal languages of Europe, this want is supplied by the work before us, the third edition of which has recently been completed by the publication of French-German-English, and German-French-English volumes. The third edition of the English-German-French volume was published in 1878. The first edition dates as far back as 1852, and since then the work has been thoroughly revised and new matter added.

The work embraces the terms employed in the arts and sciences, engineering, architecture of every description, navigation, astronomy, meteorology, mining, artillery, &c. In addition to the terms relating to the various appliances, processes, and substances, there are also those applied to the different orders of people concerned with them, from the "doffer" of the spinning mill to an "Admiral of the Fleet." Teachers of scientific and technological subjects will also find the equivalents of the great majority of the terms they find it necessary to employ, the names of chemicals and minerals included. The work is wonderfully comprehensive, and the arrangement is all that could be desired.

The best authorities have been consulted, and tedious processes adopted, with the view of obtaining indisputable accuracy, and this has practically been accomplished. No effort has been spared to make the work deserving of the important place in literature which it should naturally occupy, and no recommendation of ours is necessary. It certainly ought to be available for reference in all libraries of any importance. A. F.

Transactions of the Sanitary Institute of Great Britain. York Congress, 1886.

THE valuable work done by the Sanitary Institute cannot be altogether gauged by the annual volumes of Transactions, one of which now lies before us. It must be remembered that, besides the reading of papers and holding of discussions on subjects of sanitary interest, the Sanitary Institute endeavours, by means of its Congresses and annual Exhibitions, to arouse the interest of the inhabitants no less than of Town Councils and municipal authorities in the health and well-being of the towns visited. That such visitations have a beneficial influence, by awakening public interest in measures of sanitary reform, both local and general, can hardly be doubted; and, as pointed out by Sir Spencer Wells in his Presidential Address, if further legislation on sanitary matters is not to be ridiculous, it must be accompanied by increased knowledge on the part both of the persons charged with administering the Sanitary Acts as well as of the public themselves.

The modern science of hygiene is hybrid, embracing as it does special branches of most of the leading sciences—medicine, engineering, architecture, geology, chemistry, meteorology, &c. The subjects treated of by means of papers in such a Congress must be very varied, and such we find to be the case; but as far as possible the papers are relegated to one of three sections, where their merits will be best understood and most adequately discussed. The standard of the papers submitted to the York Congress is fully up to the average, many of them treating of subjects of wide interest, or having important bearings on the prevention of disease and maintenance of the public health.

Science Sketches. By David Starr Jordan. (Chicago: A. C. McClurg and Co., 1888.)

IN this neat and handy little volume we have a very interesting and intellectual collection of sketches and addresses more or less scientific. Some of the articles, which, as the author tells us, have been published before, have been freely retouched or re-written; but the papers on "The Dispersion of Fresh-water Fishes," "The Evolution of the College Curriculum," and the address on "Darwin" appear for the first time. The subjects treated are of various kinds, so that anyone who takes up the book will be sure to find in it something that will interest him. The appendix contains a list of the scientific papers of the author, and we hope it will not be long before we are favoured with another such book as the above.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

"Coral Formations."

SINCE writing the letter published in NATURE, March 22 (p. 488), I have checked Mr. Ross's figures. The result is somewhat surprising. Instead of 8400 tons of carbonate of lime removed from $12\frac{1}{2}$ square miles of lagoon representing a sheet half an inch thick, it really only amounts to a film of that area $\frac{1}{252}$ of an inch thick.

At this rate per annum it would in round figures take eighteen thousand years to dissolve out a lagoon a fathom deep, or a *million* years for the creation of a lagoon 60 fathoms deep. When we consider that this could only happen on the impossible assumption of the atoll remaining stationary for a million years, while no accumulation of coral sediment or organic calcareous growth took place in the lagoon, it is at once seen, on the showing of its own supporters, how impotent is the solution theory to account for the formation of lagoons in atolls.

To represent the figures in a familiar way, I may point out that the film removed annually would be a little less in thickness than one of the pages of "Prestwich's Geology." A volume of 36,000 pages (18,000 leaves), minus covers and well pressed, would be a fathom thick. No one acquainted with my geological work will accuse me of being parsimonious of geological time, but this is really beyond my mark altogether.

Mr. Irvine asks (NATURE, March 29, p. 509): "Can Mr. Reade give any observations or figures in support of his view of the rate of accumulation of oceanic calcareous deposits?"

If Mr. Irvine will refer to Mr. Murray's paper (NATURE, vol. xxii. p. 352), he will see that the pelagic life in a square mile of ocean water 100 fathoms deep is estimated by him to represent sixteen tons of carbonate of lime.

I am not aware of the length of life of such organisms, but if they lived on an average *only one day*, and the whole of their tests were rained down on to a submarine peak at the rate of sixteen tons per diem, *and none were dissolved by sea-water*, it would take twenty-nine years to accumulate 1 inch in thickness of solid carbonate of lime in this pelagic cemetery. In this way, if anything so improbable were to happen, a submarine peak half a mile below the range of coral growth might be levelled up into a suitable platform in 900,000 years. I could add much more, but respect for your valuable space bids me conclude.

T. MELLARD READE.

Park Corner, Blundellsands, April 3.

"The Dispersion of Seeds and Plants."

IN support of the views expressed in Mr. D. Morris's interesting article on the above subject (NATURE, March 15, p. 466), I beg to be allowed to state the following facts. In the Island of Porto Rico, the *Panicum barbinode*, called there "malojilla," has been cultivated for many years in the low humid lands, and it is a current opinion among farmers that it is reproduced by means of the animals feeding on it. Some fruit-bearing trees and shrubs, which are a favourite food for the wild *Columba leucocephala* and *Columba corensis*—among them the *Solanum stramonifolium*, the *Bucida Buceras*, the wild coffee, *Coffea occidentalis*, the palm-tree, *Oredoxa regia*—appear in some mountains and regions where they were formerly unknown, and there is no doubt that they have sprung from fruits and seeds transported by these pigeons. The *Anona muricata* (soursop), the *Anona reticulata* (custard apple), the *Carica papaya* (papaw tree), whose hard seeds are sometimes uninjured by the processes of mastication and digestion, are also believed to be planted accidentally by birds, and sometimes by hogs, horses, and other Mammalia. They grow all about in pastures where these animals are fed. The statement made about the orange-tree in Jamaica also holds good for Porto Rico. Very few orange-trees were planted in the interior of the country, and the tree is now wild in all that zone by the agency of birds in great part. There is no doubt, as Mr. Morris says, that birds and cattle have been the means of distributing plants all over the island.

ANTONIO J. AMADEO.

"Balbin's Quaternions."

NATURE of December 15, 1887 (p. 145), which has lately reached me, contains a notice of a treatise on Quaternions, by Prof. Valentin Balbin, in which the reviewer alludes to the "slight alterations" introduced into the notation of quaternions by Messrs. Houel and Laisant, and apparently visits them all with equal condemnation.

To me it appears that a distinction should be made between the two points in which the French notation differs from the English. The use of letters in different type to denote different kinds of quantities, the same type being always reserved for the same kind, seems to render the processes sometimes clearer and the results more immediately and easily available for students. In spite, therefore, of the ugliness of the black-letter symbols, it would not perhaps be altogether a loss if English mathematicians would adopt this part of the French scheme.

The other change introduced by M. Houel, that of the order of the factors, writing $q\,q'$ where Hamilton writes $q'\,q$, seems, on the contrary, to be an entirely retrograde step. That, as a rule, the symbol for the operator should be written before that of the operand, is a necessity in all modern symbolic processes. The alteration can only lead to confusion. In my "Text-book of Algebra" I have suggested that while the symbol $a \times b$

should be read, a multiplied by b, the symbol $a.b$ or ab should mean a multiplied into b, so that $a \times b$ and $b.a$ or ab are identical. Perhaps a compromise might be effected on this basis in the notation of quaternion multiplication.

The "unnamed French mathematician" who is quoted in the notice in question as asserting that quaternions have no sense in them, is stated by M. Laisant to have been M. Prouhet, and to have expressed this opinion in the *Nouvelles Annales de Mathématique* (1863, p. 333), in reviewing the first work published in French on quaternions, the author of which was M. Allégret. W. STEADMAN ALDIS.

Auckland, New Zealand, February 20.

Mr. Crookes and the Transformation of Heat-Radiations into Matter.

PROF. CLIFFORD, in the *Fortnightly Review*, June 1875, wrote as follows:—"But if the ether did absorb light, what would this mean? Vibratory motion of solids, which is really a molecular disturbance, is absorbed by being transformed into other kinds of molecular motion, and so may finally be transformed to the ether. There is no reason why the vibratory motion of the ether should not be transformed into other kinds of ethereal motion; in fact, there is no reason why it should not go to the making of atoms" ("Lectures and Essays," by W. K. Clifford, vol. i. p. 246).

Mr. Crookes, in his Presidential Address to the Chemical Society, March 28, brought forward a somewhat similar hypothesis, for he says:—"If we may hazard any conjecture as to the source of energy embodied in a chemical atom, we may, I think, premise that the heat-radiations, propagated outwards through the ether from the ponderable matter of the universe, by some process of Nature not yet known to us, are transformed *at the confines of the universe* into the primary—the essential—motions of chemical atoms, which, the instant they are formed, gravitate inwards, and thus restore to the universe the energy which otherwise would be lost to it through radiant heat."

The hypotheses will be seen to be exactly alike, except for the speculation introduced by Mr. Crookes of the transformation taking place "at the confines of the universe." What do we know of the confines of the universe? Nothing. Are we *now* to begin building up hypotheses on such foundations—foundations concerning which we know nothing, and are not likely to know anything *for some time*? HUGH GORDON.

Royal Institution, March 31.

Green Colouring-matter of Decaying Wood.

MR. IRVING writes (p. 511): "After an examination of thin sections" of the decaying wood "with the microscope, I am unable to trace this to any saprophytic organism."

I have at the present time a coccus, I suppose, growing on the surface of nutrient gelatin, which is stained a beautiful green, highly fluorescent, by the colouring-matter absorbed by it from the micro-organisms. The cultivation is in a test-tube of nutrient gelatin inoculated by scratching the surface of the gelatin with a needle which had been rubbed on a colony, isolated by plate-cultivation, and obtained from a bad water.

The growth, a greenish-white, is entirely on the surface—so entirely that the scratches made in inoculating the gelatin are still visible, three weeks after the inoculation, and the gelatin is perfectly transparent.

Under these circumstances it is quite certain if I made sections of the green gelatin no micro-organisms would be found in them.

It may be that the decaying wood is stained in the same way by colouring-matter absorbed by the sap or the moisture of the wood by micro-organisms growing on its surface.

It is a fact, I believe, that the colouring-matter formed by chromo-genic micro-organisms does not reside in their structures but in the interspaces between them, so it would naturally be absorbed by any solvent they were in contact with, while the organisms themselves might remain entirely on the surface, as they do in the case of my gelatin.

This may explain why Mr. Irving has failed to find micro-organisms in the sections he made. Further, if the colour of his green wood is caused by the same micro-organism which stains my gelatin green, it is a very small one, so small that, with a 1/15-inch oil-immersion object-glass, and a No. 12 compensating eye-piece of Zeiss, I find it very difficult to decide what shape it is, but I think it is not spherical.

I think Mr. Irving will find references to the literature of chromo-genic micro-organisms in Crookshank's "Manual of Bacteriology." HENRY ROBINSON.

The University Chemical Laboratory, Cambridge.

Comet *a* 1888 (Sawerthal).

I SAW comet Sawerthal to-day at 3.40 a.m., with power 20 on a $4\frac{1}{4}$-inch refractor. It was about 50′ immediately below θ Pegasi, and had a bright broad tail, which I could only distinguish to a length of 65′, on account of the twilight, moonlight, and the comet's low altitude. I thought the tail was slightly curved, concave to south; it pointed on the average to a little above ν Pegasi, or at a position-angle of about 260°. The total light of the head was considerably fainter than θ Pegasi, and considerably brighter than ν, so that it would be from 4 to $4\frac{1}{2}$ mag.; but owing to the unfavourable conditions I could not see it with the naked eye. T. W. BACKHOUSE.

Sunderland, April 3.

THE HITTITES, WITH SPECIAL REFERENCE TO VERY RECENT DISCOVERIES.[1]

II.

THE monuments at Boghaz-Keui and Eyuk are on the east of the River Halys; and it seems doubtful whether there is evidence that the country inhabited by the Hittites extended much, if at all, beyond a line drawn from Sinope on the Euxine to the most westerly bend of the Halys, and continued through the peninsula to the Mediterranean. No doubt sculptures with Hittite characteristics have been found further to the west, as at Giaour-kalessi, in Phrygia, and at Karabel, near to Smyrna and to the coast of the Ægean; but as yet it does not appear certain that these sculptures denote permanent occupation, or that they are more than monuments of successful military expeditions. The Euphrates may be taken as marking vaguely the eastern boundary of the Hittite land. To the south, in Syria, the Hittite country certainly extended as far as Kadesh, a site on or near the present Lake of Homs.

It is with the inscriptions and the engraved seals found in, or connected with, the district I have indicated that Hittite researches are mainly concerned. The few characters on the monument at Karabel are too far obliterated to be, for the present, of much importance. In 1812, Burckhardt visited Hamah, the ancient Hamath, in Syria. He saw in the corner of a house in the bazaar "a stone with a number of small figures and signs, which appear to be a kind of hieroglyphical writing, though it does not resemble that of Egypt" ("Travels in Syria," Lond., 1822, p. 147). But, as Dr. Wright remarks, "so little interest was taken in his discovery, even by professional explorers, that Porter, in Murray's 'Hand-book,' so late as 1868, declares 'there are no antiquities in Hamah'" ("Empire of the Hittites," 2nd ed., p. 1). There were, however, other inscriptions at Hamah besides that noticed by Burckhardt, and the stone bearing one of these was supposed to possess mysterious properties, efficacious for the cure of spinal disease, so that "deformed persons were willing to pay for the privilege of lying upon it, in the hope of a speedy cure" (Burton and Drake, "Unexplored Syria"). After sixty years, or nearly so, from the time of Burckhardt's discovery, attention was called to the Hamath inscriptions, first by Mr. J. A. Johnson, United States Consul at Beyrout, and subsequently (1872) by Capt. R. F. Burton and the late C. F. Tyrwhitt Drake, in the work just quoted. In 1872, however, Dr. Wright was successful in obtaining casts of the Hamath inscriptions, while the originals were trans-

[1] Based on Lectures delivered by Mr. Thomas Tyler at the British Museum in January 1888. Continued from p. 514.

mitted in safe custody to Constantinople. The arrival of Dr. Wright's casts in this country was naturally followed by attempts at the decipherment of the hieroglyphics, though some of those who made these attempts had previously concerned themselves with the imperfect representations given by Burton and Drake in the work already mentioned. About the same time (1872) attention was called to the inscription then existing at Aleppo, but since unfortunately destroyed. Not very long afterwards an interesting bas-relief at Ibreez, in Lycaonia, accompanied by an inscription, was brought anew under notice by the Rev. E. J. Davis; and in 1876 Prof. Sayce observed with reference to this inscription, "The Hamathite hieroglyphics appear to have been an invention of an early population of Northern Syrians. Their occurrence in Lycaonia is probably due to Syrian conquest." Still later, and in view of the sculptures of Boghaz-Keui and Karabel, together with other monuments, Prof. Sayce took a much wider view, extending Hittite presence and influence through Asia Minor. Not long after taking this more extended view of the Hittites, the same scholar made a discovery of no small importance with regard to the decipherment of the inscriptions: I allude to the discovery that certain characters on the seal of Tarkutimme were Hittite hieroglyphics (*Academy*, August 21, 1880). The true nature of these hieroglyphics had not been previously seen, though, together with the cuneiform inscription round the circumference, they had been discussed by the late Dr. Mordtmann.

The history of this now celebrated seal is certainly remarkable. About the year 1860, a convex silver plate bearing the inscriptions just alluded to was presented for sale at the British Museum. Doubt was entertained concerning the genuineness of this silver plate as an antiquity, and the purchase was declined, though an electrotype copy was made and preserved. On account of the prolonged interval which has elapsed, the precise ground of doubt is not now altogether clear. It seems not unlikely, however, that the decision as to the spuriousness of the plate was arrived at after several considerations had been duly weighed. The silver of the plate may have seemed too well preserved for an object of so great antiquity, unless, indeed, it had been in some special manner sealed up and protected in a vase or other receptacle. Then the character of the engraving was probably regarded as inconsistent with the idea of its having been cut originally in silver, and especially in a comparatively thin silver plate, the engraving being rather that of stone, on which material, indeed, a seal was much more likely to be engraved. And another important fact, as agreeing with this view, is a flaw which appears on the right hand of the central figure, and which suggests the fracture or chipping of stone rather than the abrasion of metal. On these grounds, probably, the conclusion was arrived at that the plate was not a genuine antiquity.[1] Most likely it was a cast or electrotype from an ancient stone seal, this seal having been retained by the discoverer—whoever he may have been—with the view of obtaining eventually a larger profit by its sale. Proceedings of this kind are not unknown at the Museum. But where the original seal then was, or now is, has never been known; and the silver plate offered at the Museum has likewise disappeared from view. But this disappearance is of little importance to science, if the genuineness of the inscriptions can be fully proved.

In favour of this genuineness it must be urged as improbable that any from among the few Assyriologists who were to be found in Europe nearly thirty years ago would have co-operated in forging the seal. Moreover, there are two difficulties in the way of believing in such a forgery. First, the cuneiform legend has peculiarities which distinguish it from any other known type of cuneiform writing, as was observed by Dr. Mordtmann. Then there is an interspace over the head of the standing figure, which might seem, at first sight, to be interposed between the beginning and end of the cuneiform legend. It occurs, however, in the middle of a word. For this interspace a possible reason may be derived from the recently-discovered Yuzgât seal; but its occurrence does not suggest the idea of forgery by a scholar conversant with the cuneiform characters. Supposing, however, that these difficulties are put aside, there remains the much stronger argument furnished by the characters in the central space, which are certainly Hittite. Now, and for some time past, great interest has been displayed in relation to Hittite inscriptions; but in and about the year 1860 the case was far otherwise. There was then no temptation to forge these Hittite inscriptions on the seal, even if it had been possible to do so. But it may be doubted whether at the time in question it would have been possible to find and bring together the various Hittite characters. Besides, it is not difficult to discern a concinnity and agreement between the cuneiform and Hittite adverse to the idea of forgery, and consistent only with the opinion that the seal is, with its inscriptions, a veritable bilingual.

FIG. D.—Bilingual seal of Tarkutimme (enlarged).

About the time already mentioned (1860), Dr. Mordtmann examined at Constantinople a convex silver plate, then in the possession of M. Jovanoff, probably the identical plate which was offered at the British Museum. Concerning this plate, Dr. Mordtmann wrote, with date "Constantinople, December 6, 1861," a contribution to Grote's "Münzstudien," entitled "Sceau de Tarkoumdimmi, roi de Tarsous"; the designation "Tarkoumdimmi, roi de Tarsous," being Dr. Mordtmann's reading at that time of the cuneiform inscription on the seal. He noticed also, very appropriately, the resemblance of the name on the seal to the names Tarcondimotus, Ταρκονδίμοτος, as found in Tacitus, Strabo, and Dio Cassius, names employed to denote a father and son, Cilician kings who reigned in the time of Augustus. Dr. Mordtmann mentioned, also, that Plutarch gives, instead of the longer form, the shorter name Ταρκόνδημος, a name approaching still closer to that on the seal, expressing, however, at the same time the opinion that the king to whom the seal belonged was of much earlier date.[1] As to the name of the place mentioned, Dr.

[1] The decision arrived at was probably in accordance with the view of Mr. Ready, who then was, as he still is, at the Museum. What has been said as to the style of engraving and the fracture was, most likely, suggested by him at the time, though he cannot now recollect the details of the matter.

[1] Prof. Sayce has called the seal "The Boss of Tarkondemos." But I do not see how in any way the seal can be suitably designated a "boss." And, even if it be conceded as certain that "Tarcondemos" represents the name given on the seal, still it is a Græcized form which cannot be used with propriety to denote a king who, according to Prof. Sayce's view, probably lived some 700 years B.C.

Mordtmann seems to have been first inclined to give *Zousous*, but, as no other authority for such a name could be found, he changed this conjecturally for *Tarsus*.

The Hittite characters Dr. Mordtmann regarded as, on the whole, emblems of the country, with its productions, over which the king ruled, and not as forming inscriptions, or an inscription. He made, however, some important observations with regard to the relation of these characters, as well as the figure and equipment of the king, with what was to be seen on other monuments. The boots with turned-up toes were found also at Boghaz-Keui, Eyuk, and Eregli, in Cappadocia (" Les Monuments d'Uyuk, de Bogaz-keuy, et d'Éregli, en Cappodoce"), as well as on the monument at Karabel, near Smyrna. The dagger was to be seen at Boghaz-Keui, and the spear at Karabel. The figure at Karabel, also, was without a beard, like that on the seal. At Karabel, too, were the same characters found between the head and the spear.[1] The accoutrements of the king were like those of the Cilician soldiers in the army of Xerxes, as described by Herodotus (lib. vii. ch. 91).

About ten or eleven years later, Dr. Mordtmann returned to the seal, and discussed it in the Journal of the German Oriental Society (*Zeitschr. d. deutsch. morgenländ. Gesellsch.*, vol. xxvi. p. 625). He then gave, as the name of the king, " Tarkudimme," and, though regarding the name of the place, " Tarsun," as tolerably well ascertained ("ziemlich gesichert"), yet he would not make, he says, any strong opposition if it should be preferred to substitute *zu* for the *tar*, forming the first syllable of this name. With regard to the Hittite characters, this article does not mark any very conspicuous advance, except that the animals' heads, which Dr. Mordtmann had previously regarded as the heads of horses, he now more accurately described as the heads of goats "Ziegenköpfe").

Though Dr. Mordtmann anticipated Prof. Sayce, not only in perceiving a relation between monuments (now recognized as Hittite) in different places in Asia Minor, but even in recognizing that certain characters on the seal were the same, or of the same kind, with those found on the Karabel monument, yet he did not perceive that these characters formed a Hamathite or Hittite inscription. It would have been scarcely possible for him to do this at the time; and this fact furnishes, in accordance with what has been said, one of the strongest arguments for the genuineness of the inscriptions on the seal. It was reserved for Prof. Sayce to detect that the seal presented a true bilingual, Assyrian and Hittite (*Academy*, August 21, 1880).

Although, as already stated, the Assyrian characters have peculiarities which distinguish them from any type of cuneiform writing otherwise known, nevertheless, with the exception of one important character, there is a tolerable agreement as to the way in which the Assyrian inscription is to be transcribed and read. If we begin with the first character after the vacant space over the king's head, this inscription, it seems to me, may be read thus:—

-me-e ▌ *Tar-ku-u-tim-me sar mat Zu-*

But if we begin, as we certainly ought to do, with the vertical wedge standing before the king's name, and denoting that the name of a man follows—represented here by a vertical black line—we have :

▌ *Tar-ku-u-tim-me sar mat Zu-me-e.*
"Tarkutimme king of the country of Zume."

The precise pronunciation of the second dental in the royal name it is impossible to determine with certainty. Some might prefer to read "Tarkudimme." But this is not of very much consequence. The difficulty to which I have adverted relates to the first character in the name of the country—the character immediately before the vacant space over the king's head. Prof. Sayce reads this as *er*, and gives as the name of the country *Er-me-e*, "Erme." To me the probability has seemed that the character should be read *su* or *zu*, the alternative reading which had been suggested by Dr. Mordtmann.[1] Certainly, with the reading *su* or *zu*, a symmetrical rendering of the Hittite inscription can be given, but with *er* this seems scarcely possible.

On comparing the spaces on the one and the other side of the figure of the king—for certainly this figure must be intended as a portrait of King Tarkutimme—it will be seen that the characters are repeated, though apparently with some variations in size and in the order of sequence. But these variations may be accounted for, if the exigencies of the space at the engraver's disposal are considered. On the left side of the king there is the greater space, and a division is effected by the king's arm and staff or spear. Consequently, in decipherment, the order observed on this side would seem to furnish the more satisfactory guidance. We may reasonably begin at the top, with the two characters above the king's arm; and these, it can scarcely be doubtful, represent the royal name Tarkutimme, inclosed and shut off as they are from the rest. How the name is to be divided between the two characters may seem not quite clear. Does the upper one denote *Tarku* and the lower *timme*, or should we divide *Tar-kutimme?* There are grounds on which the latter view seems the more probable. The Hittite hieroglyphics may possibly have been used by non-Semitic peoples, but at present the balance of evidence seems to be in favour of a Semitism more or less pure. On the Semitic hypothesis, and with the unequal division last given, both elements of the name admit of tolerably easy explanation. The first character seems certainly to be the head of a goat. There is little difficulty, in accordance with well-known vocal changes, including the substitution in Aramaic of *t* for *s*, in understanding how *tar* may represent the Hebrew *se'ar, sa'ir;*[2] and the second character likewise may be reasonably explained.[3] Beneath the arm is a tall cone which must certainly represent "king." This may be argued from the character being placed close to the king on both sides, as well as from its position on his left side immediately under the characters representing the royal name. It

[1] "Les mêmes caractères verticaux entre la tête et la lance que nous voyons figurés sur le sceau près de la main qui tient la lance et de l'autre côté près du grand obélisque." If anything more was intended than *characters of the same kind*, there seems to be a mistake.

[1] This conclusion was arrived at when, in 1880, my attention was first directed to the seal; and I then consulted three well-known Assyriologists. One of these was Dr. Strassmaier, who still adheres to the opinion then expressed. The very large number of texts which he has examined in the interval, while preparing his laborious and comprehensive contributions to Delitzsch and Haupt's Assyriological Library, gives his opinion the greater weight. He bases his opinion on the convergence of the two smaller wedges towards the larger horizontal wedge: strict parallelism would have been required to give the value *er*. Dr. Haupt, now Professor of Semitic Languages at the Johns Hopkins University, gave, if I recollect rightly, the value *su* rather than *zu*. Mr. S. A. Smith, who is editing and translating the Assurbanipal texts, also gives his opinion in favour of *su*.

[2] We ought also to remember in this connection that Tarsus was the chief city of Cilicia. According to the oldest authority which we have for the name (the black obelisk of Shalmaneser), the city was called *Tar-zi*. That this city should be called after the goat can scarcely seem unlikely, if we recollect how famous Cilicia was for its goats with thick and long hair, out of which Cilician cloth was made—a cloth of which, according to some, St. Paul was a weaver.

[3] In his contribution to the "Münzstudien," Dr. Mordtmann spoke of this character as "un objet difficile à reconnaître, mais qui ressemble au *pudendum muliebre*." And afterwards, in the *Z.D.M.G.*, he used similar language: "ein schwer zu bestimmendes Symbol, vermuthlich ein pudendum muliebre." Supposing this to be the object intended, there is no difficulty in understanding its bearing in a Semitic dialect the name *kutimme*. First, there is the Assyrian *katamu*, with the Arabic *katama*, "to conceal," connected with which the word would have a sense nearly equivalent to *pudendum*. But, having regard to what has been said on *tar*, it may seem that we ought to look to the Aramæan; and here we have *ketham*, "signavit." with a derivative, "*kuthema*," nearly identical in form with *kutimme*. In Gal. vi. 17 (Pesh.), *kuthema* is used in the plural of the στίγματα, which the apostle says he bore in his body. The transition from this sense is not difficult. Dr. Mordtmann's is the only probable explanation of the symbol. Special and local causes may account for its forming part even of a king's name.

may here be observed that there was at Aleppo a Hittite inscription which unfortunately has been destroyed, and which, though it had evidently suffered from the weather and time, was in several respects of great interest. From drawings which were made from it, especially by the late George Smith, we are able, however, to form a good estimate of its evidence with regard to the king-symbol. It presented a figure—no doubt of the person celebrated in the inscription—with a symbol similarly formed and similarly marked to that in the Tarkutimme seal.

The question here presents itself: In what order are the characters outside the staff or spear to be taken? Now in the Hittite inscriptions the *boustrophedon* manner of writing is observed. A line having been written from left to right, in the next the direction is reversed, and the writing goes back from right to left. This fact could not have been known to a forger in 1860, yet it is in accordance with this principle that our inscription is engraved. Having therefore read from top to bottom, we must go back, and read from the bottom towards the top. Accordingly we shall have to take next after the tall cone denoting "king" the smaller double cone. Prof. Sayce (apparently under the influence of Dr. Mordtmann's idea that the "deux petits obélisques réunis" owe their origin to the remarkable conformation of a certain district of Asia Minor) regards the double cone as denoting "country." But Dr. Mordtmann grouped these and the taller cone together, regarding all as of similar import. And, so far as their being of similar import is concerned, the conclusion seems to me inevitable. If, however, the

FIG. E.—"King"-symbol on Aleppo inscription.

taller cone denotes "king," the smaller cones, being of similar import, must denote "men." The tallness of the single cone is in accordance with the well-known ancient practice of denoting the greatness of a king by the greatly increased size of the figure representing him. It is true that, in accordance with Assyrian custom, the cuneiform legend gives "country." But whether a monarch is called king of a country or of the people inhabiting that country depends on local usage. "Queen of Great Britain" and "King of the French" are familiar contiguous examples in recent times. We may regard, then, the double cone as denoting "people," plurality being expressed by doubling the cone, and intensified probably by the numerous transverse marks.[1]

The symbol next above the double cone is, I believe, unique, no other example being found, so far as I am aware, on any of the inscriptions. To me it seems clear that this symbol is an ideograph of the country *Zume*. There are, it will be seen, on the lower side of the lower limb three projections, which may be reasonably regarded as representing mountains. The number *three* probably denotes a good many mountains. We may take it that Zume lay along the banks of a river or estuary with mountains on one side. A remarkable analogy is presented by one of the monuments in the British Museum from Jerablûs. We have here again the oval ideograph of "city,"[2] already mentioned in connection with the

[1] I have no hesitation in referring these cones to a phallic origin. This in early times would be regarded as a very natural way of representing "man"; and, like other designations of men, cones might easily come to denote both sexes, and a people generally.

[2] The ideograph is slightly broken on the monument.

Boghaz-Keui bas-reliefs. Here again, as on the Tarkutimme seal, we have the three mountains, occurring in this case on both sides. The intention is, to indicate a city located in, or at the head of, a valley lying between mountains.[1] The name of the city is in all probability denoted by the other characters, to the reader's left, the doubled curve (the doubling denoting plurality) and what is probably intended for a tree beneath.[2] This twofold indication of the name, for the sake of clearness, is entirely in accordance with the usage of both the Assyrian and Egyptian monuments. And similar evidence might be adduced from more remote sources.

Thus a twofold indication of the name Zume in the Hittite inscription on the seal must be regarded as altogether probable; and it seems to me beyond reasonable doubt that the last characters, the four nearly vertical strokes, with one horizontal, express the name Zume phonetically. Dr. Mordtmann observed, with reference to these strokes, that it would be difficult to give them a phonetic value without regarding them as numerals, but that so to regard them would be fruitless in result. In this last remark he was, I think, in error. They are, in my judgment, numerals, though here used, not with reference to their numerical value, but merely as phonetic signs; and to show that they are to be so taken, the engraver has placed them at an angle, or, so to speak, tilted them up. The last character, the two vertical strokes with one horizontal, gives precisely the Assyrian symbol for 100, but written after the archaic manner, before the wedge-writing was introduced. *Me*, the Assyrian for "a hundred," is, moreover, precisely the value that we require here. *Zu* or *su* will then be the name for 2, the first character.

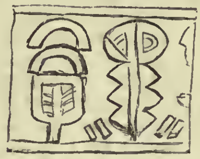


FIG. F.—Symbols from Jerablûs monument in the British Museum.

There is no great difficulty in connecting this with the Assyrian *sanu* 2, or *sunnu* ½, supposing that the *n* was slurred over in pronunciation and eventually dropped. And it must be remembered that, if those who made this seal spoke a Semitic dialect, there is no reason to suppose that this dialect was absolutely identical with any of the Semitic dialects otherwise known to us.[3]

It will be thus, I think, seen that there is a reasonable correspondence between this Hittite inscription and the cuneiform inscription round the circumference of the seal. It should be observed, too, that both with regard to the king and the country the phonetic designation is supplementary—in the first case to the portrait of the king, and in the second to the ideograph of the country. The inscription is mainly ideographic. It is important that this fact should be kept in view in the decipherment of other inscriptions.

With regard to the characters behind the king, or on his right side, it should be observed not only that the engraver had on this side a smaller space at his disposal, but also that he probably thought it necessary or desirable to place close to the figure the tall cone denoting "king."

[1] Dr. E. B. Tylor observes, "Map-making is a branch of picture-writing with which the savage is quite familiar, and he is often more skilful in it than the majority of civilized men" ("Early History of Mankind," p. 89). But of course the authors of these monuments were by no means savages.

[2] Having regard to the position of Jerablûs, where the monument was found, and to some other facts in relation thereto, I read the name conjecturally *Bamoth-elah*—that is, "Bamoth of the Terebinth."

[3] The two strokes similarly tilted up, and repeated occur on the monument in the British Museum mentioned just above (see Fig. F). With the same value as on the seal we should have *Su-su*, or *Zu-zu*, a reading by no means improbable; but I cannot in this place discuss the matter further. Cf. Zuzim, Gen. xiv. 5, and Zamzummim, Deut. ii. 20.

That the two signs for the royal name are not engraved immediately above the cone may have resulted from the space above the king's right arm being too contracted. There is another change in the characters on the king's right side which is noteworthy. It will be seen that both the ideograph of the country and the numerals giving the name are inverted. On the left side of the king the characters were to be read from left to right of the reader, but on the right side they are to be read from right to left. This change is in accordance with the *boustrophedon* manner of writing previously mentioned, but it is a change which seems totally incompatible with the idea of forgery.

(*To be continued.*)

ELEMENTS AND META-ELEMENTS.

THE President of the Chemical Society, in his address at the anniversary meeting, has further developed views which he had already propounded in his address to Section B of the British Association at Birmingham, and in a subsequent Friday evening lecture at the Royal Institution. He would have us believe that the atoms of an element are not all precisely of one absolute pattern; that atomic weights, in fact, are not constants, as generally supposed; but that we must regard each element as a species of which many varieties exist almost infinitely more like unto each other than to the atoms of any other approximating species of element; and that what we term the atomic weight is but a mean value around which the actual weights of the individual atoms of the species range within certain limits. Could we separate atom from atom, we should find them varying in weight within very narrow limits on each side of the mean.

Mr. Crookes supports his arguments by a wealth of illustration culled chiefly from his own unique experience; and, whatever the ultimate intrinsic value to science of his hypothesis, there cannot be a question that the study of the transcendent problem of the nature of the elements will have gained greatly in fascination by its promulgation; that lines on which such study may be carried on will have been indicated; and that he will have lightened the inexpressibly wearisome labours of fractionation by casting around them the poetic play of fancy. The subject is of such importance that it appears desirable to consider the position which chemists may fairly take up, and from which it is permissible to criticize the arguments that have led to the suggestion of the existence of meta-elements.

Apart from the higher interest which Mr. Crookes has now infused into them, his researches on the rare earths will ever excite admiration in all who study them, as models of scientific investigation; and they will afford undying testimony to his determination and patience in search of truth, as well as to the incomparable fertility of resource in experimenting of which he is possessed. Among the individual observations are many of a most suggestive and striking character which, sooner or later, must claim attention; but it cannot be denied that the data are as yet insufficient for their exact interpretation. This is true also of Krüss and Nilson's remarkable observations; indeed, it may be questioned whether their results all admit of the absolute interpretation which they are inclined to put upon them. In the paper in which the omnipresence of samarium is demonstrated, in giving an account of the many anomalies which he encountered in his search for x—the substance characterized by an orange-coloured band in the phosphorescent spectrum, and which subsequently turned out to be samarium—Mr. Crookes tells us how he came to the conclusion that samaria (x), which of itself gave little or no phosphorescent spectrum in the radiant-matter tube, became immediately endowed with this property by admixture with certain other substances—lime, for example—which substances likewise of themselves had no power of phosphorescing with a discontinuous spectrum. Many substances were found effective; and there was a general resemblance between the spectra, but nearly all of them differed from one another in detail. Mixtures of samaria and yttria gave spectra differing to a very marked extent according to the proportions in which the two substances were present. All who take note of these observations must agree that they are of a most remarkable and significant character: they certainly leave no room for doubt as to the necessity of exercising the utmost caution in inferring the absence or presence of particular substances from spectral appearances and changes. Judging from Mr. Crookes's observations, and from our general knowledge of the rare earths, it would almost appear that they have the power to form double oxides akin to double salts, and the effect on the spectrum produced by associating one oxide with another may be compared with the somewhat similar effect of a solvent on the spectrum of a coloured substance. The part that such double oxides perhaps play appears as yet to have been left out of consideration. It is desirable also to take into account the possible presence of double salts, and of their influence on the spectrum, before deciding as to the bearing of Krüss and Nilson's observations.

Reference is made by the President of the Chemical Society in his address to Carl Auer's investigation of didymium. Now the differences between Auer's neo- and praseodymium—the reputed constituents of didymium—are very marked; but as yet unfortunately we have no information respecting their atomic weights. This is true also of the various reputed constituents of the rare earths studied by Mr. Crookes and Krüss and Nilson. Until such information be forthcoming, the suggestion that what is commonly regarded as the atomic weight of an element is but an average value, therefore, can only serve to direct attention anew to the extreme importance of the most exact and exhaustive study of atomic weights.

What is called yttria, according to Mr. Crookes (Proc. R.S., xl. 506) is a highly complex substance capable of being separated into several simpler substances, each of which gives a phosphorescent spectrum of great simplicity, consisting for the most part of only one line. Now, supposing that the several constituent meta-elements of ordinary yttria be found when isolated to differ almost imperceptibly from each other both in chemical properties and in weight, yet the spectral differences will admittedly be very marked—as marked perhaps as are the differences between elements which exhibit very diverse chemical properties and atomic weights; and it will be illogical to deny to these meta-elements the right to rank as elements proper—as distinct species, not mere varieties.

Why, then, does Mr. Crookes think it inadmissible in the elementary examination to open the doors so wide that the number of admissions will be limited only by the number of applicants? It is because he thinks that the periodic system of classifying the elements offers an insuperable barrier to this course. Undoubtedly, if this were granted, there would be little choice left us; but can it be granted? We think not. The scheme at present accepted is after all but a very imperfect and provisional classification. The successional order of the elements in the horizontal series is indeed determined in all cases in which the atomic weight is known with a sufficient approximation to truth; and in certain cases where the properties are clearly marked it is possible to assign the true position in the order of succession to an element even when the atomic weight is very inexactly ascertained; tellurium is an example, having been placed before iodine long ere its atomic weight was ascertained to be lower and not higher than that of

iodine. But in arranging the elements in vertical series we have often great difficulty in determining which are true homologues: we have no difficulty in grouping the alkali metals, the halogens, or sulphur, selenium and tellurium, but how are we to place copper, silver and gold, for example? Are we justified in regarding them as true homologues, and in inserting them as intermediate terms in the group of the alkali metals? Ought we not rather to look upon them as but pseudo-homologues, and ought we not to place them apart from the alkali metals, and apart even from each other in vertical succession? This would lead us, instead of classifying the elements in linear vertical series, to arrange them in pyramidal groups, of which the elements of lowest weight form the summits. In fact, there is no justification whatever for the conclusion that the elements belong to only eight families; the most illiberal treatment leads us to recognize at least twelve, and there is no reason to accept this as the limit. We can thus foresee the possible existence of a far larger number of elements than is at present known, differing probably from each other to a marked extent both in atomic weight and properties. But even then the limit is not reached. Those who have classified the elements according to the periodic system, after all—consciously or unconsciously—have but followed the practice adopted in classifying carbon compounds; and if we consider the results arrived at by the study of hydrocarbons, and apply the conclusions to the elements, there appears to be no difficulty in finding place for a far larger number of meta-elements than even Krüss and Nilson would require to accommodate their host of new claimants for elemental rank. If we arrange homologous hydrocarbons side by side in the order of molecular weight, a scheme corresponding to that devised for the elements will result; but, if molecular weight only be considered, the existence of isomeric hydrocarbons escapes notice: if, however, isomers are included, each simple vertical group at once assumes a pyramidal form. In like manner, if the possible existence of isomeric elements be granted, the periodic scheme would admit without difficulty of the existence of a still larger number of elements even than was above indicated.

Nickel and cobalt have often been supposed to be isomeric elements. According to the most recent determinations of their atomic weights, however, cobalt has a higher weight (58·74) than nickel (58·56); but this result is discredited by the fact that cobalt is usually placed before nickel in the periodic scheme, and should therefore have the lower weight, unless the two elements are isomeric.

Whether among the meta-elements of the rare earths there are not numerous cases of isomerism, remains for the future to determine. Unless, however, some new mode of discriminating other than that involved in determining the atomic weight be introduced, the problem is one which appears beyond our present powers, as experimental error cannot be entirely eliminated. But it is perhaps of all the problems in chemistry the most important to solve, on account of its bearing on the higher problem whether the elements are simple or compound substances. So many converging lines of evidence now render it probable that the elements are compounds that the discovery of isomeric elements would probably suffice to carry conviction to the minds of all who are open to argument on this question. H. E. A.

THE DURATION OF LIFE.[1]

JOHANNES MÜLLER, the celebrated German zoologist, said: "All organic beings are transitory; life passes from individual to individual with the appearance of immortality, but the individuals themselves perish." This proposition is perhaps not so true as it seems to be. Nevertheless, it is certainly true that life has its natural limits, at least in all those animals and plants that ordinarily come under the notice of the layman. But the duration of life is very different in different animals, and it would be interesting to know the reason of this. Differences in length of life have been thought to depend on differences in structure and composition. Obviously the size of an animal will fix a certain minimum of time required for growth: owing to the relation between increase of bulk and increase of absorbent surface, pointed out by Leuckhart and Spencer, a larger animal will require a longer time to secure the surplus of nutriment required for reproduction. The degree of structural complication will also fix a minimum time: the activity of the vital processes, the rate of metabolism,—*because it influences the time at which reproductive power, the goal of individual life, is reached*—will influence the total duration of life. But these inner conditions do not fix the duration of life. Birds, whose vital processes are so rapid, may far surpass in age the sluggish Amphibia. Among ants, the males, females, and workers are practically identical in size, complication of structure, or rate of metabolism; yet the females and workers live several years, the males only a few weeks.

We must seek in the environment for the forces finally determining the duration of life. We find the length of life to be in each case an adaptation arranged by natural selection in the interests of the species. So soon as an individual has produced young enough to fill up the gaps caused by death, it ceases to be of use for the species. Where fostering of the brood obtains—be it uterine or post-uterine—we expect and find a longer duration.

The apparently accidental causes of death remove far more individuals than natural death. The longer an individual lives the more chances of accident does it undergo; *and so selection, acting in the interests of the species, rather than prolonging the life, hurries on the time of reproduction.* At first, it seems impossible that the great age reached by many birds (Raptores may survive their century) is the shortest possible. But the enemies of the eggs and of the young of birds are very numerous. The death-rate is enormously greater than in the case of mammalian embryos developing within the parent. Adaptation to rapid flight precludes great fertility. Bad fliers like the *Phasianidæ* lay many more eggs in a season and live through far fewer seasons.

The adaptation is very clear in the case of the larval life of insects. The larvæ of bees and of many ichneumons placed in the midst of an abundant food supply become pupæ in a few days. The larval stage of predacious larvæ which have to waste time and energy in securing their prey, and of vegetable-feeding larvæ, on account of the less nutritious nature of their food, lasts very much longer. The usually short life of the imago bears no relation to the length or shortness of the larval life, but is directly adapted to its own purposes. In the simplest case, where copulation takes place as soon as the wings are dried, and where the eggs are deposited rapidly and carelessly, the whole adult life lasts but a few hours. Where the mate has to be sought, or the eggs deposited in special conditions, or where active habits preclude simultaneous maturation of eggs, the duration of life is prolonged in correspondence with the special requirements. Adult insects are perhaps the most hunted of animals, and in them is found the extreme case of adaptive shortening.

The inner changes on which natural death depends are not very clear. They can hardly depend on cell destruction; for it is upon that that the processes of life are based. More probably they depend on a failure to produce new generations of cells to replace the cells broken down in the vital processes.

The occurrence of death at all is a provision to secure

[1] "Ueber die Dauer des Lebens." Von Dr. August Weismann. (Jena, 1882.)

the greatest possible number of contemporary individuals of full strength. Contact with the world wastes away individuals with here an accident to-day, there an accident to-morrow. The possession of immortality by the individual, while a doubtful boon to it, would be a harmful luxury to the speies. Death makes room for new, complete individuals. Death is, however, by no means a universal attribute of organisms. In unicellular organisms the single cell is at once somatic and reproductive, and, while liable to accidental destruction, is potentially immortal. The Protozoon divides without a remainder; and the life of each Protozoon alive to-day has descended in direct continuity from the life of the primordial Protozoon.

In the Metazoa a division of labour has separated reproductive cells from somatic, and their complexity, by admitting of mutilations short of destruction, has rendered them mortal. The reproductive cells had to remain capable of an indefinite number of generations lest extinction of the race occurred; but when the somatic cells became specialized, there at once arose the possibility and the necessity of a limit to the number of generations.

It is clear that the size of an individual is an inherited property. Conditions of nutrition can only negatively determine growth. No superfluity of nutrition could build up the framework of a dwarf into a giant. Natural selection acting on variations has fixed the average size of individuals. It has in fact fixed the space limits of cell reproduction, and could have equally well fixed the limits in time—the duration of life—of individuals. There is a continuity of life from organism to organism through the divisions of the immortal germ-cells. The somatic cells arising from the germ-cell in each generation possess a limited reproductive capacity, and the limits are fixed by natural selection for each species so as to maintain the greatest possible number of contemporary individuals of full vigour.

P. CHALMERS MITCHELL.

NOTES.

THE French Association for the Advancement of Science has had a successful meeting at Oran, in Algeria. M. Laussedat, the President, chose as the subject of his address the civilizing influence of the sciences. This was the second meeting of the Association in Algeria, the first having been held in 1881.

THE nineteenth annual Conference of the National Union of Elementary Teachers was opened at Cheltenham on Monday. The President, Mr. Pope, in his inaugural address, spoke bitterly of the existing system of elementary education, which he denounced as a "failure." On Tuesday, the same tone was adopted by the Rev. E. M. M'Carthy, of King Edward's School, Birmingham, who read a paper to show that the system violates two of the fundamental principles of true education. Those principles are: (1) that the course of studies laid down for each stage should be in harmony with, and adapted to, the natural development of the individual child's mind and body; and (2) that all educational processes should develop faculties so as to produce habits of ready and accurate thinking, besides furnishing the mind with knowledge for use and imparting mechanical skill in the use of it.

PROF. KIEPERT, of Berlin, will start immediately on a journey of research in Western Asia Minor. He will be accompanied by Dr. E. Fabricius, the archæologist. The journey will last three months.

ON Easter Monday, 12,374 persons visited the Natural History Museum, South Kensington. The number of visitors on the corresponding day last year was 6570.

THE Report of the Meteorological Council for the year ending March 31, 1887, which has recently been issued, shows that at that date observations were being taken for the Office on 143 ships, exclusive of the vessels of the Royal Navy, all of which are supplied by the Council with instruments, although the keeping of a special meteorological log is optional. The work in hand by the marine branch is: (1) the completion of the synchronous charts of the North Atlantic; (2) a discussion of the meteorology of the Red Sea; (3) current charts for the Atlantic, Pacific, and Indian Oceans; (4) charts of the Aden cyclone of June 1885. In order to discover the cause of this storm and of its unusual course across the Arabian Sea, synchronous charts of the North Indian Ocean for the month of June are being prepared. In the weather branch, forecasts are drawn three times a day. A comparison of the results of the 8 p.m. forecasts gives 81 as the total percentage of success. Hay harvest forecasts were issued to some selected stations, as in previous years. Storm-warning telegrams are issued to 141 stations; the trans-Atlantic messages appear to have been of no practical value for the purpose of these warnings—rather the contrary, as they have occasionally caused the premature issue of warnings to our coasts when no storms followed. The principal changes in the climatological branch have been the erection of self-recording anemometers at Fleetwood and North Shields, and of an electric anemometer at Valentia Island, but unforeseen difficulties have hitherto prevented this from being brought into operation. The Report contains a table showing the distribution of gales round the coasts of the British Islands during each month for the fifteen years 1871–85.

M. L. CRULS, the Director of the Imperial Observatory at Rio de Janeiro, has made an appeal to all meteorological observers for assistance in the compilation of a "Universal Climatological Dictionary," which is intended to comprise, in a methodical form, the principal meteorological elements from as many stations as possible over the whole globe. The data asked for are the mean monthly and yearly temperatures, together with the monthly maxima and minima, and the dates of the yearly absolute extremes; the relative humidity, amount of cloud, rainfall, number of days of rain, thunderstorms, and frost, and the prevalent wind, in each month; the mean annual height of the barometer, and its mean annual oscillation. The work proposed by M. Cruls would be very useful, as, although information already exists for a great number of stations, it is dispersed in many different publications, and is expressed in different measures, so that comparisons are difficult. Details relating to the meteorological elements of his own country especially are much wanted.

IN the *Annales du Bureau central météorologique* of Paris for 1885, vol. i., M. Renou has discussed the rainfall of Paris for the last 200 years. The observations were begun in 1688 by Lahire. At that time the Observatory was outside Paris, some distance to the south, but it is now in the midst of a district surrounded by high buildings. It is a curious fact that soon after Leverrier assumed the directorship he planted some trees near the rain-gauge, which in time affected its readings; these trees were afterwards cut down by Admiral Mouchez. The rainfall seems to have undergone some changes in this long period. At the time of Lahire there was a marked maximum in July; now there are two less marked maxima in June and September. The number of rainy days amounts on an average to 169. Snow occurs very irregularly, but it is never entirely absent in any winter. The heaviest rainfall in a short period was on the 9th of September, 1865, which yielded over 2 inches on the terrace of the Observatory in $2\frac{1}{4}$ hours; the gauge on the ground overflowed.

WHILE studying the laws of dissolution of salts, M. Umoff came to the following correlation, which seems not to have been yet remarked, and which he communicated in a paper in the Memoirs of the Odessa Society of Naturalists (vol. xii. 1). For

potassium chloride, bromide, and iodide, as also for natrium iodide, the weights of salt necessary to saturate a given amount of water at 100° C. are proportionate to the cubes of densities of the respective anhydride salts; while for sodium chloride the same law is true with regard to the saturation weights of water at 0°. The saturation-weights of potassium chloride and natrium iodide at 100° being the double of what they are at zero, they belong simultaneously to both groups.

M. LINDELÖF has contributed to the Proceedings of the Scientific Society of Finland (tome xvi.), a paper on the trajectory of a body moving over the earth's surface under the influence of terrestrial rotation. The author considers that the explanation of the movements of atmospheric currents, for instance, as generally given, is far from sufficient, and leads to inexact ideas. The paper is divided into four parts: the first three deal with the equations of the different movements of a body, and with the forms taken by the trajectory; in the fourth part the theory is applied to the calculation of the passage of the atmospheric wave observed after the Krakatão eruption in August 1883.

PROF. W. BRÖGGER lately submitted to the Swedish Geological Society an account of the work done by the Committee appointed for the purpose of obtaining reports on earthquakes occurring in Sweden. It was decided that trustworthy reporters should be appointed in all parts of the country, and that a number of inexpensive seismographs should be purchased. At the same meeting Baron Nordenskiöld exhibited a new silicate of lead from the Harstigs Mine, in Värmland. Among recent papers of special interest published by the Society is one on the meteors observed in Sweden in 1887, by Dr. Svedmark.

AT a recent meeting of the Seismological Society of Japan, reported in the *Japan Weekly Mail* of February 4, Prof. Milne read a paper on earthquake sounds. These frequently precede the shock, are often heard during its progress, and sometimes have been heard after the earthquake proper has ceased. Their character is very varied, from a low, barely audible rumbling, to a loud rattling, like a cart on a stony street, or a volley of musketry. They are heard better in some districts than in others; better probably where the earth's structure is hard and solid than where it is loose and soft. After discussing some of the explanations that have been given, Prof. Milne suggested that there is a close connection between these sounds and the smaller vibrations which invariably precede the shock proper. He had counted as many as seven per second of these sinuosities, and we are warranted in assuming the existence of still smaller and quicker vibrations preceding even these. With more delicate seismographs we might be able to catch the very early infinitesimal movements that herald the approach of an earthquake. With thirty or forty vibrations per second we should have an audible note of very low pitch. It was suggested in the subsequent discussion that as seismographs show a tail-end of sinuosities very similar to the initial ones, we should expect to hear sounds succeeding as often as preceding an earthquake.

THE Report of Mr. Cautley, the Acting-Consul at Trieste, on the forests of Austria, just issued by the Foreign Office, says that perhaps Austria has a larger proportion of forest in comparison with its area than has any other country. The woods cover about 3,500,000 acres, of which 80 per cent. is timber forest, and the remainder of young growth. The Government and the large land-owners own 69 per cent. of the total forest area, the parish authorities 20 per cent., the clergy 5½ per cent., and peasants about 1½ per cent. The forests are, in fact, the principal source of wealth to Austria, and, calculating the cubic contents of all the timber, and reckoning each cubic foot at nine-tenths of a penny, the wealth of the whole country in this respect may be set down at close on £40,000,000 sterling. The yearly increase in the value of the forests is said to be over half a million sterling.

IN the *Zoologist* for April, Mr. Postlethwaite, of Hallthwaites, Cumberland, states that, last autumn, while netting for salmon in the Duddon Estuary, fishermen brought to the surface some massive horns of the red deer. One pair, with the skull attached, must have had at least fifteen points; the length of one horn is 40 inches; the distance apart at the top of the horns, 42 inches; the circumference of the burr, 11 inches. In another case, a skull was recovered with only a portion of one antler attached; and of a greater size than in the previous example. The horn is broken just above the third tine, the length from the base being 14 inches; the length of one tine, 13½ inches; and the circumference of the burr, 10½ inches. A scapula was dredged up and brought to shore at the same time. The weight of each of these specimens was great, the first-named being as much as a man could comfortably carry. Similar horns were found some years ago, and in the neighbouring estuary of the Esk at various times many such antlers have been discovered, most of which are preserved at Muncaster Castle. Mr. Postlethwaite adds that the channel of the Duddon is shifting and running close into the sides of an old peat moss, from which it seems not unlikely that the horns have been washed. In an editorial note appended to this interesting communication it is suggested that the animals which possessed these fine horns may have been wanderers from the great forest of Bowland, in Lancashire, where red-deer lingered until the early part of the present century; and that they may have roamed over Martindale Fell, in Westmoreland, "where a few of their descendants are still preserved, a pleasing link of association with the past."

THE whale fishery in the Greenland seas and Davis Straits was very unprofitable in 1887. In an article on the subject in the current number of the *Zoologist*, Mr. Southwell says that whales are by no means exterminated. Capt. Gray saw fourteen of them in Greenland, and Capt. Adams is reported to have seen seventeen in Davis Straits; but, from long persecution, they are now "simply unapproachable."

THE French Consul at Bilbao states in a recent report that the pilchard or sardine fishery on the Atlantic shores of the northern portion of Spain proved in 1887 a most disastrous failure. During the three months of June, July, and August, which are generally the most abundant in the year, nothing was caught but sardines far too large for the boxes commonly used in the trade. The amount taken in 1886 was 1650 tons, and during the corresponding months in 1887, it was only 790 tons. This large falling off is supposed to be due to the fact that the fish do not find the food they require on this coast, formerly one of their favourite habitats. Perhaps in a large measure it is owing to their having been driven away by the reckless system of fishing which has been adopted in the past.

THE Report of the Mason Science College, Birmingham, for the year ending February 23, 1888, has just been issued. The Chairman of the Academic Board testifies that, although the year was not marked by any new or striking developments in the educational policy of the College, or by any special additions to the existing curriculum of the subjects taught, the general progress of the College upon the lines laid down in previous years was eminently satisfactory. Not only was the total number of day students larger than in any previous year, but the increase affected, in varying proportions, nearly all the different departments. The year was also characterized by a marked increase in the number of systematic students. By "systematic" students are meant those who enter the College with the object of preparing for the various University or medical examinations, for technical dip-

lomas, or for the Associateship of the College, or who are studying some definite subject with the view of teaching, or original research, or with regard to its practical application to manufacturing industries.

WE have received the Proceedings of the Royal Physical Society, Edinburgh, for the session 1886–87. Among the contents is an interesting Presidential Address, by Mr. John A. Harvie-Brown, on the faunal importance of the Isle of May, purely from an ornithological point of view.

WE have received the first part of what promises to be an admirable work—"An Illustrated Manual of British Birds," by Mr. Howard Saunders. The book will be completed in about twenty monthly parts. It is being issued by Messrs. Gurney and Jackson.

A TWELFTH edition of the late Dr. David Page's "Introductory Text-book of Geology" (Blackwood) has been issued. It has been edited by Prof. Charles Lapworth, who, in order to bring all the departments up to date, has found it necessary to recast or re-write almost the whole of the work, with the exception of the introductory and concluding chapters.

MR. HALY, Director of the Colombo Museum, has published a first Report on the Collection of Birds in that institution. It fills about eighty pages demy octavo.

THE new number (the third) of the American periodical—the *Technology Quarterly*—opens with a valuable account, by Mr. W. O. Crosby, of the methods of instruction in mineralogy and structural geology in the Massachusetts Institute of Technology. Mr. S. W. Hunt continues his discussion of the precision of measurements; and Mr. F. W. Clark contributes notes on the assaying of lead, silver, and gold.

THE Johns Hopkins University, Baltimore, has received from the Maryland Academy of Sciences a considerable portion of its scientific collection. Among the specimens is the skeleton of a young fin-back whale captured in the lower part of Chesapeake Bay. Stumps of cycads, which were presented to the Academy by Mr. P. T. Tyson, are also greatly valued. They were taken from the Upper Jurassic clays of Maryland.

ACCORDING to a communication by M. A. Pavloff to the Moscow Society of Naturalists, the meteorite which fell in August last at Okhansk, in Perm, is one of the largest yet known. Its weight, before it was broken, was about 1100 lbs. It belongs to the group of stony meteorites. As it contains particles of unoxidized nickel iron, it must be classified with the sporado-siderites. Its spherical mineral aggregates bring it under the heading of chondrites.

THE following extract from a private letter by a British officer, dated Sittang, Upper Chindwin, February 4, 1888, may be of interest to anthropologists:—"We have arrived here after eight days of hill-marching with very many ups and downs—the highest point just over 5000 feet. We are now completely out of Burma—the hills were sparsely inhabited by uncivilized Chins and Nagas—and are now in a small State, a plateau in the mountains at a level of nearly 3000 feet. The ruler and his people are Hindus by conversion or adoption some hundred years or less ago—the only example I know of Hindu proselytes. The Burmans are tattooed from waist to knee with a fine pattern in blue, looking as if dressed in short dark tights. They wear the hair long, rolled on the top of the head, and covered with a bright-coloured silk kerchief, put on somewhat as one sees in pictures of Negro women of the Southern States in America. The Shans, who were our neighbours in the hills near the Ruby Mines, wear very baggy trousers, like the Chinese, of coarse blue cotton stuff, have uncut hair, and for a head-covering a hat, either of straw or a coarser kind of wicker, of colossal circumference. This hat is as big as an ordinary silk umbrella, but flat except in the middle, which is conical for the reception of the top-knot, and as this might sometimes prove an insecure hold they often wear a fastening under the jaw. They tattoo more extensively than the Burmans, and sometimes stow away jewels under the skin. I have seen lumps which may have been so caused, from their appearance, but I never had the chance of proving their secretion by enucleation. The Nagas, whom we have used in the last few days as carriers, do not tattoo, and wear a skimpy kilt. The hair is uncut and coiled on the front of the head, the lump or coil of hair secured by a band round the base; the band often made of strings of blue beads or a tape of leather, on which two or three rows of small white shells are sewn. A silver or other metal skewer, about ten inches long, is often stuck through the hair, like the arrows worn by some belles of the West—whether only for adornment, or used as a fork or harpoon, I know not. All these savages have the ears pierced. The Naga carries his snuff in a bit of bamboo thicker than an ordinary lead-pencil; and the Burman, who smokes eternally, sticks his cigar *in* his ear-lobe—and his cigar is about the size of that Mr. Verdant Green was induced to smoke, of such calibre that it would not pass through a Colt's revolver barrel. The Nagas here are not tall, but their calves and thighs would attract attention even at a Highland gathering at Athol or Braemar. Their loads they carry with a neatly made neck-and-shoulder yoke. From the yoke in front is a brow-band, while behind a rope-loop passes under the load."

THE Students' Engineering Society of the University College, Bristol, concluded the winter session, on March 26, with a public disputation on the gas-engine *v.* the steam-engine, and on March 27 gave a *conversazione.* The electrical and engineering exhibits attracted much attention, and a highly appreciated concert was rendered by the students and their friends.

THE additions to the Zoological Society's Gardens during the past week include a Gannet (*Sula bassana*), a Greater Black-backed Gull (*Larus marinus*), British, presented by Mrs. Rickards; a Hawfinch (*Coccothraustes vulgaris*), British, presented by Mr. Chas. Faulkner; a Common Swan (*Cygnus olor*), British, a —— Penguin (*Eudyptes pachyrhynchus*, from New Zealand, deposited.

OUR ASTRONOMICAL COLUMN.

THE PERIOD OF ALGOL.—Mr. S. C. Chandler publishes, in Nos. 165, 166, and 167 of *Gould's Astronomical Journal*, a careful and thorough discussion of the period of this interesting variable. Starting with the observations of Goodricke in 1782, he had at his disposal the times of nearly 700 minima as observed by about fifty astronomers, spread over a little more than a century. His first task was to reduce these observations to a common system—an operation the more necessary, since, in the present low state of our knowledge, differences in the processes of reduction are more important in their effect, if they do not completely overshadow personal differences in observation. Mr. Chandler decided, therefore, to abandon the use of the minimum phase as a reference-point, and re-reduced the entire mass of observations on a method the essential principle of which consisted in taking, as the reference-point, the mean between the times of equal brightness on the descending and ascending branches of the light curve. This involved the abandonment of 199 minima, for which sufficient details could not be procured, but left 496 to be employed in the investigation. Unfortunately these are not by any means evenly distributed as to time, and in the earlier part of the present century satisfactory observations are very scarce.

That the period of Algol was itself subject to change was suspected by Wurm and proved by Argelander, but the formulæ deduced by the latter have not represented later observations. Mr. Chandler has succeeded, however, in reducing its apparently highly complicated anomalies to a comparatively simple law.

This law comprises two inequalities, with the periods respectively of 141·3 years and 37·7, and coefficients of 173·3 and 18·0 minutes of time. A third period of 17 years, with a coefficient of 3·5 minutes, was suspected, but the coefficient is so small as to bring it almost within the limit of errors of observation. The resulting elements are as follow: 1888 January 3, 7h. 21m. 29·23s. (G.M.T.) + 2d. 20h. 48m. 55·425s. E′ + 173·3m. sin. ($\frac{1}{50}$ E′ + 202° 30′) + 18·0m. sin ($\frac{3}{40}$ E′ + 203° 15′) + 3·5m. sin ($\frac{1}{6}$ E′ + 90° 20′); where E′ = E (Schonfeld) − 11210. The interpretation of the theory is as follows:—The period at the time of Goodricke's discovery of the character of the variation was 2d. 20h. 48m. 58·0s., lengthening to 59·8s. in 1798, diminishing again in the next ten years to 57·2s., and then again lengthening irregularly to 59·2s. in 1830. A rapid diminution shortly followed, and the rate was reduced to 54·0s. in 1843. After a halt a further but less rapid diminution set in, and in 1858 the period was 52·8s. The following six years saw an increase of 1·6s., followed by another shortening, until in 1877 the period had fallen to 51·1s., from which time it has remained nearly constant; but should the theory be correct, a period of increase must shortly set in, which, with halts and retrogressions, will attain a maximum somewhat late in the coming century.

The paper concludes with a table of heliocentric times of minima up to August 1898.

M. Oudemans, Director of the Utrecht Observatory, is likewise preparing a work on this variable, and requests observers to transmit to him copies of their notes on all observed minima since 1883.

OBSERVATIONS OF VARIABLE STARS.—Mr. Edwin Sawyer has given, in Nos. 164 and 165 of *Gould's Journal*, his observations of several variable stars made during the year 1886. The following table will show how some of these compare with the ephemerides given week by week in NATURE.

Star.	Phase.	Observed.	Calculated.
V Cancri	*M*	1886 March 29	April 12
R Ursæ Majoris	*M*	1886 April 29	May 12
R Virginis	*M*	1886 April 8	April 10
S Coronæ	*M*	1886 May 10	April 10
R Scuti	*m*	1886 July 21	June 27
	M	1886 Sept. 12	Aug. 1
	m	1886 Dec. 2	Nov. 17

Mira Ceti was observed at maximum 1886 January 9; *g* (30) Herculis at minimum June 14, and at maximum September 20; and W Cygni at three epochs, viz. *m* July 8, *M* September 10, and *m* November 5.

Gore's new variable near χ_1 Orionis, to which Mr. Sawyer gives the lettering U Orionis, but which other astronomers have generally designated T, attained a maximum about 1887 December 14. The maximum was only a feeble one, − 7·5 mag. The light remained almost stationary from 1887 November 29 to 1888 January 2, a period of thirty-four days. The period of the star must be almost exactly a year.

The variable Lal. 40083, discovered by Mr. S. C. Chandler (see NATURE, vol. xxxv. p. 282), and to which he has given the name X Cygni, has shown from further observation that its light-curve is not constant in different periods, the minimum brightness being especially variable, but since the bright and faint minima do not alternate regularly the star does not belong to the β Lyræ class. Mr. Chandler's revised elements for the star are as follow: 1886 October 13, 14h. 20m. G.M.T. + 15d. 14h. 24m. E. Approximate duration of increase 5·6 days, of decrease 10·0 days. The maximum brilliancy is generally about 6·4m.; the minimum ranges from 7·2m. to 7·7m.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 APRIL 8–14.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on April 8

Sun rises, 5h. 20m.; souths, 12h. 1m. 43·3s.; sets, 18h. 43m.: right asc. on meridian, 1h. 10·4m.; decl. 7° 29′ N. Sidereal Time at Sunset, 7h. 53m.

Moon (New on April 11, 9h.) rises, 4h. 46m.; souths, 10h. 6m.; sets, 15h. 35m.: right asc. on meridian, 23h. 14·6m.; decl. 8° 21′ S.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	4 52 ...	10 29 ...	16 6 ...	23 37·0 ...	5 8 S.
Venus ...	4 47 ...	10 32 ...	16 17 ...	23 40·9 ...	3 40 S.
Mars ...	18 53*...	0 22 ...	5 51 ...	13 28·6 ...	6 48 S.
Jupiter ...	22 57*...	3 10 ...	7 23 ..	16 16·9 ...	20 19 S.
Saturn ...	10 59 ...	18 58 ...	2 57*...	8 7·8 ...	20 48 N.
Uranus...	18 9 ...	23 46 ...	5 23*...	12 56·4 ...	5 17 S.
Neptune..	6 56 ...	14 37 ...	22 18 ...	3 45·7 ...	18 11 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

April.	h.	
8 ...	23 ...	Mercury in conjunction with and 1° 16′ north of the Moon.
9 ...	1 ...	Venus in conjunction with and 2° 24′ north of the Moon.
11 ...	6 ...	Mars in opposition to the Sun.
14 ...	4 ...	Mercury in conjunction with and 1° 10′ south of Venus.

Variable Stars.

Star.	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·4 ...	81 16 N. ...	Apr. 12,	4 2 *m*
Algol	3 0·9 ...	40 31 N. ...	,, 13,	3 26 *m*
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,, 11,	20 7 *m*
U Monocerotis ...	7 25·5 ...	9 33 S. ...	,, 12,	*m*
S Cancri	8 37·5 ...	19 26 N. ...	,, 13,	19 30 *m*
δ Libræ	14 55·0 ...	8 4 S. ...	,, 10,	22 56 *m*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 11,	2 57 *m*
			,, 11,	23 5 *m*
W Sagittarii ...	17 57·9 ...	29 35 S. ...	,, 9,	4 0 *m*
Z Sagittarii... ...	18 14·8 ...	18 55 S. ...	,, 11,	1 0 *M*
U Sagittarii... ...	18 25·3 ...	19 12 S. ...	,, 10,	3 0 *m*
			,, 13,	2 0 *M*
η Aquilæ	19 46·8 ...	0 43 N. ...	,, 14,	2 0 *m*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	,, 13,	4 0 *M*
R Vulpeculæ ...	20 59·4 ...	23 23 N. ...	,, 13,	*M*
δ Cephei	22 25·0 ...	57 51 N. ...	,, 12,	3 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near α Ursæ Majoris ...	163° ...	60° N. ...	April 10 and 11.
,, 42 Herculis... ...	248 ...	50 N. ...	,, ,,

GEOGRAPHICAL NOTES.

A SHORT excursion into the almost unknown interior of San Domingo was made last summer by Baron H. Eggers, in the course of which he explored the mountainous district, and made a complete study of the vegetation of this elevated region; he further discovered a route along which the exploration of this little-known mountain region may be carried out with facility. The following details are taken from the traveller's own account of his journey, published in *Petermann's Mitteilungen* (Part 2, 1888). He left Puerto Plata, on the north coast, on May 2 last, and about the middle of the same month found himself at Jarabacoa on the Rio Yagin, having passed through Santiago on his way. While at Jarabacoa he ascended Monte Barrero (4100 feet) in the vicinity of the town. The steep slopes of this peak are covered with lofty pine woods. In the small ravines and between rocks the traveller observed many interesting plants, *e.g.* the dark red *Fuchsia triphylla*, a bright red *Siphocampylos*, a large *Pentarhaphia*, and a beautiful *Cyathea*; he also found a large number of hitherto unnoticed plants, including an ilex, several Compositæ, Labiatæ, &c. The animal life in these pine forests appears to be very poor: there are scarcely any insects, and a species of crow is the only bird seen. At the end of May the traveller with a small party of blacks set out in a due southerly direction for the Valle de Constanza. The valley is well watered, and its height above the sea is 3840 feet. Its inhabitants, numbering 100, are engaged in cattle-rearing, and the cultivation of beans, maize, cassava, tobacco, &c. The climate is cool, and from November to March dry; during the rest of the year it rains. The thermometer at 6 o'clock in the morning of May 28 stood at 59° F. The higher part of the surrounding mountains, which almost everywhere contain gold, though in small quantities, are quite unexplored. From the Valle de Constanza the traveller made a

further excursion to the south-east to a savanna region, situated in a depression among the mountains, and called by the natives "Valle Nuevo." The path led over forest-clad mountains with intervening gorges, and formed a continual ascent till the Valle Nuevo was reached, which is 7450 feet above the sea. One of the forest tracts which the traveller traversed was especially dense and almost impassable; beautiful mosses, ferns, orchids, lycopods, and other epiphytes were growing on the trees. The Valle Nuevo is surrounded by low hills, which form the culminating points of the range; the highest of these, viz. Pico del Valle Nuevo (8630 feet above the sea-level) was ascended by the traveller.

DR. RINK contributes to the current number of *Petermann's Mitteilungen* an account of the results of the recent journeys made by Lieuts. Ryder and Block along the coast of Greenland to the north of Upernivik in 1887. By accurate measurements made in the ice-fiords of Angpadlar Fok, &c., both in April and August, some interesting and important results have been secured as regards the physical geography of this region. Some of the ice-fiords are very prolific in ice-bergs, notably that of Giesecke, where the edge of the permanent ice has retreated considerably within recent years. The results show not only the extraordinary rapidity, but the great variableness in the movements of the ice, apart apparently from the temperature of the time of year. The average temperature of the air during the measurements from April 20 to 24, was from $-9°$ F. to $-15°$. On January 28 the water temperature, at a point where the ice-fiord was 512 fathoms in depth, was as follows: at the surface 27°·7 F., at 50 fathoms 28°·9, at 200 fathoms 32°, and at 287 fathoms 32°·2. The question of the limit and movements of the inland ice of Greenland, to which the attention of recent Danish explorations has been directed, and towards the solution of which the results obtained by Lieuts. Ryder and Block have materially contributed, is discussed by Herr Rink in his paper, which also gives some interesting notes on the botany, geology, and ethnography of the country.

IN the April number of the Proceedings of the Royal Geographical Society there is an excellent new map of Siam, based on the surveys of Mr. James McCarthy. There also will be found the second and third of General Strachey's Cambridge geographical lectures.

AT the last meeting of the Royal Geographical Society a paper of unusual interest and originality, on the Solomon Islands, was read by Mr. C. M. Woodford, who spent several months in the group in 1886–87. Mr. Woodford's attention was mainly directed to Treasury Island, his head-quarters for some months being at Alu, on that island. He made many journeys into the interior, and was so successful that he obtained nearly 17,000 specimens in natural history, which, so far as they have been examined, have been found to comprise three new genera, and eight new species of mammals, fifteen new species of birds, six new species of reptiles, and over a hundred new species of Lepidoptera. Mr. Woodford visited, besides Treasury Island, the islands of Fauro, New Georgia, Guadalcanar, and others, exploring their interiors as far as possible, and in the case of Guadalcanar attempting to ascend Mount Lammas (8000 feet), without, however, succeeding. He followed the Bokokembo River as far as possible, finding the vegetation most luxuriant, and composed of large Ficus and other forest trees, with occasional clumps of sago and areca palms, but few coco-nuts. The coast natives are greatly afraid to venture into the interior, partly through fear of the bush-folk who live in the mountains, and partly through superstition. Mr. Woodford's observations on the natives are of great value; he had unusual opportunities of observing their modes of life. They are mostly inveterate head-hunters and cannibals. Natives of different parts of the group differ considerably from one another, but they belong to the Melanesian or Papuan type. Mr. Woodford believes, however, that on the island of Ysabel there is a strong infusion of Polynesian blood from Ongtong Java, or Lord Howe's Group, as canoes are known to have been driven in bad weather from that group, and to have arrived on the coast of Ysabel. The natives of Bouka and Bougainville, and of the islands of Bougainville Straits and of Choiseul, are intensely black in colour, but as one journeys eastward the colour changes to a dark brown. They have woolly hair, but occasionally natives are met with wavy, and in some cases straight hair. Mr. Woodford attributes this fact to an infusion of Polynesian blood, and has noticed it in natives from Ysabel, also at Fauro.

THE Royal Geographical Society of Sweden has awarded the *Vega* Gold Medal—instituted in honour of Nordenskiöld's voyage—to Dr. Wilhelm Junker, the celebrated African traveller. The medal, which has not been awarded since 1884, has hitherto had only four recipients, viz. Nordenskiöld, Palander, Prejevalsky, and Stanley.

THE ATOLL OF DIEGO GARCIA AND THE CORAL FORMATIONS OF THE INDIAN OCEAN.[1]

DIEGO GARCIA is a typical atoll; a narrow strip of land varying in width from a mile to 30 yards, nearly completely encircles a lagoon of irregular shape. The lagoon is open to the ocean towards the north-west, its mouth being divided by three small islets into four channels, of which three are sufficiently deep to allow ships to enter the lagoon. The whole of the land composing the atoll is very low; the highest point in the island is not more than 30 feet above the level of high tide, and this height, which is quite exceptional, is due to the accumulation of great heaps of sand through the action of the south-east trade winds which blow with considerable strength for more than one-half of the year. Diego Garcia is the southernmost atoll of the Chagos Group; it lies in S. lat. 7° 26′, E. long. 72° 23′, and forms the last of the great chain of coral formations reaching from the Laccadive Islands, through the Maldives to the Chagos Group. To its south-west lie the submerged atoll-shaped reefs known as Pitt's Bank and Centurion's Bank, to its north lies the huge submerged atoll known as the Great Chagos Bank. It is an interesting fact that throughout the Laccadive, Maldive, and Chagos Groups there is no instance of a fringing or of a barrier reef; nothing but coral structure rises above the waves; all the islands are atolls; none of these are upraised, but there are several submerged banks. The existence of this long line of atolls seemed to be one of the strongest arguments in favour of Darwin's theory of the formation of coral reefs.

In Diego Garcia the nature of the soil varies considerably from place to place. In some localities it consists of nothing else than bare coral rock upon the surface of which coral boulders are scattered about; in other places it is composed wholly of calcareous sand, and one may dig down for 6 or 8 feet without finding coral rock. It is obvious after a short examination that some parts of the land are older than others, and that the great strip of land was formerly a series of disconnected islets which have since been joined together by the accumulation of sand and coral *débris* between them. In the older parts of the island, which have apparently been covered with vegetation for a considerable period, a thick peaty mould has been formed by the decay of fallen leaves and stems of trees and shrubs.

Throughout the island the outer or seaward shore is higher than the inner or lagoonward shore, owing to the pile of coral boulders thrown up in the form of a low rampart along the former by the action of the waves. In most places a flat reef extends fully 60 yards seaward of the rampart; and this reef is just uncovered at low spring tides. As a rule the inner shore slopes gently down into the lagoon for some distance, and then pitches down rather suddenly to a depth of 10 or 12 fathoms, but in some places there is a depth of 6 or 8 fathoms close up to the inner shores. Marshy pools of fresh or brackish water are found in the centre of the strip of land on the south-east and west sides of the island; into these the sea enters in many cases during the highest spring tides, and at the south-east and south ends of the island it has established permanent breaches into some of these pools, through which the tide runs in and out regularly from the lagoon. Thus there are formed sheets of water like secondary lagoons within the strip of land; these are known on the island by the name of *barachois*, and they are of some importance when one comes to consider the amount of change which is continually going on in the island.

Externally the shores slope away very rapidly to considerable depths, the sounding-line giving depths of 250 fathoms and upwards at a distance of a few hundred yards from the edge of the reef, excepting at Horsburgh Point at the south-east side, where a depth of 45 fathoms is found at a distance of 1 mile from the shore. After a stay of two or three months on the island one cannot fail to be impressed with the immense amount of

[1] By G. C. Bourne, B.A., F.L.S., Fellow of New College and Assistant to the Linacre Professor in the University of Oxford. Communicated to the Royal Society by Prof. E. Ray Lankester, F.R.S.

change which is continually in progress. Large masses of sand are in the space of a month deposited in one spot to be swept away during the next month and deposited in another. Everywhere there is evidence that the sea has encroached upon the land, or that the land has in its turn gained upon the sea. In one place numerous dead and fallen cocoa-nut palms show where old-established land has been carried away; in an adjoining spot tracts of sand, either bare or covered with a scanty growth of young shrubs, show where the combined action of wind and waves has added a new piece to the island. Within the lagoon the currents are constantly changing in force and direction, and their every change affects the growth of coral in their track. In estimating the structure of the atoll these changes should be kept in mind, although their complexity makes it far more difficult to arrive at a correct conclusion.

In the course of my investigations I learnt to distinguish the following kinds of coral rock formed by the action of the waves or wind, or both combined.

Firstly, *reef rock*, a tolerably homogeneous mass of compacted coral *débris*, the component parts of which are so thoroughly infiltrated with carbonate of lime held in solution in the sea-water that the masses of fragments of coral composing the rock are rarely distinguished from one another. This form of rock exhibits a fine horizontal stratification; it is invariably formed under the sea or between tide marks.

Secondly, *boulder rock*, formed just above high-tide mark by means of the masses of coral which are transported across the reefs by the waves and are piled up to form the low rampart already alluded to. The interstices of the boulders are soon filled up with coral *débris* and sand, and are cemented together by the spray. Such rock is only formed on the seaward shores, and invariably shows a stratification dipping downwards towards the sea.

Thirdly, *shingle rock*, which may be of two kinds. The first kind is horizontally stratified, and is scarcely distinguishable from reef rock, except in its finer texture; it is formed below water or between tide marks by the agglomeration of small pieces of broken coral, among which are included numerous shells of mollusks, remains of Crustacea, Echinoderms, &c., and in the more sheltered parts of the lagoon it may include considerable masses of dead Madrepores embedded in their natural position in the rock. This rock is of a looser texture than the reef rock. The second kind of shingle rock is formed above high-water mark by the action of the waves. It is entirely composed of small fragments, and exhibits a fine stratification dipping seawards at an angle.

Lastly, there is the *sand rock*, formed above water by the action of the wind. Wherever masses of fine sand are piled up within reach of the spray they are gradually compacted, and form a friable rock, the stratification of which dips seaward.

In many parts of the island I observed that the land was composed of stratified reef or shingle rock, the strata of which were perfectly horizontal, and did not dip down towards either shore. Having observed the manner in which the different kinds of coral rock were formed, I was at a loss to understand how such horizontally stratified masses could have been formed in their present position above high-water mark, and could only believe that they were originally formed as reef or shingle rock below high-water mark, and had been subsequently raised to their present position. I was thus led to believe that a slight elevation had taken place, and this belief was strengthened by a study of the formation of East Islet. This islet is about 800 yards long, and nearly 100 yards broad; its westermost extremity is composed of masses of sand piled up on the underlying reef rock, and in this place there is a clump of high trees (*Hernandia peltata*). The eastern and by far the larger part of the islet is of different formation. The even surface of the soil is covered with a low scrub, but bears no trees nor cocoa-nut palms. It forms a low plateau, the surface of which does not slope down towards the lagoon, but is perfectly horizontal, and stands 4 feet above the very highest spring tides. The shore on the lagoonward side shows an abrupt fall of 6 feet to the reef, which in this place extends for a distance of 50 yards towards the lagoon, and is only left uncovered at the lowest spring tides. At the eastern extremity of the island there is no reef, but from 1½ to 2 fathoms of water are found within a few yards of the shore. This point is exposed to the ocean, and a strong and constant current sets against it, so that it is undergoing a considerable amount of erosion. On the north or seaward side the reef again extends outwards from the shore, the latter differing from the inner shore in the presence of a talus of large boulders which have been thrown up against it by the waves. Wells have been sunk in numerous parts of the island, though, for some reason which I cannot explain, water is only found in one of them. Numerous pits, some of which are 9 feet deep, have also been dug for the purpose of planting cocoa-nuts. These pits and wells expose the interesting structure of the superficial part of the island. Beneath a thin surface layer of sand and mould lies a horizontal layer of stratified shingle rock, in which large embedded coral masses may occasionally be distinguished; this layer is about 2½ feet thick. Beneath is a layer of loose coral sand about 18 inches thick, and beneath that is another layer of coral rock of the same character as the first, and about 3 feet thick. Beneath this is another layer of friable sand lying on the solid reef rock into which the excavations did not penetrate. These layers lie perfectly horizontally, and do not dip in any direction. They crop out above the reef on the steep eastern and southern shores, and as the loose sand is washed out by the waves the overhanging layer of rock breaks off and falls down in large masses. The central parts of this area are absolutely beyond the reach of any waves at the present time, and as the strata of rock and sand run evenly through it there is no evidence of its having been formed by successive additions of material through the action of the waves. Nor can it possibly have been formed under the surface of the water unless it has since been raised to its present position, for, as I have said, its upper surface is 4 feet above the level of high spring tides. On one occasion when the tide rose to an abnormal height and invaded several parts of the main island, I saw that the water reached to within 3 feet of the top of the shore, but even then the whole of the upper stratum of coral rock was well above the waves. It is scarcely credible that an even layer of shingle rock could have been formed above the highest high-water mark.

My belief in elevation is further strengthened by the following facts, communicated to me by M. Spurs, a resident for twenty-five years at Diego, an ardent naturalist, and much interested in coral formations.

A small shore crab of the genus *Ocypus* is always to be found on the sandy flats between high and low water marks. These crabs, as is well known, form numerous galleries in the fine muddy sand, which they line with seaweed, &c., to prevent their falling in. These galleries open to the surface by short passages placed perpendicularly, the mouths of which open only a few inches above the level of low tide. This crab is only found on the shore between tide marks; on the dry land its place is taken by *Gearcinus*, another genus of crab, which forms different burrows. In the west part of East Islet there is an aggregate of friable, scarcely compacted sand, which has somewhat the appearance of half-dried clay. It lies 5 feet above high-water mark, and was found by M. Spurs, during some excavations which he had to make for the purpose of constructing a slip for boats, to be riddled with the sea-weed-lined galleries of *Ocypus*, evidently long since disused and empty.

Having made this observation on East Island, M. Spurs made a search in similar formations on the main island, and found, he tells me, precisely the same facts in several instances, aggregates of sand lying at some distance above high-water mark, riddled with the abandoned burrows of *Ocypus*. Now, since the burrows of *Ocypus* are quite characteristic, and could not have been mistaken by so good an observer as M. Spurs for those of another species, and since they are in the present day only found between tide marks, these observations afford a further presumption in favour of a slight elevation having recently taken place. In any case they preclude the idea of any subsidence being in progress, as Mr. Darwin fancied to be the case in the Keeling atoll. M. Spurs further informs me that, during the time that he was superintendent of the oil company's estate, he caused more than 30,000 pits to be dug on the main island for the purpose of planting cocoa-nut palms, and that he frequently observed in different localities the same alternate layers of sand and rock that I have described as existing on East Island. These alternations of sand and rock would suggest alternations of very slight subsidence with very slight elevation, rather than a single movement of upheaval, yet on the supposition that all the layers were formed beneath the water, as their horizontal stratification leads me to believe, I can venture on the following explanation. The mass of rock which forms the base upon which the islets and other dry land rests is solid reef rock, and the whole floor of the lagoon is similarly formed.

The latter is covered at depths of 3 or 4 fathoms and upwards by a layer of fine sand, which may attain a thickness of 2 or 3 feet. In protected parts of the lagoon and in spots where the changeable currents have ceased to deposit any quantity of sand, corals will grow in considerable quantities, chiefly those wide-spreading species of Madrepora which cannot find a lodging on the exterior of the reef, where they would be dashed to pieces by the waves. By the continual growth of new colonies on the top of the old ones which have died, a layer of solid rock of considerable thickness may be formed. Whilst diving for corals at the lower part of the lagoon, I often noticed such layers of half-formed rock on which living coral was growing or not, according as the constantly changing currents were at that time throwing up sand in the locality or not. Thus on the west side of the lagoon, off Point Marianne, there are large tracts of recently formed coral rock, on which no living corals are to be seen, whilst on the east side of the lagoon, exactly opposite to Point Marianne, a similar basis of rock is luxuriantly covered with growing coral.

Now, as the currents are constantly changing, and as the changes may, as I saw, affect an area some miles in extent, one may suppose that an area was first covered with corals growing on the sand, which everywhere covers the reef rock, when the latter lies more than a fathom below the surface. A change in the currents brought abundant sand to the spot, killed the corals, and deposited an even layer of sand of some little thickness over the rock formed by the skeletons of the dead corals. A further change in the currents would again render the spot suitable for coral growth, and a new layer of rock would be formed over the last layer of sand. I have seen quite analogous formations in progress in a fathom of water a little way above Point Marianne. Raise the formation to the surface, and you get that stratification which occurs in so many parts of the island, a stratification which cannot be explained on any theory of subsidence, and is scarcely less difficult to explain on the supposition of rest. At first I had some hesitation in extending to an island on the borders of the lagoon, as is East Island, a view of the formation of layers of sand and rock derived from an inspection of the interior of the lagoon, but afterwards I saw that similar layers were being formed just within the large reef known as Spur's Reef, west of Middle Island, so that no objection can be raised on that score. The whole character of the Chagos Group is very much opposed to the theory that atolls and barrier reefs are formed during subsidence. There are several atolls rising above the waves, that of Peros Banhos being 55 miles in circuit, and composed of numerous small islets placed upon a ring-shaped reef through which there are several large and deep channels. Egmont or Six Islands is an instance of an atoll in which the encircling reef is perfect and unbroken by any channels, the land consisting of six islets placed for the most part on the southern and western sides of the reef. There are several submerged banks, nearly all of which have an atoll form. Of these the best known is the Great Chagos Bank, a huge submerged atoll 95 miles long and 65 miles broad, having a depth of 4 to 10 fathoms over a narrow rim around its periphery, and a central lagoon of a depth varying up to 45 fathoms. South-west of the Great Chagos Bank, distant less than 15 miles, lies the atoll of Six Islands, and on the other side of these, scarcely 12 miles distant, lies another submerged atoll, known as Pitt's Bank. South-west of Pitt's Bank are two smaller banks, Ganges and Centurion's Banks. Darwin considered that the Great Chagos Bank afforded particularly good evidence of the truth of the subsidence theory. He regarded it as an atoll carried down by a too rapid subsidence below the depth at which reef-building corals flourish. The same would be the case for Pitt's Bank and the two others just mentioned. A more intimate knowledge of the Great Chagos Bank, and of the relations of it and other submerged banks of existing land, shows this view to be untenable. In the first place, the rim of the Great Chagos Bank is on an average not more than 6 fathoms below the surface, and therefore situated in a depth eminently favourable for coral growth, and there are actually six islets on the northern and western edges rising above the water, and some of them inhabited. Secondly, any such rapid subsidence could not have affected areas only 30 miles apart without involving the Six Islands atoll lying directly between them. A similar argument might be extended to the more northern islands of the Chagos Group, and even to Diego Garcia itself, although it lies somewhat apart from the rest of the group. Again, if atolls and barrier reefs are formed around subsiding peaks, it is at least curious that throughout the Laccadive, Maldive, and Chagos Groups there are no instances of high islands surrounded by barrier reefs, marking the last remnants of pre-existing land. In the more western parts of the Indian Ocean, between Madagascar and the Seychelles, there are numerous atoll islands, and in long. 60° E. there lie the submerged Saya de Malha Bank and the reef known as Cargados Carajos. Between these two lies the extensive Nazareth Bank, having over it depths of from 14 to 45 fathoms. The Saya de Malha Bank appears to have the characters of a submerged atoll, having a central depression of 65 fathoms, surrounded by a rim which has only 8 to 16 fathoms on its eastern side, but 22 fathoms on the western. Some of the groups north of Madagascar afford very good evidence of upheaval. Aldabra Island, situated in lat. 9° 22′ S., long. 46° 14′ E., is a perfect instance of an upraised atoll. Captain Wharton describes the external shores as consisting of low coral cliffs, about 20 feet high, the surface of the land being composed of jagged coral rock. The lagoon is entered by a passage varying from 11 to 5 fathoms in depth, but its internal portions are either very shallow or partly dry at low water. Not far distant is the Cosmo Ledo Group, a perfect atoll, with a lagoon some 4 fathoms deep, or less. There are ten islets of various sizes on the reef, and all of them appear to have been elevated some 10 feet. There are some hills 40 and 50 feet high on the two largest islands, but these appear, according to Captain Wharton, to be formed of blown sand. The Farquhar Group and Assumption Island, situated within the same area, have been raised, according to the same authority, some 10 feet. Providence Island, in lat. 9° 14′ S., long. 51° 2′ E., appears to be a low island situated upon the edge of the atoll-shaped Providence Reef. At a distance of 19 miles from Providence Island is the island of St. Pierre, which has no fringing reef. It is particularly interesting, for although it is in close proximity to the low Providence atoll, it has been raised about 40 feet above high water, and in the absence of a fringing reef the sea breaks with great violence against a low cliffy coast, hollowing out a number of caverns which, from the description given in the sailing directions for Mauritus and its islands, appear to open inshore by "blow-holes." [1]

Near and among these raised coral formations are several submerged banks, the most important of which is the McLeod Bank, situated in lat. 9° 57′ S., long. 50° 20′ E., between Providence Island and the Cosmo Ledo Group. The details show that there is a group of coral formations, situated in lat. 10° S., north of Madagascar, in which are found raised atolls—atolls whose dry land just rises above the waves and submerged banks. There can be no clearer proof that atolls are formed in areas of elevation, and, if the facts which I have already stated concerning Diego Garcia are of any weight, it would seem that most of the coral formations of the Indian Ocean mark areas of elevation rather than of rest, certainly they are not evidence of subsidence.

Those who have felt that the evidence brought against Darwin's subsidence theory is too strong to be resisted, must often have felt that no satisfactory explanation of the lagoons of atolls or the lagoon channels of barrier reefs has been given in its place. Semper was the first to suggest that the lagoon was formed by a solution of the interior parts of the reef, and more recently this view has been urged with great force by Murray, who points out, in addition, that corals on the periphery of a reef must, from their position, get the advantage over those more interiorly situated, being more directly in the track of food-bearing currents. Neither of these explanations has completely satisfied me. That sea-water exercises a solvent action upon carbonate of lime does not admit of doubt, and that the scour of tides, combined with this solvent action of the water, does affect the extent and depth of a lagoon is obvious. But I challenge the statement that the destructive agencies within an atoll or a submerged bank are in excess of the construction. It would be nearer the mark to say that they nearly balance one another. In the first place, the carbonate of lime held in solution by sea-water is deposited as crystalline limestone in the interstices of dead corals or coral *débris*. Anyone who is acquainted with the structure of coralline rock knows how such a porous mass as a Mæandrina head becomes perfectly solid by the deposition of lime within its mass. This deposition can only be effected by the infiltration of sea-water. In reckoning the solvent action of sea-water, therefore, account must be taken of the fact that a not inconsiderable proportion of the carbonate of lime held in solution is redeposited in the form of crystalline limestone. Of

[1] For the information on the islands north of Madgascar I am indebted to the courtesy of Captain W. J. L. Wharton, R.N., F.R.S.

this, it seems, Mr. Murray has not taken sufficient account, and has, therefore, overstated the destructive agency of the sea. Secondly, the growth of corals, and the consequent formation of coral rock within the lagoon, is generally overlooked.

Whilst diving for corals at Diego Garcia, I had abundant opportunities of studying the formation of coral rock within the lagoon, in depths under 2 fathoms. The layers of tolerably compact rock thus formed are of no mean extent or thickness; they soon become covered with sand, and are thus protected from the solvent action of the water. I have found it impossible to reconcile Mr. Murray's views with what I saw of coral growth within a lagoon. Not only do the more delicate branching species of the Madreporaria flourish in considerable numbers, but true reef-building species—Porites, Mæandrina, Pocillopora, and various stout species of Madrepora—are found there. It is a mistake to suppose that certain species of corals are restricted to the external shores, others to the lagoon. My collections proved that many of the species growing in the lagoon at distances of 5 miles and upwards from its outlet are identical with those growing on the outer reef. In addition to them are numerous species, such as *Seriatopora stricta*, *Mussa corymbosa*, *Favia lobata*, *Fungia dentata*, and many others that are not found on the outside. The reason is that the last-named are either free forms, such as Fungia, or are attached by such slender and fragile stems to their supports that they could not possibly obtain a foothold and maintain themselves among the powerful currents and waves of the open ocean.

These various species, numbers of which grow close together, form knolls and patches within the lagoon, and it cannot be doubted that their tendency is to fill it up.

These considerations have led me to discredit the solution theory as an explanation of lagoons and lagoon channels, and other objections have been lately urged with great force by Captain Wharton. The conclusion which I reached, after carefully considering the conditions of submerged lakes of atoll form, is that the ring-shape of the outer reef is to be explained by the peculiarly favourable conditions for coral growth found on the external slopes. Although corals may, and do, flourish in lagoons, they are only found in knolls and patches, and are always liable to be smothered, when, by a change in the tidal currents, sand is thrown down upon the place where they are growing. On the external slopes, however, corals grow in extraordinary abundance, and chiefly those massive forms whose skeletons take so conspicuous a share in the formation of coral rock. If once it is admitted that the periphery of the reef offers peculiarly favourable conditions to the growth of reef-forming corals, it follows that, as the reef rises to the surface, its external parts will outstrip the more internal, and will reach the surface first, forming a rim around a central depression or lagoon. This elevated ring will be as marked a feature in submerged as in complete atolls.

Corals are always thickest along the slopes around a coral reef, and the reef tends to increase at its periphery, growing upwards there, whilst it tends at the same time to spread outwards. These principles hold good in the case of a submerged bank as well as in the case of a reef that is awash, and a submerged bank must tend in the course of time to reach the surface in its circumferential portions, and form an atoll-shaped reef, on the rim of which detritus may be heaped from place to place, forming shingle cays or islets which may temporarily form dry land. In atolls where storms are of frequent occurrence, regular storm-beaches may be formed, till the fragments piled high upon one another may form low islets standing some 6 or 10 feet above high-water mark, upon which vegetation may subsequently find a footing. Atolls are often formed in this way, without any elevation taking place, and such has undoubtedly been the case in the Florida reefs, where atolls (the Tortugas) and barrier reefs and islands have been formed in an area of complete rest. No one who has read the admirable work of Alex. Agassiz on the Florida reefs can fail to agree with the author's conclusion that the islets there have been formed by the action of the wind and waves alone, without any assistance from the upheaval of the bed of the sea. But I am not satisfied that this has been the case in the Chagos Group. Storms are of very infrequent occurrence there, and the horizontal masses of reef rock standing above high-water mark cannot be attributed to the normal action of the prevailing winds and currents.

In the Florida reefs the nature of the soil betrays its origin—its strata slope towards the sea on every side, and the lamination of the rocks attests the long-continued action of waves and spray. But the alternate horizontal layers of sand and rock occurring so abundantly at Diego Garcia are quite different; they do not dip seawards, their composition differs from the rocks of the Florida reefs, and their edges, instead of showing signs of accumulation of fresh material, are often bluff, and show that the sea is gradually eating them away. It is difficult to explain these appearances except on the hypothesis of slight elevation. It might be objected that if any upheaval had taken place, the banks lying at various depths below the surface would have been raised to different heights, and that it would be in the highest degree unlikely that so many would be raised some 4 or 5 feet above high-water mark and no more, throughout so large areas as the Laccadive, Maldive, and Chagos Islands, and the various low groups in the Pacific. The force of the objection must be admitted, but it may be observed that atolls raised from 10 to 40 feet above the waves are not so uncommon as has been hitherto supposed, and that the numerous submerged banks lying at very various depths show that all the reefs have not been raised to one height in a single area of elevation. The uniform level of many atolls and barrier reefs admits of a further explanation. A reef raised some few feet above the sea-level is at once attacked by the waves, and as the rim is very narrow, it must soon be worn away till the whole of the land is eaten away, and its surface is brought awash once more. Thus every slight movement of elevation would soon be compensated by the denuding action of the waves. The island of St. Pierre, already described, is a good instance of this process of erosion. It cannot be doubted that this island, which has recently been raised 40 feet, is undergoing rapid waste, and must soon be reduced to the level of the sea. At Diego Garcia I was astonished at the rapid destruction of dry land which is in progress, on the outside as well as the inside of the lagoon. The destruction is not so great on the outside as on the inside as a rule, for in the former case the rampart of coral boulders thrown up by the waves compensates in many places for their erosive action. But in the bay above Horsburgh Point, exposed to the full strength of the south-east trades, the destruction is very great. M. Spurs, an old resident of the island, writes to me on this subject: "Cette destruction est très rapide; Diego perd en moyenne un pied de terrain par an, tant intérieurement qu'extérieurement, excepté aux pointes nord-est et nord-ouest, où une partie des sables, entrainés du fond de la baie par les vents de sud-est, conservent à ces deux pointes leur largeur première."

M. Spurs has over-estimated the rate of destruction, but there can be no doubt that it is very considerable. It is most conspicuous along the shores bordering the lagoon. The stumps of cocoa-nut palms, the newly-made breaches into the land, forming shallow inland lagoons, the vertical faces of old banks of half-consolidated sand, all attest it. Just above Point Marianne is a road running along the lagoonward shore, which when I left the island had been narrowed by the action of the sea to a mere path, and was in some places almost impassable, as the sea had made clean breaches across it, and found its way into some shallow fresh-water lagoons lying on the other side of the road. I was assured that this road had been over 12 feet wide some years previously, and that it was formerly separated from the lagoon by a narrow strip of land of an equal width. Perhaps the best evidence of the destruction of land is afforded by the "barachois" at the southern extremity of the island. These barachois are inland lagoons connected with the main lagoon by a narrow outlet some 2 fathoms deep or more. They are filled and emptied every tide, and their floor is intersected by numerous small channels running in every direction. No corals grow within the barachois, and a slight study convinces the observer that the daily scour of the tides is denuding their shores and floors very considerably.

Barachois are formed in the following way:—During unusually high tides, when the waters of the lagoon are dammed back by a north-westerly wind of unusual violence, the water rises to great heights and invades the land in several places. In some instances it actually makes a breach in the lagoonward shore, and fills up the shallow depressions which are often found in the middle of the strip of land. A pool of salt water is thus formed, which kills the cocoa palms and other vegetation growing in its bed, and, as this process is repeated again and again, in the course of a few years a channel is cut out between the pool and the lagoon, which finally becomes so deep that spring tides, and finally even neap tides, run in and out of the pool regularly. As soon as these conditions are established, the channel is scoured out and deepened, and the daily tides scour out the bed of the pool, forming a complete barachois.

It is not easy for one who has not seen it to understand how

much of the loose soil of a coral islet can be moved by a single tidal encroachment. It happened that I was riding past the very thin strip of land between Minni Minny and Barton Point the day after an abnormally high tide. The strip of land here is not more than 30 yards across, and the sea had washed right over it on the previous day, clearing away an amount of soil which was almost incredible. My companion, M. Casimir Leconte, told me that the sea had not been known to wash over this place before. It was apparent that, after a few more of such high tides as I had witnessed, a permanent breach would be made at this spot, and another lagoon outlet would be formed, which would be continually deepened as the tide set through it. At the south-eastern side of the island I noticed that the land was being rapidly destroyed on the outer shores just opposite to a half-formed barachois, whose margins are situated not 60 yards from the outer shore. If the same process of external destruction continues, whilst the barachois is deepened and scooped out from within, it will not be many years before the ocean makes a new channel into the lagoon at this point. Thus the continuous strip of land which now nearly encircles the lagoon of Diego Garcia is tending to be split up again into a series of islets. At the points where the breaches are made the tides and ocean currents will rush with great force into the lagoon, and will scour out deep channels similar to that now existing between Middle and East Islets.

These facts taken together show how the normal action of tides, winds, and waves is constantly tending to lower to the sea-level any dry land that may have been formed by elevation or otherwise. It does not seem to me to be surprising that the majority of atolls and barrier reefs are, under such circumstances, only just able to maintain their surfaces above the sea-level.

No explanation of atoll formation would be complete if it did not include an explanation of the Maldive atolls. This has been felt by Darwin, who has explained the formation according to his theory. Without attempting to enter into a lengthy discussion, I will give my own explanation of the atoll. Tilla-dou-Matte atoll is, as is well known, a huge atoll composed of atolls. The islets forming the rim of the main atoll are themselves atolls with their own lagoons; the main lagoon contains a few secondary atolls corresponding to the coral patches in an ordinary atoll. It will be generally admitted that coral reefs are constantly increasing to seaward because of the excessive growth of coral on their external slopes.[1] As the inward shores of an atoll are constantly being removed, and an atoll if completely formed tends to be broken up again into small islets when it has reached a certain size, and as the channels between the islets must be continually deepened by the scour of the tides until deep passages are formed, an atoll like Diego Garcia may be expected to reach in time a condition like that of Peros Banhos. It is probable that a large bank like the Great Chagos Bank, when it reaches the surface, can never give rise to a continuous strip of land, but must consist of a chain of islets separated by channels of some depth and by tracts of submerged reefs. The islets and tracts of reef in either case would be bounded by deeper channels, and these channels, swept by strong currents, would become wider and deeper, for corals could not thrive in them. After a time the islets would become so far isolated, and the entries into the lagoon would become so large and numerous, that oceanic conditions would prevail in the lagoon, and then there would be around each separate islet or piece of reef all the necessary conditions for the formation of a new atoll. The currents would impinge upon one side of the islet or reef, sweep round it, and give a backwash at the further side; the corals would flourish in the circumferential parts of the reef surrounding the islet, and new atolls with shallow lagoons would be formed.

In Tilla-dou-Matte the lagoons of the secondary atolls are tolerably deep. In this case they must have been formed before any land reached the surface. Applying the same reasoning as in the former case, it can readily be understood how in the case of the Great Chagos Bank, which has wide and deep breaches in many places, the isolated reefs as they grow to the surface must tend to assume an atoll form. An examination of the chart shows that this is the case. The Great Chagos Bank in the course of time will rise to the surface as an atoll composed of secondary atolls or atollons, similar to, but on a smaller scale than, the Tilla-dou-Matte atoll. The explanation of atollons in the centre of a large lagoon in which oceanic conditions have been established, is quite obvious.

[1] This statement may at first sight seem at variance with what I have just said about the rapid destruction of land on the outer and inner shores of an atoll; but in the latter case it is *land above water* that is destroyed. Coincidently with this process the reef rock below water is constantly tending to raise itself and to spread in all directions, owing to the perpetual growth of corals and the accumulation of their skeletons.

THE ROYAL HORTICULTURAL SOCIETY.

On Tuesday, March 27, the Scientific Committee of the Royal Horticultural Society met in the Committee-room of the Drill Hall. Among the numerous subjects brought forward were the following:—

Dispersal of the Seed in Pinus insignis.—Dr. Masters, alluding to the great differences that exist in the species of *Pinus*, as to the time at which the constituent scales of the cone separate in order to liberate the seed, showed a series of cones of *Pinus insignis*, the oldest of which bore the date 1864. In this all the scales were widely separate. The most recent cones dated from 1877, and in them the scales were not at all separated. Between these two extremes, cones were shown exhibiting almost every intermediate stage of separation. It is to be remarked that the separation begins generally just above the centre of the pendulous cone on the side furthest away from the branch, at the place where the excentricity of the cone, due to the free exposure to light and air, and the absence of obstacles afforded by the branch was greatest, and that it follows a spiral course towards the base of the cone. The scales separate in successive spiral coils, till, at length, all except a few at the base and apex respectively, and which are probably sterile, are separated one from the other.

Semi-double and other Orchids.—Dr. Masters explained the construction of numerous malformed orchids which were interesting as throwing light on the morphology of the order. Some extraordinary malformations of Fuchsias were shown, and a drawing was exhibited of a magnificent new Anthurium, which had appeared accidentally with an importation of *Cattleya Gaskelliana*, in the garden of the Right Hon. J. Chamberlain. The heart-shaped leaves are of gigantic size, and the large boat-shaped spathe is of the richest crimson colour.

Eucalyptus urnigera.—Dr. Masters showed specimens of this Tasmanian species in flower and fruit. They had been received from Whittinghame Gardens, Prestonkirk, near Edinburgh, not far from the sea, and where the tree is perfectly hardy.

Daffodil with Crested Corona.—Rev. E. C. Gabbett sent through Dr. Masters two flowers of a curious Daffodil from plants growing on his lawn in Co. Limerick. The "frill," or outgrowth, is produced from the outer surface of the corona, which has thus a very peculiar appearance.

Douglasia lævigata.—Mr. G. F. Wilson alluded to this plant as having been shown for the first time. It is a low-growing Primulaceous plant, with tufted leaves and lilac flowers, like those of an Androsace, but larger, and with the tube of the corolla longer than the calyx, and with only two seeds to the capsule. The species are the natives of North-Western America, the first known species having been collected by Douglas not far from the sources of the Columbia River, and named in his honour by Dr. Lindley.

Araucaria imbricata Timber.—Mr. Ford, gr., Leonardslee, exhibited slabs of wood cut from a tree of this species, and which at 6 feet from the ground girthed 26 inches, the tree being 35 feet in height. The wood was yellow, soft, evenly grained, and, judging by the distance between the rings, quickly grown.

Numerous other plants and objects of interest were exhibited and commented on.

SOCIETIES AND ACADEMIES.

London.

Royal Society, March 15.—"A Class of Functional Invariants." By Mr. A. R. Forsyth, F.R.S.

The memoir is occupied with the investigation of a class of functional invariants, constituted by combinations of the partial differential coefficients of a dependent variable, z, with regard to two independent variables, x and y. The definition of the invariant is given by the property that, when the independent variables are transformed to X and Y, and the same combina-

tion as before is formed with regard to these new variables, the equation

$$\Phi = \left\{ \frac{\delta(x, y)}{\delta(X, Y)} \right\}^m \phi$$

is satisfied.

The transformations for which any detailed results are given are of the general homographic type. The characteristic properties of such invariants are :—

(i.) Every invariant is explicitly free from the variables, but necessarily contains both the differential coefficients p and q of the first order.

(ii.) It is homogeneous in the differential coefficients, and is of uniform and the same grade in differentiations with regard to each of the independent variables.

(iii.) It is symmetric or skew symmetric with regard to these differentiations.

(iv.) It satisfies four differential form-equations and two index-equations, all linear and partial of the first order.

An invariant is said to be proper to the rank n, when the highest differential coefficient of z which occurs in it is of order n. By means of the solutions of the form-equations, the following propositions relating to irreducible invariants in a single dependent variable, z, are established :—

Invariants can be ranged in sets, each set being proper to a particular rank.

There is no invariant proper to the rank 1; there is one proper to the rank 2; there are three invariants proper to the rank 3.

For every value of n greater than 3 there are $n + 1$ invariants proper to the rank n, which can be chosen so as to be linear in the partial differential coefficients of order n.

Every invariant can be expressed in terms of this aggregate of irreducible invariants; and the expression involves invariants proper to rank no higher than the order of the highest differential coefficient which occurs in that invariant.

Some of the properties of the irreducible invariants involving differential coefficients of two dependent variables are obtained, and, in particular, it is shown that there is a single irreducible simultaneous invariant proper to the rank 1, and that there are four such invariants proper to the rank 2.

The theory of eduction is next considered, with some examples. Finally, it is shown that the theory of binary forms can be partly connected with functional invariants.

March 22.—"Second Preliminary Note on the Development of *Apteryx*." By T. Jeffery Parker, B.Sc., C.M.Z.S., Professor of Biology in the University of Otago. Communicated by W. K. Parker, F.R.S.

Chemical Society, March 15.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read :—The nature of solutions as elucidated by the heat evolved on their dilution; Part 1, calcium chloride, by Mr. S. U. Pickering. To determine the nature of the action which takes place on diluting aqueous solutions, the author has examined calcium chloride, and, in a series of elaborate experiments, has obtained results which form a curve of great regularity. This regularity, however, is only apparent, since on differentiation a number of independent curves are obtained, each of which on further differentiation gives a straight line. The points at which these lines meet, when produced, indicate percentages of water corresponding to distinct hydrates of the salt, and moreover coincide in every case, within the limits of experimental error, with the points obtained by treating in a similar manner the curve expressing the densities of the various solutions. The author contends that these results, taken in conjunction with the fact that the variation in the electrical conductivity and the density of sulphuric acid on diluting with water also point to the existence of certain hydrates in solution, make it no longer reasonable to doubt that solutions do in reality consist of such hydrates, and is of opinion that any theory of the nature of solutions which ignores their existence must be rejected absolutely and for ever. A new form of mixing calorimeter, devised for these experiments, was exhibited.—The action of thiocyanates on aldehyde-ammonias, by Dr. A. E. Dixon.—Carboxy-derivatives of quinone, by Dr. J. U. Nef. Ethylic paradiketohexamethylenecarboxylate, obtained by the reduction of ethylic quinonetetracarboxylate with zinc dust, exists apparently in three distinct modifications, only two of which, however, have been studied—the one modification is green and crystallizes in needles, the other is yellow and crystallizes in plates; after fusion, the former appears dark yellow and the latter bright yellow. If either modification be separately dissolved in carbon bisulphide, a solution is obtained from which the two substances crystallize out together; the solution also has the same colour and the same absorption spectrum whichever modification be dissolved. The author calls attention in the paper to a number of similar cases of dimorphism.—The action of acetone on ammonium salts of fatty acids in the presence of dehydrating agents, by Dr. S. Ruhemann and Mr. D. J. Carnegie.—A method of estimating nitrites either alone or in presence of nitrates and chlorides, by Mr. T. C. Day.

PARIS.

Academy of Sciences, March 26.—M. Janssen in the chair.—New theory of the equatorial *coudé* and of equatorials in general (continued), by MM. Lœwy and P. Puiseux. Here are given the general formulas promised in the previous communication, together with the terms depending on the position of the outer glass.—On the relations of atmospheric nitrogen with vegetable humus, by M. Th. Schlœsing. A detailed account is given of the experiments carried out according to the already described method for the purpose of ascertaining whether gaseous nitrogen is fixed by vegetable soil. The disappearance of the oxygen shows in six different cases that the combustion of the organic substances takes place in various degrees depending on the quantity and nature of such substances. During this combustion nitric acid is formed with disappearance of the ammonia. The volume of gaseous nitrogen contained in the soil does not perceptibly vary.—On the absorption of saline substances by plants (continued), by MM. Berthelot and G. André. The experiments here described deal with the acetate of potassa, an organic salt analogous to those present or produced in the plants; also with the nitrate of potassa, the formation or accumulation of which is characteristic of certain species, especially of the Amaranthus group. This accumulation is shown to depend rather on the period of vegetation than on the proportion of the salt in the ground.—New nebulæ of a remarkable character discovered in the Pleiades, by means of photography, by MM. Henry, and described by M. Mouchez. Besides a new nebula round Maia in the Pleiades, the more recent researches of MM. Henry have revealed a great mass of cosmic matter covering a large part of this constellation. But the most remarkable discovery, and one of an absolutely unique character, is a rectilinear thread of nebular matter projected from the central mass nearly in the direction from east to west for a distance of 35′ to 40′ of arc, but with a thickness of no more than 3″ to 4″. This thread crosses on its path seven stars, which it seems to string together like the beads on a rosary, and slightly changes its direction at the point where it meets the largest of these stars. A second streak, somewhat similar, but shorter, is perceptible in the middle of the nebular mass.—Preliminary work for the execution of the photographic chart of the firmament, by M. Mouchez. Reference is made to the publication of a *Bulletin* specially devoted to this object. Two more Observatories, those of Potsdam and Oxford, are announced as intending to take part in this great work, making thirteen stations altogether. These, it is stated, are already sufficient to secure the completion of the undertaking in the course of four or five years.—Treatment of auriferous sands by amalgamation, in ancient times, by M. Berthelot. The second part, just published, of the already noticed "Collection des Alchimistes grecs," contains the works of Zosimus, a writer of the third century of the new era, dealing with the extraction of gold by means of its natural ores treated with mercury. This process appears to have been substituted for a still more ancient method, in which the ore was fused with lead, salt, a little tin, and barley bran, and submitted to a genuine process of refining.—Observations of the Comet 1888*a*, made at the Paris Observatory with the equatorial of the West Tower, by M. G. Bigourdan. The observation here recorded was taken on March 25, when the comet, discovered at the Cape, on February 18, by M. Sawerthal, was approaching the northern hemisphere.—On a new mercury-bath for the observation of the nadir, by M. Périgaud. This valuable appliance at last gives the long-sought solution of the problem, how to employ the mercury-bath for determining the vertical, and for taking observations by reflection in all states of the weather, and on ground subject to the constant vibrations produced by carriage traffic, as in large towns.

Berlin.

Meteorological Society, March 6.—Dr. Vettin, President, in the chair.—Dr. Zenker gave an account of his work, which has been awarded a prize by the Paris Academy, on the distribution of heat over the surface of the earth. When considering the total heat which reaches the earth's surface, it is of course dependent upon the distance of the sun, and is greater at perihelion than at aphelion in the ratio of the inverse square of the sun's distance. The varying ellipticity in outline of the earth in its various positions has no influence on the heat received owing to the extremely slight difference thus produced. If any one point of the earth's surface is alone considered, then the heat received is determined by the sine of the sun's altitude or the cosine of its zenith distance, for which the speaker gave an equation expressed in terms of amplitude and declination. From the above relationships it follows, leaving the air out of account, as has usually been the case, that the heat received by the Pole on a summer day is greater than that which falls on a point at the equator. Thus taking as unit the heat received during twenty-four hours by a place at which the sun is in the zenith, the North Pole receives an amount of heat represented by 0·397, and a point on the equator an amount represented by 0·292. But the air absorbs a large part of the sun's heat. The speaker considered it unreliable to estimate the height of the atmosphere from the amount of heat-absorption, as is frequently done, inasmuch as the chief absorption takes place in the deeper layers of the air. For the determination of the coefficient of absorption Dr. Zenker accepts the values obtained by Langley from his bolometric experiments, with a reservation, however, as regards the absorption which takes place in its highest layers, which he did not admit. One factor of great importance is the diffusion of heat, already described by Clausius, from the small particles of water, dust, and air in the atmosphere, which are calculated under other definite assumptions. Another factor which must not be lost sight of is the reflection of heat at the earth's surface; this is calculated for the three cases of a surface of water, land, and snow. Special tables are given of the heat reflected from these three kinds of earth-surface for separate places per day and per year. The application of this theoretical part of the research to the climatology of the earth's surface, the speaker intends to lay before the Society at some future time.—Dr. Less drew attention to the meteorological conditions of the past few days. A minimum temperature on March 1 was succeeded by a thaw on the evening of the 2nd, which was followed by a second very low temperature which again gave way to a thaw on the 6th. The rise and fall of the barometer corresponded to the above: the very considerable double variation in atmospheric pressure was caused by a minimum passing through South-West Sweden across the East Sea to Russia, which was succeeded by a partial minimum following the same course. Exactly similar meteorological conditions were in existence from February 4, and were caused by a minimum with its succeeding partial minimum following the same course as above. Such an exact similarity of path and action of two minima is of very rare occurrence, and deserves to be carefully studied; on both occasions, in February and March, very wintry weather was observed.—Dr. Hellman drew attention to the unusually heavy snow-fall of the past winter. As yet the maximum number of days on which snow falls in Berlin has been fifty, but this year up to the present time it has already fallen on fifty-eight days; in the same way, until this year never more than eight consecutive days of snow-fall have been observed, but this winter there has been one period of sixteen consecutive days on which snow has fallen.

Stockholm.

Royal Academy of Sciences, March 14.—Studies on the Characeæ and Violæ of the Isles of Gotland and Oeland, by Dr. Wahlstedt.—Studies on the geographical distribution of the plants in the province of Wermland, by Dr. Ringius.—On the currents of disjunction, by Dr. Mebius.—On the institution of pendulum observations in Sweden, by Prof. Rosén.—A review of the Orthoptera of Scandinavia, with descriptions, by Dr. Haij.—Analyses of gadolinite and hornilite, by Dr. W. Pettersson.—On the production of nitro-cymol and its products of oxidation, by Prof. Widman and Dr. Söderbaum.—On the occurrences of *Limnadia lenticularis* on the Isle of Nordkoster in the province of Bohus, by Hr. Hanson.—A thunderstorm combined with water-spouts near Upsala, by Hr. Th. Wigertz.—On fossil wood from Egypt and Eastern Asia, by Prof. Schenk, of Leipzig.—Volcanoes in the interior of the north-eastern parts of Iceland, by Hr. Thoroddsen, of Reykjavik.—On the determination of the constants in the diurnal rotation, by Dr. Bohlin.

Amsterdam.

Royal Academy of Sciences, February 25.—M. Martin exhibited a geological chart of the course of the River Surinam, appending the communication that, during his stay in the West Indies, he succeeded in discovering the geological formation in which the gold occurring in those parts, and long since known as wash-gold, was originally deposited. This formation is the crystalline schist, a stratum in which, in Brazil also, most of the gold is met with. The speaker urged that Brazil and Surinam offer striking points of resemblance both in the order and nature of their stratifications.—M. de Vries made a communication on his determination of the molecular weight of raffinose. His results, based upon physiological methods, tended to support the formula of Loiseau and Scheibler,

$$C_{18}H_{32}O_{16} + 5H_2O.$$

—M. Hubrecht described the early stages in the development of the blastodermic vesicle of the hedgehog. He claimed that the stages observed and described by him go a long way towards explaining the questionable points in the early stages of the human blastodermic vesicles that have yet been noticed.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Fundamental Principles of Chemistry: R. Galloway (Longmans).—Reminiscences of Foreign Travel: R. Crawford (Longmans).—An Examination of the Theory of Evolution: G. Gresswell (Williams and Norgate).—Johnston's Botany Plates, II. (Johnston).—Key to the Volapük Grammar: A. Kirchhoff (Sonnenschein).—Specimens of Papers set at the Army Preliminary Examinations, 1882–87 (Macmillan).—Companion to the Weekly Problem Papers: Rev. J. J. Milne (Macmillan).—An Indictment of Darwin: O. Dawson (Freethought Publishing Company).—An Increase in the Produce of the Soil through the Rational Use of Nitrogenous Manure: P. Wagner; translated by G. G. Henderson (Whittaker).—Smoke in Relation to Fogs in London: Hon. Rollo Russell.

CONTENTS.

THURSDAY, APRIL 12, 1888.

SOUTH KENSINGTON SCIENCE TEACHING.

WE are glad to notice that the attention of the House of Commons has at last been called to the deplorable condition of the accommodation allotted to the teaching of science at South Kensington. Our readers are aware that this subject is by no means a new one, as attention has frequently been called, not only in our columns, but in those of the leading daily political journals, to what Sir Henry Roscoe, not too strongly, termed "the disgraceful state of things." We believe, however, that Friday evening was the first occasion upon which the subject has been brought before the House of Commons.

When the House went into Committee on the Civil Service Estimates, the vote being for £9900 to complete the Science and Art Department buildings, Sir Henry Roscoe pointed out, in the first place, that the accommodation for the teaching of physics in this our only Government College for the training of science teachers, would in Germany be thought a disgrace in a third-rate town. The site of the makeshift laboratory, which, owing to the increase in the number of the students in this department, was arranged in a temporary building belonging to the French annexe, is now required for the Imperial Institute; and no substitute has yet been found, nor any suggestion offered, beyond that made by Mr. Plunket, that two of the official residences should be devoted to this purpose —a scheme which, we are not surprised to learn, did not meet with the approbation of those who know what is wanted, viz. the authorities of the Department. Mr. Mundella, as a former Vice-President, strongly indorsed the statement respecting the absolute necessity of steps being taken to place the Royal Normal School in a decent position, as far at least as its physical department is concerned. He pointed out the undesirability, to say the least, of removing the residences of the officials of the Museum from the ground, not so much on account of the immediate aid which the resident departmental heads would give in case of fire (though this we consider is important), as because their presence would insure the removal and proper care of the most valuable of the exhibits should such an accident happen. But, apart from these considerations, the idea of the Treasury suggesting that the only Government Science School in England should resort to such means for accommodating perhaps the most important of the experimental sciences is one which could only occur to the English official mind. After all, as Sir George Campbell said, "we are not a nation of paupers," and we may well demand decent accommodation for our National Science School.

The debate was not confined to this relatively small though not unimportant point. Sir H. Roscoe proceeded to explain that this opened the door to a much wider question, viz. that of the permanent housing and protection of the collection of scientific instruments and apparatus, of which he remarked that few persons were aware that we are possessed of one of the finest collections in Europe, containing not only a large number of the most delicate instruments used in physical research, but also apparatus of unique historic value. Such a collection as ours, if it existed in France or in Germany, would be appropriately housed in buildings worthy of its interest and importance; witness the industrial and scientific museums of Berlin and Vienna, or the still more palatial accommodation existing for similar collections in Paris. But in our metropolis these collections are housed in a temporary shed used by the various International Exhibitions, for which miserable accommodation the Government are actually paying a yearly rent of £2000. Reference was made during the debate to the existence of the inter-departmental Report on this subject moved for by Sir Henry Roscoe in June 1886. From this important document it is clear that the proposal to consolidate certain Government scientific institutions, to build a series of galleries on the land west of Exhibition Road, for the purpose of accommodating not only the science collections, but also the National Portrait Gallery and some other collections, met with the approval of all the members of the Committee, consisting of such men as Lord Lingen and Sir F. Bramwell, with the exception of Mr. Milford, at that time the Permanent Secretary of the Office of Works, whose opinion was apparently adverse to the possession of any national science collections at all As might be expected, no steps have, since the publication of this Report, been taken, beyond the removal of the National Portrait Gallery to Bethnal Green. Surely it is time that a state of things which would not be permitted to exist in any decently-sized town on the Continent should be amended. The buildings of the Imperial Institute are now raising their head on the site of the late International Exhibitions, and a road is being driven through from Queen's Gate on the west to Exhibition Road on the east. Plots of land, one directly south of the Imperial Institute buildings, and one north of the Natural History Museum, are now available, and can be purchased from the Commissioners of the 1851 Exhibition by the Government for a comparatively small sum. If this is not soon done, the Commissioners intend to sell their land to private individuals, to build a row of dwelling-houses fronting the road and looking on to the Imperial Institute. Will such a course of things be permitted? Is it possible that the Government, after the report from the ablest men of science and statesmen of the time, should allow this opportunity to pass? We must not; and we have good hopes that the promise of the First Lord of the Treasury, that this question will receive the attention of the Government, will not turn out to be an empty form, and that a statement will be made by the Government on this matter without unreasonable delay.

The debate was enlivened by a passage of arms between Lord Randolph Churchill and Mr. Mundella. The former, in his character of an economic reformer, repudiating what he called the excessive expenditure on buildings, told the House that it had not the remotest idea of the hundreds of thousands of pounds spent by the country in the payment of Professors' salaries, and other forms of encouraging science and art. It is a pity, for the sake of the "Professors," that the return for which Lord Randolph asked is confined to expenditure on bricks and mortar, otherwise he might have learnt how

far his statement of the existence of these luxurious professorships is borne out by fact. Still, we do not wish to quarrel with Lord Randolph's economic mood. We are not concerned to defend every expenditure on buildings or art collections in South Kensington or elsewhere ; and it is quite possible that, if these matters are looked into, an extravagance in this direction may be proved. In that case, in Lord Randolph's words, "the hon. member for South Manchester may have more to spend than he has at present," or, to express this in non-Parliamentary phraseology, a larger proportion of the present grant may be devoted to the pressing and important requirements of science.

EXPERIMENTAL RESEARCHES ON HYDRAULIC CEMENTS.

Recherches Expérimentales sur la Constitution des Mortiers Hydrauliques. Par M. H. Le Chatelier, Ingénieur des Mines. (Paris: Vve. Chas. Dunod, 1887.)

THE large employment of concrete for the construction of harbour-works, for building houses, paving streets, and other purposes, has created a considerable demand of late years for hydraulic cements. Cement manufacture is one of the comparatively new industries which have taken root in Ireland. This treatise of M. Le Chatelier is so valuable an addition to our knowledge of the chemistry of a great and important manufacture, that a short abstract of its contents will be welcome to many of the readers of NATURE, especially as the work is of unusual scientific interest.

The chemical reactions which result in the baking and hardening of plaster of Paris, mortars, and hydraulic cements are treated under the following heads: (1) plaster of Paris; (2) barium silicates; (3) hydraulic mortars and cements.

The first scientific investigation of the baking and setting of plaster of Paris was made by Lavoisier, and the process is thus explained by him. There are two stages at which the water is removed from gypsum; three-fourths of the water of hydration are much more easily expelled by heat than the last fourth. When gypsum is dehydrated by heat it absorbs water again with avidity, and suddenly becomes a confused and hardened mass of crystals.

Berthier's observation that plaster of Paris ordinarily contains from 4 to 8 per cent. of water has been confirmed by Landrin. The baking and dehydrating of gypsum was investigated by M. Le Chatelier by observing the periods measured by a chronograph, during which a thermometer marked successive increments of 5° of temperature when plunged into powdered gypsum heated progressively and regularly in a test-tube standing in a bath of paraffin. The longest periods correspond with the greatest heat absorption and dehydrating effect. From 130° to 140° C. the period was 20 minutes 40 seconds, between 165° and 180° C. it was 5 minutes. Dehydration is partial at 155°, but complete at 194° C. There are two distinct phases of dehydration of the compound $CaSO_4 \cdot 2H_2O$: the first corresponds to the formation of $(CaSO_4)_2 \cdot 3H_2O$, the second with $(CaSO_4)_2 \cdot H_2O$; this is plaster of Paris. The hydration which causes the quick setting of plaster can be represented by the equation: $(CaSO_4)_2 \cdot H_2O + 3H_2O = 2(CaSO_4 \cdot 2H_2O)$.

Cause of the Hardening.—It has been shown that a solution of hydrated calcium sulphate dissolves dehydrated plaster, and this after a short interval separates out as crystals of gypsum. This action explains the setting of plaster; water hydrates the compound partially, and dissolves the hydrate to saturation; this dissolves anhydrous sulphate to supersaturation, and deposits it as a hydrate, after which more of the anhydrous salt becomes dissolved. These two opposite actions take place simultaneously at contiguous points. The more rapid the hydration, the greater is the degree of supersaturation, and the quicker the setting of the plaster. Many anhydrous salts harden when in contact with water, as for instance sodium sulphate, but in every case there is a previous formation of a supersaturated solution.

It is established that crystallization which accompanies the setting of plaster of various kinds results from the difference in solubility of the compounds which set, and those which are formed during the setting: the first occur in a state of unstable equilibrium in presence of water, and can have only a transitory existence.

The crystals which form during setting are frequently, if not always, extremely delicate prisms united by one of their ends round central nuclei so as to form little spherical groups.

The mechanics of setting and hardening can be referred to crystallization. Starting with the idea that the hardening of mortars is not an isolated phenomenon without analogy, and that it is certainly similar to, if not identical with, one or other of the known methods, M. Le Chatelier describes these as follows:—Hardening by *compression*, of powders; by *desiccation*, as with clay or gelatine; by fusion and *solidification*, metals; by *crystallization*, soluble salts.

These can be referred to two simpler and more general phenomena:—

Mutual adherence of solid particles, produced at a minute distance from each other.

Mobility of the particles, which admits of their coming together. The momentary solution of a salt which sets affords the necessary mobility of the particles. The setting of mortar evidently enters into the category of phenomena of hardening by solution and crystallization. When the solid particles have once come together the specific hardness will depend upon the internal *cohesion* of the crystals and their mutual *adhesion*.

The *cohesion* of substances varies within very wide limits, of which the extreme terms amongst substances which enter into the composition of cements are: *plaster*, which is soft enough to be scratched by the nail, and *quartz*, hard enough to scratch steel. All we know about cohesion is that it is a primordial property of matter.

Adhesion, unlike cohesion, is a very complex and consequently a very variable phenomenon. Its variations can almost exclusively serve to explain the considerable differences in resistance which are often a distinguishing property in analogous cements. It varies with the chemical nature of the bodies in contact. The adhesion of a crystal of calcium sulphate to a glass plate is *nil*; on the contrary, it is so great with barium silicate that the

crystals break rather than become detached. It also varies with physical conditions, as, for instance, the more or less polished condition of surfaces in contact. The total adhesion is evidently proportional to the extent of surfaces in contact: it will be so much the greater as (1) the volume of empty spaces due to excess of water employed in mixing is less; (2) as each crystal for a given weight of matter presents a great extent of surface (the form of elongated prisms recognized in the crystallization of plaster and of all similar products is eminently favourable to the development of adhesion); (3) as the crystals are grouped so as to increase the volume of empty spaces and so as to diminish their number and isolate them one from the other. A structure like that of pumice is particularly favourable to strength. The nature of the solvent, temperature, and nuclei of crystallization, all serve to modify considerably the growth of crystals, and consequently to influence in a like degree the strength of the mortar. A study of barium silicate has led to the conclusion that its hydration may take place in a manner denoted by the following equations:—

$$BaSiO_3 + 6H_2O = BaSiO_3 \cdot 6H_2O$$
$$Ba_2SiO_4 + 15H_2O = Ba_2SiO_4 \cdot 6H_2O + BaO \cdot 9H_2O$$
$$SiO_2 + BaO \cdot 9H_2O = BaSiO_3 \cdot 6H_2O + 3H_2O.$$

The setting of siliceous baryta cements is due to the production of the same hydrated silicate, $BaSiO_3 \cdot 6H_2O$, in whatever manner it may be formed.

Mortars and Hydraulic Cements.—Calcareous mortars are divided into two classes: air-dried mortars; hydraulic mortars and cements.

Air-dried mortar is made from quick-lime slaked with water and mixed with sand. As Vicat has shown, the first stage of its setting is caused by the desiccation of extremely fine particles of lime, and is identical with the hardening of clay. The sand acts as in the making of bricks—it prevents too much shrinkage by forming an incompressible base or body. A further degree of hardening is caused by the conversion of the lime into carbonate.

The burning of limestone, unlike the dehydration of gypsum, is the result of the phenomenon of dissociation, as was proved by Debray. Various kinds of lime all become burnt at 850° C.

Hydraulic Cements.—These are obtained by baking natural or artificial mixtures of lime and clay, containing from 21 to 27 per cent. of clay.

They are divided into *slow-setting* and *quick-setting* cements. The former are baked at a much higher temperature than the latter. The setting of the former proceeds for some hours, and much facilitates their use.

There appear to be three different anhydrous calcium silicates, of which one only, the tricalcic silicate, $SiO_2 \cdot 3CaO$, is attacked by water, and is capable of setting; there are three calcium aluminates, which all set very quickly after mixing with water; there are ferrites of lime, which slake and swell out like quick-lime, and numerous other compound silicates which are more or less unalterable by water.

The only hydrated salts which can exist in presence of an excess of lime, and which are formed from the above, are:—

Silicate, $SiO_2 \cdot CaO \cdot 5H_2O$.
Aluminate, $Al_2O_3 \cdot 4CaO \cdot 12H_2O$.
Ferrite, $Fe_2O_3 \cdot 4CaO \cdot Aq$.

A microchemical study of anhydrous cements has shown that there are colourless crystals of a pseudo-cubic or hexagonal appearance. The intervals between these crystals are filled with coloured matter without crystalline character, which has been in a state of fusion. The crystals are formed by chemical precipitation in the midst of the brown fusible matter which afterwards solidified on cooling. The composition of the crystals is that of a calcium silicate, and of the amorphous portion of silicates of alumina, iron, and lime; the first compound alone is alterable by the action of water, and is that which plays so important a part in the setting and hardening of cements.

Analyses of four different varieties of cement support the view that this substance is essentially a tricalcic silicate.

There is no free lime in Portland cements of good quality, though there may be aluminates and ferrites.

Drawings taken from microscopic thin sections serve to illustrate the appearance of hydraulic cement when anhydrous and when undergoing hydration.

Lastly, the author deals with the causes of the destruction of hydraulic mortars in the air, in fresh and in salt water.

W. N. HARTLEY.

ELEMENTARY MICROSCOPICAL EXAMINATION.

Elementary Microscopical Examination. By T. Charters White, M.R.C.S., late President of the Quekett Microscopical Club. 104 pp. (London: Roper and Drowley.)

THE author of this work tells us that he has aimed at leading "the possessor of his first microscope into the smooth path of progress, by pointing out the simplest and most elementary methods of observation, and, after so far clearing the way, leading him gradually to the higher branches of microscopical manipulation." It must be admitted that he has succeeded in doing this. His modest little volume is both sound and original, and confirms the conviction that popular treatises, to be of good effect, must be produced by those who have themselves endured the drudgery of routine work and who have acquired their experience first hand. One sees throughout this work traces of a generation which is past, but as the book is not written for the schoolmen of to-day, criticism is, from their stand-point, disarmed.

The little volume is, notwithstanding, one of considerable merit. There may be cited, as bearing testimony to the care with which the author has selected his recipes, the incorporation of the glycerine-alcohol method of preparing delicate tissues, originally introduced by Strasburger. As evidence of originality, we may cite the following: "It (glycerine) needs discrimination in its use, as it cannot be employed for calcareous tissues as bone or shell, as they would become decalcified after being exposed to its influence for some time." Hints such as these, which are the very salvation of the *dilettante*, can only be the outcome of prolonged practical experience, and they testify most powerfully to the intrinsic merit of the work in which they appear. In dealing with photo-micrography the author describes an apparatus designed by himself. He is evidently an expert in this field, and in his device he has aimed at producing a machine which may be constructed by the worker at a minimum cost. A very worthy motive this, but experi-

ence can alone show how far he has succeeded. We should doubt the efficacy of his instrument ourselves, and we certainly cannot indorse his belief that, "however scrupulous the draughtsman may be, however unbiassed he intends to be, errors may creep in, and therefore photo-micrography . . . . comes in to insure complete veracity with a saving of labour."

Woodcuts are given of some few of the accessories enumerated. Chief among them is a very monotonous array of scalpels and probes, which form the frontispiece. Strange to say, the author makes not the least mention of most of those in his text, despite a reference in the index. It is clear, however, from the context, that they are to be regarded as aids to the study of insect anatomy: we have here a superfluity, for choice among the knives represented would be so embarrassing that, by the time the operator made up his mind, the subject itself would be far advanced towards decomposition. The introduction of curved scissors is no less to be deprecated. Apparatus and accessories have a fascination for most people, but the best work has always been done with the simplest tools. It must never be forgotten that it is the head at the one end, and not the mechanical aid at the other, which does the real work.

We would take exception to the introduction of the words "mountant," "semi-hard," and one or two others which might be named. The statement that the mites are "almost the smallest class of created beings" falls very unhappily from a pure microscopist, while the definition given of the Foraminifera needs modification.

We are pleased to note that the author has been mindful of the charms of the tow-net—perhaps the most important instrument in the future of marine zoology. If Mr. White's work be divested of its bugs' heads, and other similar objects which are the mainstay of those for whom he writes, there remains a solid substratum which far excels in merit that of many more pretentious works on the subject.

OUR BOOK SHELF.

A Manual of the Geology of India. Part IV. Mineralogy. By F. R. Mallet. Published by order of the Indian Government. (London: Trübner and Co., 1887.)

WHILE the third volume of this work possessed a certain interest for the statesman and the capitalist, including as it did descriptions of the minerals of economic value, the present one will only claim the attention of scientific readers. It may be a matter of surprise that nearly all that is certainly known about the minerals of India should be capable of compression into less than two hundred pages. But, as the author points out, excavations for mining or other purposes have not, as a rule, been superintended by men possessing the knowledge requisite to enable them to record facts of scientific importance; further, there is no demand for non-economic minerals, and consequently no mercenary incentive to collect specimens.

In looking over the book we are at once struck with the meagre character of much of the information given. Numbers of questions occur which we should like to see settled, but which are unanswerable in the present state of our knowledge, or rather ignorance, of Indian mineralogy. But our author is certainly not to blame for this. He has made the most of the scanty materials at his command, and the result is a valuable contribution to mineralogical science, which will serve as a basis for a future work on the subject worthy of our Indian Empire. The classification adopted is that of Dana, as given in his "System of Mineralogy."

In the collection of materials for the book which we hope will grow out of this, English residents and educated natives might do science much service. The study of mineralogy was extensively pursued in England until displaced by the more attractive subject of stratigraphy, but as India presents such a vast field there is no reason why the subject should not become popular again. Workers in this department will find Mr. Mallet's book of the greatest service.

Through the Yang-tse Gorges. By A. J. Little, F.R.G.S. (London: Sampson Low, 1888.)

MR. LITTLE recently undertook a two months' journey from Shanghai, the metropolis of the Chinese coast, to Chung-King, the commercial metropolis of Western China. The present volume consists of the journal kept during his travels, and an admirable journal it is, full of the results of careful and minute observation, and written in a fresh, lively, and entertaining style. Few travellers, with the exception of "the ubiquitous missionary," have ascended to the highest navigable point of the Yang-tse, the only road of intercommunication between the eastern and western districts of the Chinese Empire. Most readers, therefore, will find in this book much that is new to them about the Chinese people and their country. There are many vivid descriptions of the varied scenery through which Mr. Little passed, and his notes on industries, social customs, and popular religious ideas are invariably interesting and suggestive. Upon the whole, he has no very exalted opinion of the intellectual and moral qualities of the Chinese, and he is not disposed to believe that the empire, under the influence of Western ideas, is about to enter upon a new and momentous stage of political and social development. Everywhere he found the bureaucracy intensely conservative, and bitterly prejudiced against foreigners. They are willing enough to adopt superior mechanical appliances, so far as implements of war are concerned; but in all other matters they prefer to move along the old lines, which, having been good enough for their forefathers, must, they think, be good enough for themselves.

Home Experiments in Science. By T. O'Conor Sloane, Ph.D. (London: Sampson Low, Marston, Searle, and Rivington, 1888.)

THE author of this work has produced a very readable and useful book for those who wish to employ their leisure hours in gaining knowledge and information about the elementary parts of the various branches of science. The volume consists of a collection of experiments that can be easily performed with home-made apparatus; good detailed instruction as to the necessary mechanical operations is given, together with ninety-seven woodcuts of the experiments and the apparatus employed. The branches of science included in these experiments are mechanics, general and molecular physics; the chapter on soap-bubbles contains some very interesting experiments about them; and the concluding chapter consists of hints to those who are about to begin scientific lecturing.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Prof. Rosenbusch's Work on Petrology.

OF the great value of Prof. Rosenbusch's work on petrology, so excellently reviewed by Dr. Hatch, to which your corre-

spondent "A. B." draws attention, there can be no doubt, so far as it is regarded as a storehouse of information; but whether the system of classification proposed therein will not tend to retard rather than to further progress is a question on which I am at present more than doubtful. The two points to which Dr. Hatch and your correspondent draw attention as inherent weaknesses—viz. the "dyke rocks," and the subdivision of the "effusive rocks" into palæo-volcanic and neo-volcanic—appear to me such serious defects, that to praise a system which largely rests on them is like complimenting a viaduct by saying that it is an excellent viaduct but two of its piers unfortunately have a bad foundation.

But dismissing this as a question too large for discussion in your columns, I will confine my remarks to some defects in detail, rather serious as they appear to me, which are exhibited by the classification as tabulated by your correspondent.

(1) A mere "linear" classification fails, I believe, to represent satisfactorily the relation of the igneous rocks, because it separates too widely rocks very closely related—such, for instance, as the Dacites and Rhyolites (Liparites), and their corresponding holocrystalline representatives. Hence I believe that the branching system such as I indicated in my Presidential Address to the Geological Society in 1885 is more logical and more in accordance with the facts of Nature.

(2) In regard to the above example, I fail to understand why Dacites should be included with Andesites and Liparites separated from Trachytes, or, if we speak of their holocrystalline representatives, why we should separate Granite from Syenite, while we include Tonalite with Diorite. It is true that Granites are common and Tonalites are rare, and possibly the latter always contain some hornblende; but until it is shown that a quartz-plagioclase-biotite rock does not exist, or that the substitution of hornblende for biotite is of primary importance, there does not seem any valid reason for suppressing the group.

(3) As the term Diabase has long had a recognized meaning, I fail to see any good reason for substituting it for Dolerite, to which, as generally understood, it stands in much the same relation as do many Serpentines to Peridotites. Neither can I admit the propriety of separating Gabbro from it.

(4) The wide separation of the Leucite and Nepheline rocks from the Basalts seems also to me to be of doubtful advantage.

(5) If the term Peridotite be used in the sense in which it has generally been employed (*e.g.* by Dr. Wadsworth in his excellent "Lithological Studies")—namely, to denote a rock in which silicates of magnesia and iron abound, with some also containing lime but with little alumina—it is surely not possible to regard Limburgite as its "effusive" equivalent. That rock seems to me to be more properly associated with the Picrites, not as defined by Dr. Wadsworth (*i.e.* olivine-augite rocks), but as equivalent to the Palæopicrites of some authors—viz. pyroxenic rocks, containing a fair amount of olivine, and some feldspar, which last, however, has a very variable proportion. The true position of these rocks appears to me to be as a connecting-link between the Peridotites and the Dolerites.

There are other points in the work to which I should like to call attention, but I am writing away from books, and should have refrained for a season had not "A. B.'s" letter seemed to me to call for a word of friendly protest. No one can be more deeply sensible than I am of the value in many respects of Prof. Rosenbusch's work, but until his classification rests on a firmer foundation it will not, I fear, be really helpful to students in leading them to clearer ideas on a complicated and difficult subject.

T. G. BONNEY.

The Delicacy of the Sense of Taste.

AT the Philadelphia meeting of the American Association, in 1884, we presented a paper upon the general subject of the "Sensitiveness of the Special Senses." We have since continued our investigations,[1] and have the honour to present at this time the results of some experiments upon the sense of taste.

The object of the experiments herein described was to find out what substances, or classes of substances, are most readily detected by the sense of taste, and the relative delicacy of this sense towards these substances. For the production of familiar typical effects upon the organs of sense the following substances were selected:—

I. (bitter) . . .	Quinine.
II. (sweet) . . .	Cane sugar.
III. (acid) . . .	Sulphuric acid.
IV. (alkaline) . . .	Sodium bicarbonate.
V. (saline) . . .	Common salt.

The attempt was made to include other substances in this list, but it was difficult to find any, not embraced in the five classes above mentioned, which would not betray their presence either by colour or odour. Indeed it is surprising, to one who has not given the subject attention, to what an extent we are accustomed to depend upon the aid of the sense of smell in the classification of tastes. The fact has been noticed by several authors that, if the nostrils are closed, the range of our taste becomes very much limited.

Our method of testing the delicacy of the sense of taste was to make solutions, of known strength, of the different substances; then, by successive dilutions, to make from these several series of weaker solutions; each one being of one-half the strength of that preceding it. All the bottles containing these substances, and several bottles of water, being placed side by side without regard to order, the person to be tested was requested to taste of each solution and place it in its proper class. In each series the lowest solution was so very dilute that it was deemed impossible to distinguish it from water. Unknown solutions were to be classed with water. As the tasters were found to be liable to mistake occasionally even the stronger solutions, an opportunity was given, at the close of the test, to correct such accidental errors. In carrying out these tests we found that the most trustworthy results were to be obtained by instructing each operator to pick out the stronger solutions, temporarily classifying with water all which were not immediately recognized; and then to go over the latter solutions a second time, properly classifying such as could further be detected.

These tests were made by 128 persons, between the ages of twelve and fifty—eighty-two men and forty-six women. The average results are given in the following table:—

Table of Averages.

I. *Quinine.*

Male observers detected 1 part in 390,000 parts of water.
Female ,, ,, 1 ,, 456,000 ,, ,,

II. *Cane sugar.*

Male observers detected 1 part in 199 parts of water.
Female ,, ,, 1 ,, 204 ,, ,,

III. *Sulphuric acid.*

Male observers detected 1 part in 2080 parts of water.
Female ,, ,, 1 ,, 3280 ,, ,,

IV. *Bicarbonate of soda.*

Male observers detected 1 part in 98 parts of water.
Female ,, ,, 1 ,, 126 ,, ,,

V. *Common salt.*

Male observers detected 1 part in 2240 parts of water.
Female ,, ,, 1 ,, 1980 ,, ,,

This table indicates only the *average* delicacy of taste for each substance included in our experiments. The tests brought to light many astonishing individual peculiarities. For instance, there were persons who could detect with certainty 1 part of quinine in 5,120,000, while others failed to notice 1 part in 160,000. How far this difference is due to education it is not possible to say. Among the tasters were quite a large number who had been accustomed for several years to the handling and recognition of drugs and chemicals. Their record was considerably above the general average, but they were, on the other hand, surpassed by a few individuals who had had no previous training.

The results of our experiments may be briefly summed up as follows:—

1. The sense of taste is much more delicate for bitter substances than for the others included in our list. (The relative delicacy for quinine and sugar is very nearly 2000 : 1.)

2. Taken in the order of their effect upon the organs of taste,

[1] See the following papers:—"On the Relative Bitterness of different Bitter Substances" (Proceedings of the Kansas Academy of Sciences, 1885); "On the Relative Sweetness of different Sugars" (Reports of the Kansas Board of Agriculture. 1885); "On the Sensitiveness of the Eye for Colours of a Low Degree of Saturation" (*American Journal of Science*, III. vol. xxx. p. 27); "The Sense of Smell" (NATURE, vol. xxxv. p. 74).

the classes of substances used stand as follows: (1) bitters; (2) acids; (3) saline substances; (4) sweets; (5) alkalies.

3. The sense of taste is as a rule more delicate in women than in men (in the case of all substances tried excepting salt). The number of persons experimented upon was hardly sufficient, considering the very striking individual peculiarities met with in the course of our investigation, to permit us to lay great stress upon the relative averages obtained for the two sexes. We are not inclined, however, to regard the higher degree of sensitiveness shown in the averages for female observers as accidental, and our confidence in the approximate value of the results is strengthened by the fact that in the two portions into which our data naturally divided themselves, about half the tests having been made at a different time and under different circumstances from the remainder, the averages for each set agreed very well, not only as to the relative sensitiveness to the various substances employed, but also to the relatively higher degree of delicacy exhibited by women.

4. The ability to detect a dilute bitter is very generally accompanied by inability to detect a dilute sweet, and *vice versâ*.

5. The long-continued habitual use of a substance does not seem to influence in any marked way the delicacy of the sense of taste for that substance. Our tests with quinine afforded an excellent opportunity for the investigation of this point, as some of the persons experimented upon had made long continued use of that drug as a medicine. The results obtained were entirely of a negative character. We could discover neither increase nor decrease in the ability to taste the drug on the part of those habituated to its use.

Several other questions have been raised but not answered by our experiments. How many, for instance, of these substances, each being diluted in proportion to its effect upon the organs of taste, can be detected if mixed together? If so mixed, in what order will they be recognized, and will it always be in the same order by different persons? Again, what is the influence of the temperature of the solution upon our ability to taste its ingredients?

We deplore the fact that the above tests have not been extended to a much larger number of persons, but a careful study of the results of the limited number of experiments made leads us to believe that they do not differ widely from the probable averages of a much more extended series.

E. H. S. BAILEY.
E. L. NICHOLS.

University of Kansas, July 1887.

The Salt Industry in the United States.

HAVING occasion not long since to look up the statistics of the salt industry, I naturally turned to the latest edition of the "Encyclopædia Brittanica" (vol. xxi.), where the following statement met my eye:—"The deposits of salt in the United States are unimportant. The country possesses no really considerable salt industry, but is supplied so far as interior consumption is concerned to a small extent by brine springs."

As this did not at all correspond with the knowledge I had gained by a somewhat casual glance over the field, I took pains to look up the subject more thoroughly, and find the above statement so radically wrong that I venture to call your attention to it; though this I should scarcely have troubled myself to do had it appeared in any publication of less acknowledged authority. To be sure, we have no means of knowing just how great an industry must be to be classed as "really considerable," but by comparing the annual product of the United States with that of other countries we may claim, at least, an attempt at an approximation.

But first as to the character of the beds in the United States. It is true there are as yet known no beds comparable in depth and extent with those of Barcelona or Galicia, but nevertheless they are amply sufficient to supply all demands for ages. As long ago as 1869, Dr. Sterry Hunt published, in the Reports of the Geological Survey of Canada, results of borings at Goderich, Canada, in which, in a total depth of 1382 feet, six successive beds of salt were passed through, varying in thickness from 6 to 34 feet, and aggregating a total thickness of 126 feet. What area is covered by these deposits is yet to be ascertained; but they are known to extend over Central and Western New York, Northern Pennsylvania, North-Western Ohio, and Southern Ontario. At Warsaw, in New York State, one of the beds has a thickness of 80 feet. The extent of the deposit at Petite Ainse, Louisiana, has also yet to be determined, but a vertical shaft 165 feet in depth lies all the way in solid salt, and does not penetrate it. The above, although but two out of many, I mention since they have been known for years, and it would seem Mr. Lyte could have informed himself regarding them had he so attempted. Concerning the many extensive beds in the region of the Great Basin, ignorance is more excusable. Statistics showing the annual output of both rock and sea salt will best show the extent of the industry. I give below statistics for 1883, 1884, and 1885, taken from "Mineral Resources of the United States," p. 474. One barrel equals 280 lbs.

	1883. Barrels.	1884. Barrels.	1885. Barrels.
Michigan	2,894,672	3,161,806	3,297,403
New York	1,619,486	1,788,434	3,304,787
Ohio	350,000	320,000	306,847
West Virginia	320,000	310,000	223,184
Louisiana	265,215	223,964	229,271
California	214,286	178,571	221,428
Utah	107,143	114,285	107,140
Nevada	21,429	17,857	28,593
All other States and Territories	400,000	400,000	250,000
Totals	6,192,231	6,514,937	7,038,653

Complete statistics for all countries are not available, and I have to rely to a considerable extent on Encyclopædias, whose accuracy I now have reason to question. They are as follow:—

England (1881)	1,854,000 tons.
France	300,000 ,,
Spain	300,000 ,,
Portugal	250,000 ,,
Italy	165,000 ,,
Austria	400,000 ,,

	1881.	1882.
United States	834,548 tons.	897,732 tons.

In regard to the above figures, I confess to feeling sceptical save with reference to those of the United States and England. Nevertheless, granting that they do not give the full amounts by one-half, even then the United States stands second in the list of salt-producing countries.

What, then, constitutes a really considerable industry?

GEORGE P. MERRILL.

U. S. National Museum, Washington, D.C., December 3, 1887.

Force, and Newton's Third Law.

THE point mentioned by "Nemo" in your issue of March 29 (p. 511) is undoubtedly one that troubles most students at some stage or other; but there is no room for discussion about it; the matter lies in a nutshell: *a body does not exert force upon itself*. Think, for instance, of a horse and cart. The horse pulls the cart, and the cart pulls back the horse equally; how, then, can the cart move? The only puzzle lies in the false implication that the cart's pull-back is exerted upon the *cart*. Directly it is perceived that there is only one force acting on the cart, viz. the pull of the horse, no difficulty is felt as to why it moves. The "action" of A is not exerted upon A, but upon B. The "reaction" of B is not exerted upon B, but upon A. The time-rate of change of momentum of each and every body is equal to the total force acting upon it.

OLIVER J. LODGE.

Grasmere, March 31.

The New Photographic Objective.

THE letter of Sir Howard Grubb in your issue of March 8 (p. 439) appears to make some further explanation desirable on my part. The invention of the new form of photographic objective seems to have been made about the same time in America and in England. An experimental lens of this kind was constructed by the Messrs. Clark, after consultation with me, in May 1887. The 13-inch lens which they subsequently made upon the same plan was completed on July 8 of that year. My absence during the summer in Colorado, with the intention of selecting a place for the new instrument upon some mountain of considerable height, caused me to overlook the account of the English invention in the *Observatory*. Since my return, the telescope has been mounted in Cambridge, on the grounds of

this institution, where it is found to give highly satisfactory results. Photographs of η Orionis which have been made with it exhibit the elongation of the star, although the distance between its components is only about 1″. The newspaper report to which Sir Howard Grubb refers, that a patent was granted for the invention, is without foundation. The Messrs. Clark have never patented any of the improvements made by them in optics, and have had no intention of deviating from their usual practice in this instance.

EDWARD C. PICKERING.

Harvard College Observatory, Cambridge, U.S., March 26.

Life of Fleeming Jenkin.

I HAVE read with singular pain a paragraph in your notice (signed with the initials of one whom I admire and respect) of my Life of Fleeming Jenkin. To accuse a man of falsehood in private life is a strong step. But I must explain to your reviewer, I might lie to him all day long and not be so disgraced as if I put one single falsehood in a book. For the making of books is my trade by which I live; I supply them on honour, and the public gives me bread for them in confidence. Your reviewer will perhaps more readily understand what he has done (I am sure in ignorance) if I supply him with a parallel. To say that a man of science was a liar would be highly disagreeable; but if I were to say he had falsified an experiment, and to say so publicly in print, I should be curious to see the expression of his face.

I dwell upon this because it is plain your reviewer scarcely understands what literature is, and I fear others may be equally at sea. On the merely personal matter, that I am supposed to tell a deliberate falsehood on my own authority and about my dead friend, I will make but one remark. Hasty reading is the fit precursor of hasty writing; in no word have I indicated that the certificate in question was "worthy the name"; and the terms of the document are at the reviewer's service to-morrow, if he be curious.

ROBERT LOUIS STEVENSON.

March 28.

THE HITTITES, WITH SPECIAL REFERENCE TO VERY RECENT DISCOVERIES.[1]

III.

THE conclusion has been already expressed that the Hittite inscription of the Tarkutimme seal is, in the main, ideographic, and that the phonetic element is supplementary; that, in fact, regarding the figure of the king as part of the inscription, the sense is fully given without taking into account the phonetic element. Some scholars and investigators have, however, taken a different view. This fact, together with the alleged resemblance of some of the Hittite hieroglyphs to characters of the Cypriote syllabary, has had much influence on certain recent attempts at deciphering the Hittite inscriptions. With regard to the alleged analogy of the Hittite and Cypriote characters, it may be allowed that the derivation of the latter from the former is in itself by no means impossible. As yet, however, the evidence of such derivation which has been presented is certainly inadequate: to a great extent it is little better than visionary. Moreover, if, from closeness of resemblance or otherwise, satisfactory proof of the derivation had been given, it would by no means necessarily follow that, when all or any of the Hittite inscriptions which we possess were sculptured, the Hittite writing had become already so far developed that the hieroglyphs generally, or in great proportion, had acquired distinct syllabic values. As to how far resemblances between the Hittite and Cypriote characters give evidence of essential connection or derivation, the reader may perhaps satisfy himself by inspecting the list given by Dr. Isaac Taylor ("The Alphabet," 1883) and reproduced by Prof. Sayce in Wright's "Empire of the Hittites," 1886, chap. xi. More extended lists have been given by Captain Conder (who follows to a considerable extent in the track of Prof. Sayce) in the plates of his "Altaic Hieroglyphs." But, as it seems to me, in neither case have the Hittite characters been always given with such essential accuracy as is desirable. This remark applies more especially to some of Captain Conder's figures, notwithstanding his observation in "Altaic Hieroglyphs," p. 35: "A careless reading and confusion of distinct emblems must lead us wrong; and for this reason exact copies are indispensable." But, even if this objection be waived, the evidence must still be regarded as inadequate. As to "the subject of the inscriptions," Captain Conder remarks that it "is exactly what we should have expected. They occur on statues of the gods, and they are invocations only" (*op. cit.* p. 149). Now that the inscriptions "occur on statues of the gods" is certainly not true with regard to most of those which are known to us, and as we have them. The "doorway inscription" in the British Museum and the inscriptions from Hamath are connected with no statue whatever. In other cases, where there is a statue, or large figure in relief, it is by no means to be assumed that the figure is always that of a deity. But, as a specimen of what Captain Conder finds in the Hittite inscriptions, I may give his "free rendering" of the first two lines of that very ancient inscription in the British Museum of which I have just spoken as the "doorway inscription." I give Captain Conder's "free rendering" rather than his "verbatim translation," as likely to convey a less unfavourable impression:—

"1. Prayers of the Monument of Set. Powerful words for the living fire, the Most High . . . the divine. . . .

"2. . . . to . . . (pour?) Tammuz, Āa, living fire, Most High descending (propitious?) Thee strong Set . . ." ("Altaic Hieroglyphs," p. 194).

With respect to utterances of this kind it is not necessary to say much more than that they certainly have not the claim to consideration which would result from a connected and congrous rendering. Such a rendering might have been adduced as giving some answer to the position that evidence is wanting as to the Hittite hieroglyphs representing, in the main, syllables either in the Accadian or Altaic language, or in any other language whatever. Then, as to the inscriptions being concerned mainly or exclusively with theological prayers and invocations, the analogy of the Assyrian inscriptions—which the Tarkutimme seal with its cuneiform legend itself suggests—would rather lead us to expect that the subject-matter of the inscriptions is usually success in war, with allusions to the gods, and prayers and thanksgivings, chiefly in relation to such success. And this more realistic view is in accordance with the heads of oxen and of asses, with the clubs and the swords, and other symbols of equally materialistic character which appear on the inscriptions. Moreover, somewhat more than a year ago, the British Museum fortunately obtained an engraved stone of unquestionable antiquity, giving evidence in accordance with that of the seal of Tarkutimme, and tending to show that the Hittite inscriptions are in the main ideographic or pictorial.

The allusion just made has reference to a circular hæmatite seal from Yuzgât, in Asia Minor, which was added to the antiquarian treasures of the Museum in October 1886. Yuzgât is not very far from both Boghaz-Keui and Eyuk; therefore the discovery in this locality of an important Hittite antiquity cán scarcely excite surprise. For the present, the seal is named, most conveniently, from the place where it was found, "the Yuzgât seal." This seal resembles the seal of Tarkutimme in being circular; and the two seals agree also with reference to there being an inner circle which divides the figures or characters round the circumference from those in the central space. The seals differ, however, in size, the Yuzgât seal being much the smaller. The latter seal, moreover, is not bilin-

[1] Based on Lectures delivered by Mr. Thomas Tyler at the British Museum in January 1888. Continued from p. 540.

gual, and it has not a convexity of surface, like the seal of Tarkutimme. The Yuzgât seal, in fact, is flat, with the exception of the central space, which is concave, and which consequently causes a central convexity in the impression. On careful observation it may be perceived that the figures on the circumference divide themselves into three groups. In the centre of the first group is the winged solar disk supported on a cone. It seems not unlikely that this cone is essentially identical with the "king"-symbol already discussed in connection with the Tarkutimme seal. Here it may point to the prominence and pre-eminence of the sun-god as ruler of the world, all things animate and inanimate being subjected to his sway. The solar king in the centre, with the two figures, one half-kneeling and one standing, on each side, constitute the first group. These two figures on each side present features of very great interest. Nearest to the solar emblem are two horned ox-headed figures, apparently masculine, with the palms of the hands uplifted, in the act of adoration. The ox's head is not here presented in profile, as is usually the case on the Hittite monuments, but the horns and ears and the tapering muzzle are depicted with sufficient clearness. These figures may be taken as representing the moon-god, and recalling in

Fig. G.—The Yuzgât seal in the British Museum (enlarged).

their masculinity the Babylonian moon-god Sin. This seeming masculinity should be remembered if a comparison is made with other ox- or cow-headed figures of deities, as, for example, those found by Dr. Schliemann, and about which there was not very long ago some discussion. More distant from the solar emblem are two draped figures which we may regard as types of the female deity Ashtoreth, viewed as a moon-goddess. There is tolerably clear evidence that one of these draped figures is horned, and probably also ox-headed. In the case of the other, on account of a flaw in the seal, this is not equally manifest. Though the attitude is different, these female lunar deities appear also to be worshipping the sun-god. The lunar deities, like other figures on the seal, appear all to have turned-up toes, indicating probably the so-called "Hittite boots."[1] With these figures of lunar deities may be compared a symbol of Ashtoreth as a moon-goddess on the longest Hamath inscription, giving a crescent moon with the head of an ox above and within it, while beneath is an equilateral triangle or else a cone.[1]

If we strike a diameter across the seal from the solar disk, it will come, towards its extremity, to what is apparently a king seated on his throne and wearing a cap with a horn in front. Between the king and the group of sacred figures already described, there is on each side a distinct group, making up altogether the three groups which I have mentioned. Of the two groups not yet described the more interesting, on account of its resemblance to what may be seen on the inscriptions, is the group behind the king. There appears strong reason to believe that in this group we have a genuine example of picture-writing, in which the successful chase of a stag is represented. There is first (most remote from the king) a tree, indicating the forest, where the hunt occurred. Then come two javelins, used no doubt by the hunters of the stag, and next after these there is a sort of trident, employed, I should suppose, to give the *coup de grâce*, and of this trident I shall have an additional word to say directly. Next to the trident we find a bundle, or basket with a handle, which naturally suggests the idea of carrying. Then there is a stag's head with large antlers, and beneath it two arms with hands pointing towards the king. As the king is sitting with his face towards the group of figures in front of him, the engraver, in order to denote the king's acceptance of the stag's head (which may represent the whole

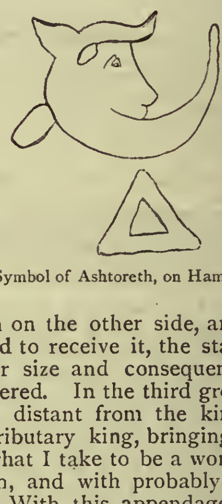


Fig. H.—Symbol of Ashtoreth, on Hamath inscription.

stag), has given on the other side, and above the king's arm outstretched to receive it, the stag's head a second time, of smaller size and consequently somewhat less artistically rendered. In the third group, beginning with the figure most distant from the king, we have what is very likely a tributary king, bringing a gift or tribute. Before him is what I take to be a woman veiled after the Oriental fashion, and with probably a baby suspended from her arm. With this appendage she may possibly have been regarded as likely to prove more acceptable to the king. Between the woman and the king is what I have regarded as a conventional symbol of a castle, indicating that the presents were received by the king in his castle. The symbol is difficult to determine; but I cannot find any more probable explanation. What it is particularly important to observe is, that the other two groups on the circumference of the seal being pictorial or ideographic, it is scarcely possible to escape the conclusion that the third group—that which I have regarded as representing the successful chase of a stag—is of the like character.

Of the objects in the central space I am unable to speak with any confidence. They may be so placed as objects of interest merely, or, taken phonetically, they may denote a name. There is a crescent, beneath it a nearly semicircular knife with a handle (if it is not possibly a ladle seen in profile), a mace or club, a sort of grating, and a trident smaller than that in the outer

[1] It is worthy of note here that about a third of the circumference is occupied by these sacred figures. From this fact may be derived a probable explanation of the vacant space over the king's head in the seal of Tarkutimme (*supra*, p. 537). The engraver, we may suppose, when he commenced engraving the king's name, intended to devote a third of the circumference to sacred objects, or at least to leave it vacant as usually so devoted. He, however, miscalculated the space at his disposal. Resolved, however, to leave some vacant space at the top, and especially over the king's head, he was compelled to leave a space in the middle of a word. Prof. Sayce has given a different explanation (*Zeitschr. für Assyriologie*, November 1886.)

[1] Mr. Rylands's drawing of the inscription gives the former, and this may possibly be right, though the cast of the inscription in the British Museum does not make this altogether clear. The original is unfortunately at Constantinople.

circle. Besides these objects, there is an equilateral triangle, like eleven others among the symbols in the outer circle. I was inclined to think that these triangles might perhaps in some way modify the meaning of the other symbols, till I noticed that not only does their size differ, but also that the vertex of the triangle, usually directed upwards, may be directed downwards to suit better the shape of the surrounding area. This is clearly seen in the space between the larger stag's head and the king. We cannot, however, come to the conclusion that these triangles are employed merely for artistic effect, and to fill up vacant spaces, even if these objects were not wholly disregarded. The recently-discovered Tarsus seal gives important evidence in favour of the sacredness of the equilateral triangle. We must conclude that the triangle is employed on the Yuzgât seal as a sacred symbol, and that as such its vertex is usually directed upward, but that this position is sometimes varied in accordance with the exigencies of space.

With regard to the group on the seal, concerned with the chase of a stag, I have spoken of its resemblance to what may be seen on the Hittite inscriptions. This is especially noteworthy with regard to the group represented in Fig. I, from the so-called doorway inscription in the British Museum. Progress in decipherment is not as yet sufficiently advanced to enable us to determine the precise significance of all the symbols, but of the general meaning there seems no room to doubt. Beginning from the end of the figure to the reader's right, the meaning

Fig. I.—Group of symbols from Jerablûs monument in the British Museum.

intended to be conveyed appears to be that booty in the shape of many oxen, asses, and other treasure, which had been obtained by the powerful assistance of the gods, was presented to the king. The parallelogram with a square on each side I regard as indicating "many." That this was the sign of plurality was the opinion of my distinguished friend, the late Dr. Birch. The head of the ox and of the ass do not seem to require remark; but above the latter is a massive and powerful right hand, with fingers clenched, and with part of the fore-arm. This would be a very appropriate symbol of strength or power.[1] Close above the right hand, and at the top, are a straight stroke, or parallelogram, with a crescent beside it. These symbols combine to form the usual symbol of deity on the Jerablûs monuments. I can only assert this now; but the evidence is abundant, and I hope to prove it fully in the sequel. Next after the closed fist with the symbol of deity comes part of an arm with the hand open and pointing towards the king. The analogy between this hand and those beneath the stag's head on the Yuzgât seal is almost too obvious to require remark. Of the value of the two crescents, which are, so to speak, back to back—a symbol not uncommon on other inscriptions—I cannot speak with any confidence.[2] At the bottom is a foot, which would very appropriately denote the act of going to the king. Next to the open hand at top is a symbol

[1] Cf. "The saving strength of his right hand" (Psalm xx. 6), and "his right hand, and his holy arm, hath gotten him the victory" (Psalm xcviii. 1). But the figurative use of the right hand as a symbol of strength presents no difficulty.

[2] It seems not unlikely, however, having regard to the symbols which the two crescents accompany here and elsewhere, that they distinguish a particular kind or class of persons.

the origin of which Mr. Rylands, to the best of my recollection, formerly referred to a bag grasped and pressed together a little below its mouth, by a hand. If this view is correct, this symbol has become, like many others, somewhat conventionalized. The bag is depicted so as to enable us to see within it at the bottom. Here are three objects, probably pieces of gold or silver used as uncoined money; and the number three may, as elsewhere, denote a great many. Beneath the bag is what has been regarded as a yoke; and, having regard to the bundle or basket on the Yuzgât seal, this may very well denote the carrying to the king. Last comes the head of the king himself, with conical cap and "pig-tail," and above him is a symbol which is perhaps best regarded as derived from the idea of a canopy above the king. As on the Yuzgât seal, the king's face is turned away, but this is because the inscription is intended to be read *with the faces*—that is, in the direction towards which the faces point—and not, as some have asserted, *against the faces.* The former arrangement is the more natural, and would have *a priori* the greater probability, but the latter is commonly, though, as Dr. Birch once said with reference to this point, not invariably, observed on the Egyptian monuments. In support of the latter view it is alleged that there is usually, at one end of the first line of the inscriptions, part of a figure with the face turned away from the other characters in the line, but with the fingers pointing towards the face or mouth, as though indicating "I have something to say." This figure, it is

Fig. K.—Figure from Jerablûs monument in the British Museum.

urged, must mark the beginning of the inscription, and, as the face is turned away, the characters must be read against the faces. But, in the first place, the figure referred to occurs elsewhere, and not solely and invariably at one end (to the reader's right) of the first line. The significance of the figure is, in all probability, as stated, but, as the figure is evidently that of a servant or minister, and not that of the king or other great personage with whose doings the inscription may be supposed to be concerned, we should expect in the first place, and before the particular message, or the subject-matter of the inscription, is entered upon, a statement of the name and titles of the person from whom the message proceeds. At the commencement of the Assyrian inscriptions there is often a very copious statement of this kind. And in fact on three of the Hamath inscriptions there is what appears to be a name immediately before the figure with the hand towards the mouth. The group of symbols discussed just above gives pretty strong evidence as to the direction in which the inscription in which they occur is to be read; and I hope to give some further evidence on this point in the sequel.

A word must be here added with respect to the trident on the Yuzgât seal. The trident is more usually associated with the sea and the sea-god than with warfare on land, or the chase. It was employed, indeed, in the Roman arena; but, as the gladiator using it was furnished also with a net, there may seem to be still some reminiscence of the sea. With regard to the trident being employed in the chase, I may adduce the evidence of a curious seal-impression which I obtained some time ago from Mr. Ready, of the British Museum. The objects

depicted are apparently the head of an animal, probably some kind of goat or ibex, parts of the animal's carcass, and a trident essentially similar to those on the Yuzgât seal. Mr. Ready is unable to tell me in what

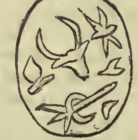


Fig. L—Seal with figures (enlarged).

collection this curious seal, which is very small, is to be found. So far as I am aware, it is not in the British Museum.

PRACTICAL EDUCATION.

PLAINLY speaking, it must be admitted that to an impartial observer the great problem of anthropology is this: Is the mind, or soul, a mysterious and supernatural, yet at the same time a definite limited *quantity*, with certain set "spiritual" functions, or is it, being of material growth, capable of infinite development? The former is the metaphysical view of the subject, the latter that of the evolutionary physiologist. Without deciding which is the true school, it may be remarked that the metaphysicians have long ceased to teach anything new, while physiology gives us, almost daily, facts of an astonishing nature. Here and there in the works of Darwin, Carpenter, Haeckel, Huxley, Bain, Maudsley, Spencer, and David Kay, we find what would have been "conclusions most forbidden," even to a Rosicrucian or Cabalist, in days of yore. And these are that man may develop his memory and other faculties in the simplest and most practical manner, as a bee builds its combs, grain by grain, until he shall far surpass what he has ever been. These discoveries as to man are in exact step with the stupendous revelations of the spectrum analysis, and the scientific reduction of the elements.

I recently published a work, the result of many years' labour, entitled "Practical Education," in which I endeavoured to give the results of experiments with nearly two thousand pupils, combined with the suggestions in the works of the writers above alluded to.[1] Having long been occupied with investigating the problem of technical education, I offered to the School Board of Philadelphia, in 1880, to devote myself entirely to the experiment of ascertaining *exactly what children could do*. That boys and girls from eight to fourteen years of age could *not* set type, make shoes, execute heavy carpenters' work, &c., had already been ascertained in Pennsylvania at a cost of about £200,000. I had, however, learned in Egypt, South Germany, and other places, that the very young can execute the decorative work which is known as that of the minor arts, and that so well that it had a market value.

Walter Smith, now of Bradford, had published a system by which design was taught at the same time with drawing. I had, two years before I met with Smith's system, which is now much employed in America, set forth the same idea in a work entitled "The Minor Arts." It soon became apparent that, by beginning with design, the youngest child developed—with invention—interest, attention, and intelligence. The results went far beyond my anticipation. It was found by the most careful inquiry that the pupils who attended the art classes had the highest "averages" in other studies, such as arithmetic, geography, and composition. This fact is the more striking from this—that the School Board, having made inquiries unknown to me, found that among 110,000 pupils the 200 who attended the Industrial Art School were among the first in *everything*.

An immediate inference from this fact is that visual perception or eye-memory (as set forth by Francis Galton) and attention or interest (as explained by Dr. Maudsley) are also factors which enter into the training of the constructive faculty. These, as is clearly explained and very fully illustrated by David Kay in his admirable work on "Memory," lead us to the conclusion that memory, by a simple process of accretion and repetition, may be developed to an incredible extent even in children. Practically, this was nothing new. Before the invention of printing, men by millions, among Druids and Brahmins and Northmen, Red Indians and mediæval scholars, Chinese and Japanese, had shown that an individual could remember perfectly what is now represented by a library. Max Müller has proved this. I myself have known a graduate of Pekin who fully illustrated it.

Memory is not "mind" or intelligence. Yet the works of Homer, the "Mahabharata," and the great scientific grammar of Pānini, were taken down and preserved for centuries by memory alone. The great history of Japan, by Hirata Atsune, was composed without the author's taking a note, and written from recollection, without reference to an original work. What man has done man may do. The deduction from all this is as follows:—

Firstly, that memory may be trained in mere children, by an easy process of committing by heart and constant reviewing, to such an extent that, guided by attention or determination, anything once read or seen may be accurately recalled. A great collection of illustrations of this may be found in Kay's "Memory," and in my own work on "Practical Education."

Secondly, that to counterbalance mere memory the mind must be trained by exercises in quickness of perception. These, in the beginning, may be merely mechanical. There are steps from inducing an infant to notice an orange on the floor up to simple games, from games to mental arithmetic or mental geography and grammar, to problems requiring the highest intelligence. The process is like that in developing memory—*little by little with constant reviewing*. And, as is the case with memory, all this has been established by innumerable practical examples. But with the one, as with the other, there should be no endeavour to cultivate thought or intellect or imagination until *both* are fairly mastered.

Thirdly, memory and quickness of perception blend and are developed in the awakening of the constructive faculty or in design, and its application to modelling, embroidery, wood-carving, and similar easy arts. And to those who object that all this does not awaken the higher faculty of intelligence or thought, it may be replied that experience or experiment have demonstrated the contrary. It is true beyond denial that a boy or girl who remembers readily and perceives quickly, and who has been trained to invention by designing, *does* think. Call them, if we will, only the tools of the great trade of thought, and a training to their use, is there no difference between two children of equal capacity, brought into a shop, when one knows what everything around is meant for, and how to handle it, when the other is yet to be taught? But the fact is beyond all dispute that children, even if trained to design alone, begin to think in every way. The experience of the Philadelphia school, and more or less that of every well-conducted Kindergarten, prove it. The trouble is, according to the requirements of a late review, that people ask for genius at once from an infant. "Teaching children to remember is not training them to think." But it is the foundation-stone.

[1] "Practical Education" (London: Whittaker and Co., Paternoster Square).

It is the giving them the faculty to collect material to employ thought. Quickness of perception is the next stage of the building. It awakens a sense of the relations which things remembered bear to one another. But the most illiterate man would not deny that a boy with a good memory, who is "sharp to notice everything," is not far off from being clever. Does not this, indeed, constitute about all the cleverness which practical life requires? But it is most unfair that any man, who has not examined the evidence, or read the facts which have been accumulated to show that extraordinary quickness of perception of every kind can be induced by proper training, should at once declare it to be impossible. It is a question not for metaphysical *a priori* assumption, but for scientific research, experiment, and test.

To render what I have said clearer, I would add that, if we begin by memorizing *mere words*, and nothing else, without any special effort to attach meaning to them, or only just so much as will aid in the work, the pupil will, in a short time, acquire a mechanical faculty for remembering. As soon as this becomes habitual, easy lessons which, so to speak, explain themselves, are introduced, and so, step by step, with great care the learner is led to acquire that which involves intelligence. Now, the whole system lies in this: that what a boy or girl perfectly remembers is easier to understand than when it is only half grasped. As it is, we begin in teaching a language by requiring a child to learn all at once to remember words, to pronounce them, and to master their grammatical structure and relations. I never knew of but one instance in my life in which anybody over twenty-five years of age ever learned to speak French like a native. This was a lady, who, before learning the meaning of a word, passed several months in mastering the pronunciation. Schliemann, the excavator of Hissarlik, who for many years learned a language every six months, advocates this system. By learning one thing at a time, at first, we are far better able to acquire several things at once in a more advanced stage. In acquiring quickness of perception, as in memorizing, the processes are identical—they begin by the simplest mechanical methods, and advance to the most refined.

The same development in a commensurate manner is observed in teaching industrial art. To give a child, or even a dull adult, some idea of design, I would allow him or her to group cardboard leaves into a pattern, and trace round them with a pencil till the fingers became familiar with the implement. There are not many cases in which this is advisable, but, having tried it many times, I can assure those who have not that it does not in the least degree prevent beginners from acquiring the boldest freehand practice. The more pains we take with the rudiments of every kind of culture, the easier is the acquisition of advanced branches.

The age is now being called on to face a great problem. It is that of over-pressure. From every side we hear in every newspaper of a thousand things which everybody is assumed to know. A certain great thinker—or writer—was said to have tested in vain "the American mind," by asking everyone he met in the United States, "Have you read Obermann?" It was not true, but it was truthful because it might have been, and because it truly represents the current pedantry of requiring, as a proof of culture, a knowledge of every German, Swiss, or French introversial- transcendental- or sentimental-ist. It is as true of society as of the school. "Shall the meeting-house be moved away from the growing dung-hill, or the dung-hill from the meeting-house?" Such was the great problem which was discussed by a Yankee town council. Shall we go on increasing the branches of popular education, or reduce them? Why not try the experiment of ascertaining whether the pupil will not learn more by first acquiring the art of learning? That is the problem which we are all bound to discuss sooner or later. It cannot be evaded. It is forcing itself upon us from every side. A perusal of all the London reviews or magazines for a month is enough to make any polyhistor—if such a man exists—feel like an ignoramus. It is becoming a clear case of *non possumus*, as the Chicago Professor declared when he recognized the impossibility of shooting two 'possums with only one ball. Either the capacities must be increased, or the contents diminished. And that the powers of memory, perceptiveness, and construction can, by a very easy system of rudimentary culture, be developed to what would seem to be miraculous, is in accordance with the teachings of the most advanced men of science, and is established by innumerable facts. All that is needed now is to combine into a single system the truths which have hitherto been scattered, and to make that a subject of general education which has been illustrated only by separate examples.

It was seriously objected, when I for the first time undertook to make industrial art a regular branch of instruction in public schools, that the number of children who had any capacity or *gift* for such a study, or enough to make it advantageous, was so limited that it would not be worth while to try the experiment. The result of several years' teaching was that while among nearly two thousand pupils only one or two were found who had this "gift," there was not one single child who was not abundantly capable of learning decorative design, and mastering the minor arts. Precisely the same thing is being said as regards teaching memory and perception. "It will succeed with *geniuses*, but not with all." Now, it is an extraordinary thing, and one to be specially noted, that the antecedent proofs and probabilities that every child can become a clever artistic artisan were very few and far between compared to those which illustrate the truth that the other faculties in question may be as generally acquired. Secondly, it was urged against the one, as it is now being urged against the other, "Where will you find teachers?" They were speedily found in the art school, for we soon developed them from among our pupils, while I had in addition a class of grown-up ladies who were specially educated as instructors. But the great objection, and the one which to this day perplexes the majority of people, is, "What profit is there in teaching pattern drawing, modelling shoes or leaves, carving patterns or hammering brass? *Will it pay?* Can a boy make a living by it?" This is precisely the problem proposed by Sam Weller's school-boy, who had indeed learned the alphabet, but doubted whether it was worth while going through so much to learn so little. "Is it not better to teach *boys* a *trade?*" is heard on every side in answer to the assertion that boys and girls of tender age should be prepared to begin to study one. In exactly the same spirit a reviewer declares that "we shall do well to ask ourselves whether it is not more important to teach our children to *think* than to remember, and whether a great deal of the matter with which children are expected to load their memories is not lumber." This is quite equivalent to declaring that it is much more sensible to teach boys algebra than have them waste time in learning the numerals or simple arithmetic. If the writer in question had ever read even a little in physiology, he might have learned that it is estimated that there are from 600,000,000 to 1,200,000,000 of nerve-cells in the brain for the generation of nerve force, and the moulding and storing up of our ideas, each having a separate existence, while Prof. Bain gives the number of fibres which transmit impressions at about 5,000,000,000. Now, if any of the objectors to "overloading" the memory do so because they find they are themselves already perilously near to possessing one thousand two hundred million ideas, and really cannot hold any more, nothing remains to be said. Truly, it has been carefully calculated that for the most retentive and richly endowed minds there are only about 200,000

acquisitions of the assumed types, but the amount of genius which a reviewer must possess must far transcend this if he can prove that people should learn to think before they can remember anything.

Ten years ago the training of children to work while studying was deemed chimerical. "It had been tried," we were told, "and it had failed." But it had not been tried properly or sensibly. Ten years hence memory and quickness of perception will also be taught to classes of pupils as a preparation for thought. What man has been we all know, but what man may be no one can tell. This only is certain, that Science now holds in her hand, at last, the key to Nature, and that ere a decade shall pass there will be such revolutions as no supernaturalist ever dreamed of.

CHARLES G. LELAND.

TELEGRAPHS IN CHINA.

THE progress of China is by no means so rapid as some interested persons would have us believe, but beyond doubt the empire is at last moving in a direction favourable to the adoption of Western arts and sciences. The simple fact that telegraphs are being provided there is in itself evidence of the wonderful change which has taken place in the past few years in the attitude of the ruling body, and which not even the most sanguine among us could reasonably have anticipated, to go no farther back than the period of the Chefoo Convention in 1877.

When, however, we find it announced that a complete network, as it were, of telegraphic connections is in course of formation there, it may be worth our while to ascertain whether the foundation of this statement is sound and trustworthy; and in making an examination we shall find it convenient to refer to the substantial progress made and the elaborate system which exists, not merely upon paper, but in absolute perfection, no farther away from China than thirty-six hours' journey by steamer.

Japan may indeed lay claim to the possession of a network of telegraphs; and to obtain an idea of the work to be done in China before a similar claim can be established there, we need only reflect that taking mileage and population into consideration the whole of the Japanese Empire could conveniently be deposited within the boundaries of even one of the eighteen provinces of the Flowery Land. To arrive at a basis of calculation, therefore, we should have to multiply the total length of the existing Japanese telegraph lines at least ten times before any comparison could be instituted. If we were to contrast the East and West, which, however, would be scarcely fair, we should find that a telegraphic system as the term is understood in Europe means something yet immeasurably more extensive and intricate.

Casting aside, then, the extravagant impressions which are often conveyed by the brief telegraphic intelligence which reaches us periodically from the Far East, it is matter for congratulation that the outlying provinces of China are gradually being brought into communication with the capital by the aid of electricity. Yunnan, on the extreme south-western border, has recently been connected, and other equally remote provinces will doubtless be reached without loss of time. With millions of labourers ready to work, the guiding and controlling forces, if present in sufficient numbers, might carry on operations simultaneously, if necessary, in all the eighteen provinces. And undoubtedly there will be a decided advantage in throwing up the lines in almost any fashion so long as they can be made to convey a message, if even, as is most probable, the entire system has to be reconstructed at no distant date. The main object is to so familiarize the natives of the interior with the aspect of these intrusive posts and wires, that they will combine to protect rather than destroy them. And here we are reminded of one point in which the Chinaman differs essentially from his near neighbour the Japanese. When first telegraphs were introduced in Japan, in 1871, the most violent opposition was encountered in the more remote regions at the hands of the agriculturists, who were by no means disposed to acquiesce in all the regenerative projects of the Government of "Benevolence and Light." In China the opposition emanated from the Government itself, inasmuch as considerable diplomatic pressure had to be brought to bear ere the introduction of a telegraph of any kind could be sanctioned, and it is tolerably safe to assume that in the peaceful interior of that vast empire nothing like strenuous objection will be raised to the formation of the line if only it be the aim of the engineers to wound the susceptibilities of the farmers as little as possible in selecting sites for the poles. In Japan the Government was very willing, but the people in many instances were not: in China it has been difficult to convince the Government, whilst the people are eminently docile.

The attitude of ready submission to law and order which characterizes the Chinese farming class affords reasonable ground for the belief that, unless there be a false step on the part of local officials, the telegraphs of China will enjoy an immunity from half the evils which have attended the introduction of the system into other lands. But something will certainly depend upon the policy pursued by the mandarins: it must be one of conciliation. Cultivated land is so exceedingly precious to the Chinese farmer that he can ill afford to have his property disturbed and partly occupied, even if it be to the extent of a square foot or two only, in order that posts may be planted to carry the wires. The system of farming adopted tends to the cultivation of a few acres merely by any one individual, but by diligence and attention a small plot is made to yield practically two and even three crops where one only would be raised in an equal space with us. This is the reason why the good will of the local residents, officials or farmers, will have to be secured.

When these initial difficulties have been overcome, a glorious field will await the development of the telegraphic system. Instead of following in the track of the railway, or journeying side by side therewith, the telegraph will be the forerunner and instigator of improved means of locomotion throughout this immense, almost unknown, region. Even if its effects were limited to the comparatively handy centres of the tea and silk trade there would, in a twelvemonth, be ample justification for its establishment.

It is one thing, however, to have erected a line of telegraph and another thing to provide adequately for its maintenance in efficient working order, without which it would be better not to construct it at all. When communications are interrupted for days together, as must inevitably occur in the absence of a thoroughly complete maintenance organization, the public confidence must be shaken anywhere, and certainly this will apply in full force to China. It is to this most important consideration that early attention should be directed, for the trouble begins the moment the lines are thrown open to the public. When once the merchant has experienced the sensation of being able to complete a bargain on the instant, he is apt to resent fiercely any curtailment of his privileges. It may not be out of place, therefore, to allude to the experience of the pioneers of telegraphy in Japan as evidence of the paramount necessity for establishing this branch of the service on the soundest basis possible. To begin with, testing stations ought never to be farther apart than a day's march on ordinary roads, and trained men are needed at these stations to be held in readiness to set out, on a word from head-quarters, with the necessary tools. Herein is

contained the one essential principle of systematic maintenance. Moreover, it is not enough that breakages of wire be promptly repaired, but the efficient performance of a lineman's duty demands that he should at stated periods patrol his district and remove the possible causes of interruption in the shape of branches of trees and other obstacles to perfect communication before they have time to bring about disaster. His must be the duty of making minute examination of the supports, lest rapid decay at the ground line render even a single post too weak to withstand a sudden shock, and the chain of communication be abruptly severed. He must paint and otherwise preserve these posts, and secure them by the attachment of ample stays against normal or exceptional strains. In a word, a man will find abundant work to fill up his allotted time in a district no more extensive than a day's walking will suffice for him to cover.

Now all this is not mere theorizing, but the relation of what has been done and is being daily carried into effect in Japan, and it is for these reasons that we assert that the Government of that country may claim to possess a telegraphic system worthy the name. At the present time the telegraphic organization extends to every town of any importance within the Mikado's dominions. In the majority of cases these stations are distinguished as being the head-quarters of the local government or prefecture, and all are thus brought into instantaneous communication with the departmental offices at the capital. The four islands are connected by submarine cables, and the Great Northern Telegraph Company's lines form a medium of communication between Nagasaki and the Western world. The Japanese engineer their own service, educate their operators and travelling linemen, manufacture their own apparatus, even of the most complex character, their own batteries, and the galvanized iron fittings for their poles. The insulators in use are of Japanese porcelain, the finest in quality ever produced, capable of withstanding the most severe tests that it is possible to subject them to. Iron poles are not used, because the pine and cedar flourish everywhere, and are obtainable on short notice; moreover, it is often cheaper to replace them, if decay sets in, than to invest in iron, which is costly at the outset, and heavy to transport inland. The rates for telegrams are sufficiently low to bring the convenience within the reach of all classes. Messages are transmitted in either Japanese or foreign languages with equal facility. Finally, the finances of the department are administered in such a way as to show a substantial balance at the end of the fiscal year.

When may we look for this in China?

With the advantages the pioneers of the service there possess we trust we shall not now have to wait long. But it will inevitably be discovered, if the maintenance of the lines be not provided for efficiently from the outset, that a mighty engine of Western civilization is being hampered and thwarted in its progress, and that among the mercantile classes, who ought to be its principal supporters, there will spring up a feeling of distrust which years of success will not entirely counterbalance. There is no reason why China should not manufacture for herself almost everything she requires in the way of apparatus and material, as Japan is now doing; for men of more deft and skilful touch, combined with high intelligence, than the Chinese do not exist. But all their perfection of workmanship will avail the State little if it be not supported by strict perseverance in those duties which appertain to efficient maintenance. Long lines hastily set up across country, with stations few and far between, and without competent workmen to look after them, under substantial control, will soon cease to convey an electrical current. As suggested before, it is one thing to build a line, but quite another matter to preserve it in working order, and it is to be hoped the example of the Japanese will not be lost upon their near neighbours.

J. M.

FLORA OF THE BAHAMAS.

At the Manchester meeting of the British Association a Committee was appointed, with a grant of £100, for the purpose of exploring the flora of the Bahamas. The vegetation of this group has long been known to present some very peculiar features, but it is poorly represented in European herbaria. The Committee were fortunate in securing the assistance of Baron Eggers (sometime Commandant at the Danish colony of St. Thomas), who had lately returned from an important botanical exploration in St. Domingo.

Baron Eggers started at the end of last year, and the following letter gives an interesting account of the progress which he had made up to the time of writing.

W. T. Thiselton Dyer.

Royal Gardens, Kew, February 25.

"*Fortune Island, Bahamas, February* 6, 1888.

"I finally succeeded in reaching here, and as this part of the Bahama Archipelago most likely is less known still than the islands nearer Nassau, I propose to explore this group (Fortune, Crooked, and Acklins Islands), which are not far from the centre of the whole, and which, especially the two latter, are of a good size and fairly wooded. From the day of my arrival I have been exploring this island, which is of a longitudinal form, 9 miles long by 1 to 2 miles broad, highest elevation 110 feet, entirely covered with a low forest or scrub about 10 to 16 feet high. The largest trees do not exceed 25 feet, and that height is rare.

"Partly on account of the season of the year, partly from the protracted dry weather, some of the shrubs and trees have neither flower nor fruit, whilst at the same time the herbaceous vegetation is almost absent. Yet I have succeeded in finding a good number of most interesting plants in flower or seed, and have made, besides, collections of woods and seeds. Cycads I have seen none of here in this island. *Guaiacum sanctum* seems to be common here. Some very curious composite shrubs I have met with. On the shore *Ambrosia crithmifolia* seems very common, as also *Passiflora pectinata.*

"Of palms are found *Sabal umbraculifera*, and another, probably *Sabal Palmetto*, called palmetto here by the inhabitants, which is common and used for making hats. A shrubby *Phyllanthus* is very common, as also a very small-leaved *Erythroxylon. Croton Ujalmarsonii* is frequent. Several species of *Cassia* are found, as also some acacias. One *Psychotria*, a *Phoradendron*, growing on *Byrsonima lucida, Swietenia Mahagoni*, two species of *Coccoloba*, a large-leaved *Euphorbia*, a *Cordia*, and a number of other shrubs and small trees. Of Epiphytes I have seen two Tillandsias and an *Epidendrum*, which latter grows among rocks. No mosses, but some lichens.

"Among common trees is to be noted chiefly *Hippomane Mancinella*, as also *Conocarpus erecta* in two forms, the glabrous and the silvery-haired ones, both growing indiscriminately together in small woods.

"Almost the whole surface of the island is covered with a layer of limestone, coarse, mixed with sand, about 6 inches thick, which appears to have formed a smooth cover over the whole whilst under water. It is now generally broken to pieces, but the pieces are still close together, and only separated by fissures, in which trees and shrubs grow, sending their roots down into the sandy, and sometimes marly, soil beneath. In many places there are hollows, in which a light red soil has been accumulated, and where a few attempts at cultivation are made.

"As a rule, the only cultivation here is on the sandbank that forms the western shore, and on which also the little town is situated. Here is raised some Guinea corn (*Sorghum*) and sweet potatoes, as well as cocoa-nut trees, which seem to thrive remarkably well. This whole north-

western shore, for at least 6 or 7 miles, might be one vast forest of cocoa-nut trees. The small plantations of fifty or sixty near the dwellings present a very healthy appearance, and are in full bearing.

"Otherwise the population of the place, amounting to about 500 I should suppose, support themselves by trading, sailing, collecting sponges, and going abroad as labourers for steamers in the West Indian trade. Some salt is made from an extensive salt-pond that stretches for 4 to 5 miles just inside the north-western shore. Another smaller pond is found on the south-eastern shore.

"The town is a decent little village, with a good church, school, post-office, jail, and very creditable dwellings. The people are very well behaved and decent on the whole. Among cultivated plants around dwellings I can mention *Poinciana regia*, *Casuarina equisetifolia*, *Terminalia Catappa*.

"It is very gratifying to see the spirit of neatness and order that pervades everything in the English islands, and which forms such a contrast to the squalor and utter wretchedness that marks much richer islands, like Hayti, Porto Rico, and Cuba. As the coloured population is of the same race in all these places, it can only be ascribed to the example set by the governing race in this case.

"As you may imagine, the vegetation of this, as of most of these islands, possesses a strong uniformity and sameness, as there are no elevations of any extent to produce variety, and partakes in fact of the character of the vegetation of the sea-shore. I therefore can hardly expect to add much to my collections in this place now, and therefore intend to pass to Crooked or Acklins Island as soon as an opportunity offers.

"In a certain sense, of course, locomotion is easy enough from one island to another, yet you must always wait for an opportunity if you do not want to hire a vessel or a boat for your own use.

"After I have finished this group I propose to go to Nassau, and from there to pass over to Andros, which, from what I have been able to gather, is somewhat different from the other islands, especially on account of its being full of swamps and fresh-water lakes, which ought to give the vegetation a somewhat different character. Andros, too, is heavily wooded, both with pine forests as also with other trees, of which many are cut and exported for timber.

"As the season advances I also expect to find a greater proportion of plants in blossom than at present, so as to make my collections from these islands as complete as possible. Still a number of trees will most likely be represented by their leaves only or at best in fruit, which of course cannot be avoided, unless the exploration were continued through the year, and this, as you may imagine, cannot be done for the amount at my disposal, of which necessarily a part has already been consumed by the voyage hither.

"From what I have collected already, I think, however, I am able to say that I shall get together a considerable herbarium, which I hope will contain no few novelties, and give a fair representation of the flora of this archipelago. I need hardly add that I make copious general notes on the vegetation, as well as on the natural history and physical conditions of the islands in general.

"Yours very faithfully,

"EGGERS."

NOTES.

WE understand that, in accordance with the arrangement made on March 24, an important deputation, consisting of Sir Henry Roscoe, Sir Lyon Playfair, Sir John Lubbock, and Mr. Howorth, met Mr. Stanhope and Lord Harris on Monday last, to discuss the regulations for the selection of Woolwich cadets, so far as they relate to natural science. We believe that the proposals submitted by the deputation will receive favourable consideration.

THE late Mr. Thos B. Curling, F.R.S., has bequeathed £200 free of legacy duty to the Scientific Relief Fund of the Royal Society.

A FRESH case of specimens from the borings in the Delta of the Nile has just been received at the Royal Society.

PROF. HOFMANN, the chemist, celebrated his seventieth birthday on Monday. The Emperor Frederick sent him a patent of nobility, and among many other birthday gifts were portraits of Queen Victoria and the German Empress. From the Prince Regent of Bavaria Prof. Hofmann received a high decoration.

DR. EMIL HOLUB, the African traveller, intends to open a South African Exhibition in the old Exhibition building known as the Rotunde, in Vienna, in May 1889. The industries, exports, and dwellings of the natives will be exhibited, as well as the collections made by Dr. Holub.

A YEARLY pension of 800 roubles has been granted by the Russian Government to M. Potanin in recognition of his work as an explorer in China and Mongolia.

THE question as to the best means of promoting technical education is being earnestly discussed in Russia. A Congress, summoned by the Permanent Committee for Technical Education, is about to meet at St. Petersburg for the consideration of the subject. The sum of £500 has been granted by the Government for the expenses of the Congress.

A RUSSIAN zoological station has been established at Villafranca, a few miles from Nice. The Bay of Villafranca is well known for the work that has been done there by some of the most prominent Continental biologists, and it certainly offers great advantages for the study of marine fauna. An old Italian prison, which was formerly sold to the Russian Government, and used as a kind of naval station for repairs of ships of the Russian Navy, has now been transformed into a zoological station, supported by the Russian Naval Ministry. It has two spacious and well-lighted halls for microscopical work, five smaller rooms, and accommodation for men of science who may wish to carry on biological investigations. It is under the direction of Dr. Korotneff.

WE regret to learn that Captain Temple has been compelled to discontinue the issue of his most interesting and valuable periodical, *Indian Notes and Queries*. His duties at Mandalay, where he is playing an important part in the work of reorganization in Upper Burmah, so occupies his time that he is quite unable to put together periodically the notes sent to him by many contributors. His other periodical, the *Indian Antiquary*, is to be maintained, and contributions to the now defunct serial will be diverted to it.

A DEPUTATION from the "Australian Natives' Association" waited recently on the Minister of Education of Victoria to urge that an Australian series of school-books should be published, so that fuller information on purely Australian subjects should be made available to the children in State schools. It was argued that there was virtually no special information about Australian geography in the books used. The Minister was asked to bear in mind that 650,000 of the total population of Victoria were native born, and that the vast majority were growing up in ignorance of the geography of their native land. Australian literature, like Australian geography, was neglected by the Education Department. The deputation laid particular stress on the argument that the Government would stimulate the Federal sentiment by giving Australian subjects prominence in the State schools.

THE death is announced at Yokohama, on February 17, of Mr. Henry Pryer, an old resident there, who had devoted much attention to the study of Japanese entomology and ornithology. The writer of an obituary notice in the *Japan Weekly Mail* states that, except for a short time, when engaged in arranging natural history collections in the Tokio Museum, Mr. Pryer was occupied in business pursuits, his spare time being given to the study of Japanese fauna; "and it is no exaggeration to state that he had become the authority *facile princeps* on all questions connected with the birds, butterflies, and moths, whilst at the same time he had acquired a most extensive knowledge and store of facts in connection with all the other branches of the zoology of Japan." Mr. Pryer was the author of papers in various English scientific journals, and the Transactions of the Asiatic Society of Japan. In November 1886 he published Part I. of a description of the butterflies of Japan, under the title "Rhopalocera Nihonica," and Part II. is said to be in the printers' hands, and almost ready for publication. It is to be hoped that the third and concluding part may be found in such a state of preparation as will insure the completion of the work. Mr. Pryer, who was the brother of Mr. Pryer of the North Borneo Company's service, like himself an enthusiastic student of Nature, was only thirty-nine years of age at the time of his death.

RUSSIAN zoology has sustained a heavy loss by the death of Prof. M. N. Bogdanoff. He died at St. Petersburg on March 16. His first work, published in 1867, was on the life and the geographical distribution of the *Tetrao urogallus*. Four years later he published a most elaborate and suggestive work, "The Mammals and Birds of the Black-earth Region of the Volga," in which he treated in detail the present geographical distribution of animals in connection with the climate and soil of Russia during the Post-Pliocene period. In 1873 he took part in the expedition to Khiva, and returned with a rich geological collection, now at the St. Petersburg University. The results of this journey were embodied in a capital work on the Khiva Oasis and the Sands of Kyzyl-kum. His next work was on the fauna of the Aral-Caspian basin, which he described in the *Mémoires* of the Society of Naturalists at the St. Petersburg University. In 1875 he began the exploration of the Caucasus, and published the results of his labours in a work, "The Birds of the Caucasus," which has become the foundation-stone of the ornithology of the region. In 1880 he visited a part of the coast of the Arctic Ocean, and the results of his journey were published in the *Mémoires* of the Society at St. Petersburg University. In 1881 he published another excellent work, "The Russian Magpies." Finally, in 1885, he began the publication of his life-work, "The Ornithology of Russia," of which only the first part has been issued. Prof. M. N. Bogdanoff's popular zoological sketches, published in a Russian review, were widely read. All the above-mentioned works, as also many smaller monographs, have been published in Russian.

THE Russian Geographical Society has lost one of its most active ethnographers, V. N. Mainoff, whose works on the Erzya-Mordvinians—their anthropological features and customs—are well known. His knowledge of the Finnish language gave a special value to his works on the remnants of paganism among the Mordvinians, and to his descriptive work on Karelia and the Onega region. He had already published a Finnish grammar in Russian, and was engaged in the compilation of a Finnish and Russian dictionary. The latter task was intrusted to him by the Senate of Finland. He had brought the dictionary up to the letter K.

ANOTHER masterly contribution to the fundamental principles of chemistry, leading us still further into the intensely interesting region of the hitherto unknown, will be found in the current number of the *Berichte*, from Profs. Victor Meyer and Riecke. We have from time to time in these columns noticed the progress of the development of the now famous "position-in-space" theory first formulated by Van t' Hoff, and it will perhaps be remembered that a short account was recently given of the remarkable results obtained by Prof. Meyer from the study of certain complex organic compounds. The main result consisted in the discovery of two new properties possessed by carbon atoms: first, that the four valencies may be deviated from their positions at the corners of a regular tetrahedron; and second, that two carbon atoms united by single bonds may be attached to each other in two ways—one in which they are free to rotate, and another in which rotation is prevented. Recognizing that the chemist must not tread this unbroken ground alone, but that he must go hand in hand with his co-worker the physicist, Profs. Meyer and Riecke have brought together evidence afforded by both physics and chemistry, and have thereupon formulated a theory which appears likely to be the germ of a grand generalization. They suppose the carbon atom to be a sphere surrounded by an ether shell, that the atom itself is the carrier of the specific affinity, while the surface of the ethereal envelope is the seat of the valencies. Each valency is conditioned by the existence of two oppositely electrified poles, situate at the ends of an imaginary straight line, short in comparison with the diameter of the envelope. Such a system of two poles is termed a di-pole. The four valencies of the carbon atom would thus consist of four such di-poles. The middle point of the line joining each pair of poles is further supposed to remain always in the surface of the envelope, but freely movable in that surface, and the di-pole itself would be able to rotate freely round this central point. It is then supposed that the carbon atom possesses a greater attraction for the positive than for the negative ends of the di-poles, so that, owing to the possibility of free rotation, the positive ends would turn towards the centre of the atom. At the same time the valencies of the same atom would repel each other, and take up their positions at the four corners of the regular tetrahedron, from which, however, they could be, as experiment shows they occasionally are, deflected. Thus the molecules of marsh gas, CH_4, or any carbon compound of the type Cx_4, would naturally be symmetrical, but when the four valencies are attached to groups of different weights their positions would probably be altered. A strikingly natural explanation is then given of the nature of single, double, and triple linking of carbon atoms, showing how the first can occur in the two ways previously indicated. It is a matter for sincere congratulation to those who have been labouring so long in building up the now immense fabric of organic chemistry, that it is by reason of the large accumulation of data concerning the carbon compounds that these important principles have been arrived at; and it is to be hoped that before long the data may be sufficient to permit of like investigations of the atoms of other elements.

So many interesting reports relative to waterspouts, sighted during January and February in the western portion of the North Atlantic, have been received by the Hydrographer of the United States Navy, that he has plotted them in a supplement to the Pilot Chart of the North Atlantic Ocean for March, together with the tracks of storms coincident with some of them. The positions of the spouts are given for fourteen days between January 12 and February 29. To specify a few cases:—On January 12 four spouts were seen in lat. 36° 41′ N., and long. 72° 27′ W., and on the 19th several were seen a little farther to the eastward. And again, on January 22, several large spouts were seen in lat. 31° 47′ N., long. 74° 33′ W. The most interesting of all are those seen on January 26–28, for the reason that they were clearly associated with a low barometer area

which moved across the great lakes on the 25th. One of these, seen on January 28, in lat. 39° 30′ N., long. 57° 20′ W., is estimated to have been a mile in diameter. On February 11 the ship *Reindeer* was completely dismasted by a spout in lat. 32° 4′ N., long. 76° 6′ W. The weather was clear at the time, and the whole affair was over in a few minutes. Generally speaking, the rotation was, as in the case of tornadoes, in the opposite direction to that of the hands of a watch, but, in some cases, in the same direction. It is suggested that if instantaneous photographs were taken of some of these remarkable phenomena they would be of great value to the science of meteorology.

AT the meeting of the French Meteorological Society, on March 6, M. Renou stated that the observations at Parc St. Maur showed that the month of February last was colder than it had been since 1855. The temperature of 5° F., observed on February 2, was the lowest on record in that month. On March 1 the minimum, 16°·2, was the lowest recorded in March since 1847. The Society is encouraging the registration of regular observations at the seminary at Port-au-Prince (Hayti). The thermometers, which are now better exposed, show that the temperatures are lower than formerly reported. Very few observations from this district have been published, so that a regular bulletin such as is hinted at by M. Renou would be very welcome. M. Hauvel read a communication on the "Tides of the Photosphere," due to planetary action. In classifying the planets according to their influence on the photosphere, he places Mercury first and Jupiter second, and he argues that at certain positions Mercury causes storms in the photosphere, giving rise to abnormal variations of temperature in our atmosphere, according to the relative position of the earth.

THE German Meteorological Office has published the results of its meteorological observations for the year 1886 (Berlin, 1888, lvi. + 223 pp.). The stations of the second and third orders now number 256, several new ones having been recently established. A regular system of thunderstorm observations has been commenced, and much attention is paid to rainfall; it is proposed eventually to increase these stations to 2000. In addition to the usual data, the maximum falls in twenty-four hours are given for all stations. The system includes several mountain stations, the highest being the Schneekoppe (nearly 5250 feet). The difficulties experienced in mountain meteorology may be judged of from the fact that in the winter the anemometer on the Schneekoppe has to be abandoned, owing to the accumulation of snow, and in the summer the earth thermometer has to be removed, owing to repeated interference by tourists. The history and outfit of the stations are given in many instances, and will be continued in subsequent reports.

THE Bureau of Ethnology, Smithsonian Institution, lately issued a full and useful bibliography of the Eskimo language, by Mr. J. Constantine Pilling. Now it has published an equally good bibliography of the Siouan languages, by the same scholar. The material for both of these catalogues has been gathered during personal visits to the more prominent public and private libraries of the United States, Canada, and France, and by correspondence with missionaries, Indian agents, publishers, and printers of Indian books, and owners of Americana.

AN interesting paper on the use of gold and other metals among the ancient inhabitants of Chiriqui, Isthmus of Darien, by Mr. William H. Holmes, has just been issued by the Bureau of Ethnology, Smithsonian Institution. The objects described by Mr. Holmes were obtained from ancient graves, of which no record or trustworthy tradition is preserved. They are all ornaments, no coin, weapon, tool, or utensil having come to Mr. Holmes's notice. The great majority of the objects were formed by casting in moulds. The work exhibits close analogies with that of the mainland of South America, but these analogies are found in material, treatment, and scope of employment rather than in the subject-matter of the conceptions. The sum of the art achievements of these peoples seems to Mr. Holmes to indicate a lower degree of culture than that attained by the Mexicans and the Peruvians, the ceramic art alone "challenging the world in respect to refinement of form and simplicity and delicacy of treatment."

THE Report, for the year 1886–87, of the Colonial Museum and Laboratory of New Zealand has been issued. It is the twenty-second annual report of these institutions. The attendance of visitors at the Museum was very large, being considerably above the average of former years, especially on Sunday afternoons, when the very limited passage-room often caused inconvenient crowding. The total number of additions to the collections during the year was 10,708. Among these additions were eleven photographs of the wonderful stone carvings and inscriptions found on Easter Island. In the Colonial Laboratory 345 analyses were performed during the year. These are classified as follows: coals and oils, 22; rocks and minerals, 117; metals and ores, 43; examinations for gold and silver, 81; waters, 36; miscellaneous, 46. In the report all the results of these analyses which have a general or special interest are rendered in full.

A SERIES of papers, entitled "Studies in Biology for New Zealand Students," is being issued by the Colonial Museum and Geological Survey Department of New Zealand. We have received the third paper of the series. It is by Mr. Alexander Purdie, Science Master at the Wellington Training College, who has chosen as his subject the anatomy of the common mussels (*Mytilus latus*, *edulis*, and *magellanicus*). Mr. Purdie points out that, as a subject for study, the mussel has the advantages of being readily procurable at most points of the New Zealand coast, and also of not being so small as to embarrass the tyro in the art of dissection.

WE have received the Report of the Marlborough College Natural History Society for the year ending Christmas 1887. During the year the numbers of the Society largely increased; its meetings were well supported; and the work of the Sections was in most cases considerable. Among the contents of the volume are interesting papers on Aristotle on birds, by Mr. W. Warde Fowler; the migration of birds, by Mr. A. H. Macpherson; and spiders and their allies, by the Rev. O. Pickard-Cambridge, F.R.S.

THE second number of the *Internationales Archiv für Ethnographie* contains the first instalment of a paper, in German, by J. Büttikofer, on the natives of Liberia. This contribution is accompanied by two finely coloured plates representing implements and weapons. Dr. B. Langkavel has an interesting article on the uses to which horses are put by races at an early stage of development.

A LITTLE controversy is going on in the *Internationales Archiv* about the northern limit of the regions within which the boomerang is used in Australia. Prof. Gerland, of Strasburg, in his map of the races of Oceania, has drawn the line about 15° 30′ S. lat. In the first number of the *Archiv*, Prof. Ratzel expressed his belief that the line ought not to be drawn so far north, and that it really extends only to about 18° 30′. In the second number, Prof. Gerland maintains his position, pointing out that Leichardt found the boomerang near the Macarthur River, while Edward Palmer, one of the highest authorities on all subjects relating to the North Australians, found it to the north of Mitchell River. The tribes of these northern districts have independent names for the weapon, which they use more frequently in the chase than in war.

A BOOK entitled "Mœurs et Monuments des Peuples préhistoriques," by M. de Nadaillac, is about to be published in Paris. The text will be fully illustrated.

THE *Selborne Magazine* will in future be published by Mr. Elliot Stock.

THE new number of *Mind* opens with an article on the conditions of a true philosophy, by Mr. S. H. Hodgson. There are also articles on the nature and functions of a complete symbolic language, by Mr. S. Bryant; on Dr. Martineau and the theory of vocation, by the Rev. H. Rashdale; and on the unity of consciousness, by Mr. A. F. Shand.

WE have received a little pamphlet by Dr. G. Y. Cadogan-Masterman, Medical Officer of Health, Stourport, entitled "Dermepenthesis: Animal Skin-Grafting," in which the author gives several interesting cases of successful grafting of the skin of rabbits on wounds on the human body.

AN International Exhibition of farmyard poultry, rabbits, game raised for reserved shooting, machinery and engines for bird-culture, hunting-dogs, and sporting apparatus allowed by law, will be held in Rome, at the Botanical Garden, from April 25 to May 10. The Exhibition is being organized by a Committee of the Agricultural Society of Rome.

ACCORDING to the *Naturforscher*, Herr von dem Borne-Berneuchen has succeeded in breeding, in his piscicultural establishment, specimens of the fish known in America as the black boss.

THE additions to the Zoological Society's Gardens during the past week include two Striped Hyænas (*Hyæna striata*) from Algeria, presented by Capt. E. B. Pusey, R.N.; an Ortolan Bunting (*Emberiza hortulana*), British, presented by Mr. W. H. St. Quintin; a Moorish Gecko (*Tarentola mauritanica*) from Cannes, South France, presented by Mr. J. C. Warbury; two Poiret's Newts (*Molge poireti*) from Algeria, presented by Mr. G. A. Boulenger; a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, deposited; a Central American Agouti (*Dasyprocta isthmica*) from Central America, purchased.

OUR ASTRONOMICAL COLUMN.

THE PARIS CATALOGUE.—The first two volumes of the great work undertaken by Leverrier a third of a century ago,—the re-observation of the stars of Lalande's catalogue,—have recently been published. The first volume contains the first instalment of the catalogue, viz. stars from 0h. to 6h. of R.A. observed during the years 1837 to 1881, whilst the second gives the separate observations. That this great undertaking has advanced so far towards completion is chiefly owing to the energy which has characterized the Paris Observatory under the directorship of Admiral Mouchez, and to the strength it has derived from the School of Practical Astronomy which was for several years connected with it. When Admiral Mouchez succeeded to the direction in 1878, barely one-third of the necessary observations had been secured, and the annual number of observations obtained was only about 6000 or 7000, a total which, however considerable in itself, was very inadequate in view of the 300,000 required to complete the original programme of a minimum of three observations in each element for the 47,390 stars of Lalande's catalogue. The gift by M. Bischoffsheim of the fine Eichens meridian-circle, and the assistance furnished by the pupils of the astronomical school have, however, raised the yearly average to 25,000 or 28,000 observations, and rendered it possible to commence the publication of results. As the observations include not only those made since Leverrier became Director, but also some 20,000 or 30,000 made between 1837 and 1854, under Arago's superintendence, but left unreduced by him, they have been divided into three periods, viz. 1837–53, 1854–67, and 1868–81, and severally reduced to the mean epochs 1845, 1860, or 1875. Observations subsequent to 1881, about one-fourth of the entire number, will be published separately, and a separate supplementary catalogue will also be formed of those stars which it has been found necessary to re-observe owing to the disproportion between the number of observations secured in the two elements, due to the R.A.'s in so many cases having been observed with the transit instrument, whilst the declinations were taken with the mural circle, the transit circles having been erected only in 1863 and 1877 respectively. The present section of the catalogue contains 7245 stars, and represents about 80,000 observations in both elements. It gives for each of the three periods the number of observations, the mean date, the R.A. and N.P.D. reduced to the mean epoch, and a comparison with Lalande. The precessions for 1875 are also added. The introduction, by M. Gaillot, who has superintended the reduction, contains a discussion of the probable errors of the observations, and is followed by a comparison of the present catalogue with Auwers' Bradley, and an important investigation by M. Bossert of the proper motions of a large number of stars, followed by a table of errors in Lalande's catalogue, which the present and other catalogues have brought to light.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 APRIL 15–21.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on April 15

Sun rises, 5h. 5m.; souths, 11h. 59m. 53·6s.; sets, 18h. 55m.: right asc. on meridian, 1h. 36·2m.; decl. 10° 1′ N. Sidereal Time at Sunset, 8h. 32m.

Moon (at First Quarter April 19, 12h.) rises, 7h. 23m.; souths, 15h. 9m.; sets, 23h. 4m.: right asc. on meridian, 4h. 46·0m.; decl. 18° 18′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	4 41 ...	10 38 ...	16 35 ...	0 13·8 ...	1 21 S.
Venus.....	4 34 ...	10 36 ...	16 38 ...	0 12·6 ...	0 20 S.
Mars......	18 6 ...	23 39 ...	5 12*...	13 16·9 ...	5 55 S.
Jupiter....	22 26*...	2 40 ...	6 54 ...	16 15·1 ...	20 14 S.
Saturn....	10 33 ...	18 31 ...	2 29*...	8 8·5 ...	20 46 N.
Uranus...	17 40 ...	23 17 ...	4 54*...	12 55·3 ...	5 10 S.
Neptune..	6 28 ...	14 10 ...	21 52 ...	3 46·6 ...	18 15 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

April.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
16 ...	χ^2 Orionis	... 6	... 21 27	... 22 26	... 139 300
19 ...	θ Cancri ...	... 6	... 19 34	... 20 45	... 70 308

April.	h.	
19 ...	13 ...	Saturn in conjunction with and 1° 5′ north of the Moon.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·4 ...	81 16 N. ...	Apr. 17,	3 41	*m*
Algol	3 0·9 ...	40 31 N. ...	,, 18,	21 4	*m*
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,, 20,	22 14	*m*
δ Libræ	14 55·0 ...	8 4 S. ...	,, 17,	22 30	*m*
U Coronæ	15 13·6 ...	32 3 N. ...	,, 17,	3 31	*m*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 16,	3 44	*m*
		and at intervals of		20 8	
X Sagittarii... ...	17 40·5 ..	27 47 S. ...	Apr. 15,	4 0	*M*
Z Sagittarii... ...	18 14·8 ...	18 55 S. ...	,, 15,	0 0	*m*
β Lyræ...	18 46·0 ...	33 14 N. ...	,, 18,	22 0	m_2
R Lyræ	18 51·9 ...	43 48 N. ...	,, 17,		*M*
S Vulpeculæ ...	19 43·8 ...	27 1 N. ...	,, 20,		*M*
R Sagittæ	20 9·0 ...	16 23 N. ...	,, 18,		*m*
T Cephei	21 8·1 ...	68 2 N. ...	,, 16,		*M*
δ Cephei	22 25·0 ...	57 51 N. ...	,, 19,	3 0	*M*
R Aquarii	23 38·0 ...	15 54 S. ..	,, 17,		*M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers.

	R.A.	Decl.		
Near β Serpentis	232°	17° N.	Very swift.	
From Hercules...	255	37 N.	April 12–25	Very swift.
	268	33 N.	*Lyrids*, April 18–20	
	272	20 N.	April 18–24	
From Vulpecula	300	24 N.	April 19–20.	Swift.

GEOGRAPHICAL NOTES.

THE Russian Geographical Society elaborated at its last meeting the following programme of work for the next summer. M. Kuznetsoff will continue his geo-botanical work on the northern slope of Caucasus, and M. Rossikoff will continue his survey of the Caucasian glaciers on the little-known southern slope of West Caucasus. M. Listoff will also resume his exploration of the caves containing layers of ice in Crimea. Pendulum measurements will be done by Prof. Sokoloff in Poland and West Russia; and an Expedition of three persons will be sent out for the exploration of the Kola peninsula.

THE following details of the Brazilian Expedition, headed by Dr. von Steinen, have been received from Dr. Ehrenreich, one of the members of the Expedition. Their object was to investigate the Kuluene River, a tributary of the Xingu. Dr. Ehrenreich gives the following as the chief results of the Expedition: (1) the discovery of great Caribbean races in the centre of South America, named respectively the Bakairi and the Nahugua; (2) the discovery of the Kanayura and Anite tribes, who still speak the ancient Tupi language, and use remarkable weapons, amongst which is the very peculiar arrow fling. Surveys of the Kuluene were made and many ethnographical specimens have been collected, forming a complete picture of the original culture of these Indians, who, even to-day, do not know the use of metal, but are still in the period of implements made of flint, bone, and fish teeth.

OUR ELECTRICAL COLUMN.

J. T. BOTTOMLEY showed that the temperature of a wire conveying electric currents varied with the air-pressures surrounding it, and that a wire which remained dull at ordinary atmospheric pressure incandesced when a moderate vacuum was obtained. M. Cailletet has been working in the opposite direction. He has shown that a current which would fuse a wire under ordinary pressure will scarcely raise it to redness when the pressure is sufficiently great. These experiments show how essential free convection as well as radiation is to the incandescence of filaments in glow-lamps, as well as to the heating of conductors.

LECHER (*Rep. der Physik*, xxiii. p. 795) has experimented on the much-vexed question of the counter-electromotive force of arc lamps, and he finds that its existence is not proved, that the observed difference of potential which is expressed by the formula $a + bl$ varies with temperature, and that it is probably due to discontinuity in the current.

CONSIDERABLE attention has lately been devoted to the potential difference between the various constituents of a voltaic cell by direct measurement, an operation facilitated by Helmholtz's capital observation that this difference between an electrode of mercury flowing in drops through a capillary tube and an electrolyte is *nothing*. The mercury thus acquires the potential of the electrolyte, and can be measured. Moser (*Beiblätter*, xi. p. 788) has thus measured the Daniell and Clark cells, and Miesler has been following it up. Thus in the Daniell cell—

$Zn \mid ZnSO_4$	$= +\ 1{\cdot}06$	volt
$ZnSO_4 \mid CuSO_4$	$= +\ {\cdot}22$	,,
$CuSO_4 \mid Cu$	$= -\ {\cdot}22$	,,
Total PD ...	$1{\cdot}06$	,,

In the Grove cell—

$Zn \mid H_2SO_4$	$= +\ 1{\cdot}06$	volt
$H_2SO_4 \mid HNO_3$	$= +\ {\cdot}36$	,,
$HNO_3 \mid Pt$	$= +\ {\cdot}20$	,,
Total PD ...	$1{\cdot}62$	,,

He makes the PD—

$C \mid HNO_3$	$= +\ {\cdot}38$	volt
$C \mid H_2CrO_4$	$= +\ {\cdot}62$	,,
$H_2SO_4 \mid H_2CrO_4$	$= +\ {\cdot}5$	,,
$PbO_2 \mid H_2SO_4$	$= +\ 1{\cdot}3$	,,
$H_2SO_4 \mid Pb$	$= +\ {\cdot}9$	,,

all the measurements, except that of the Grove cell, according fairly well with known and accepted measurements.

HERTZ, WIEDEMANN, AND EBERT have been experimenting on the influence of rays of high refrangibility on electrical discharges, and M. Hallwachs has been verifying their results. He finds that a well-insulated disk of zinc charged with electricity rapidly loses its charge when the rays of an arc lamp fall upon it. It is more rapid with negative than with positive charges.

PENDULUM SEISMOMETERS.

PENDULUM SEISMOMETERS are among the oldest forms of instruments employed to record earthquake motion upon a stationary plate. In 1841 crude forms of such seismometers were used to record shocks at Comrie in Scotland. The objections to the older forms of these instruments are that they are not provided with any arrangement to magnify the motion of the earth, the writing indices are not sufficiently frictionless, and the value of the records are destroyed because the pendulums almost invariably swing (see "Experiments in Observational Seismology," by J. Milne, Trans. Seis. Soc., vol. iii. p. 12). The first pendulum seismometer with which I am acquainted which has a multiplying index is the one described, constructed, and successfully employed by Dr. G. Wagener (see Trans. Seis. Soc., vol. i. p. 55). From Dr. Wagener's account of this instrument it was the inventor's intention to counteract any tendency of the pendulum bob to swing by the inertia of the multiplying index, and from his experience with the instrument, owing to frictional resistance or otherwise, it seems that even if the pendulum was set in motion it quickly came to rest.

The multiplying arrangement, or "indicating pendulum," in Wagener's instrument was a lever, which we will call $a\ b\ c$, 25 inches in length (Fig. 1); the upper end of this at a geared

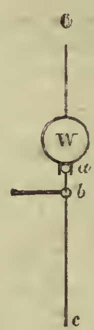


FIG. 1.

in the base of the main pendulum bob W by a ball-and-socket joint. One inch below, at b, a second ball-and-socket joint connected the lever with the earth. Now if a remained at rest, and b, being connected with the earth, moved backwards and forwards, a multiplied representation of this movement was produced at c, 24 inches lower down. The question which arises is whether W tends to remain at rest, and what effect the jointed system $a\ b\ c$ exerts upon it.

Imagine that an impulse is received towards the right, so that the point of suspension of W at o, and the point b, move to the right. The tendency of W is therefore to move to the right. If the centre of oscillation of $a\ b\ c$ relatively to b as a centre of percussion is *below* b, then a will move to the right and assist W in its swing; if, however, the centre of oscillation is *above* b then W will be retarded in its motion. In Dr. Wagener's instruments the centre of oscillation was below b, and hence the index retarded W by its inertia and friction only. Still, the instrument was the first one where there was an attempt to use an "indicating

pendulum," first as a multiplying index, and secondly as a means to check the motion of a large pendulum. In pendulum seismographs, which I have largely used in Japan (see Trans. Seis. Soc., vol. iv. p. 91), *a b* was loaded with a brass ball, and thus the centre of oscillation raised above *b*. The moment that *a b* exerted on W was not, however, sufficient to prevent W from swinging, and its movements were retarded and rendered "dead beat" by frictional resistance directly applied to the surface of W, which was a disk of lead suspended horizontally. During the last two years I have had several seismographs constructed in which *a b* was long; and, as near to *a* as possible, a weight sufficiently large to render W feebly stable was placed. This important suggestion of loading *a b* originated with Mr. T. Gray. Later, Mr. Gray drew attention to the necessity of rendering an ordinary pendulum, for small displacements, absolutely astatic, and he suggested various means by which this might be accomplished (Trans. Seis. Soc., vol. iii. p. 145).

In the same publication, vol. v. p. 89, Prof. Ewing, attacking the same problem, described a duplex pendulum, a modified form of which he described in vol. vi. p. 19. In vol. viii. p. 83, Prof. Sekiya described an improved form of Prof. Ewing's instrument (see also NATURE, vol. xxxiv. p. 343). In the duplex pendulum seismograph an ordinary pendulum is rendered astatic for small displacements by placing an inverted pendulum beneath it, and so uniting the bobs of the two pendulums that any horizontal motion is common to both, and the jointed system so proportioned that neutral or feebly stable equilibrium is obtained. Although these instruments are, for seismometrical work, theoretically good, in practice such of them as I have had, which are the best to be obtained in this country, present many serious objections. Among these objections I may mention the following: (1) the difficulty of adjustment; (2) the difficulty of inserting and removing smoked glass plates; (3) the fact that the pointer being cranked at its upper end it does not give so satisfactory a record in directions at right angles to the plane of the crank as is desired; (4) their incapability of recording an earthquake of greater amplitude than 5 mm.

By introducing arrangements for adjustment, alteration in the form of the recording index, &c., these instruments might be improved. Possibly in the instrument recommended by Prof. Ewing for use in Observatories (see NATURE, vol. xxxiv. p. 343), although it appears to be practically similar to those I have in Tokio, the objections may not be so serious.

The instrument of this class which I have in all respects found the most satisfactory is, in its essential features, shown in Fig. 2,

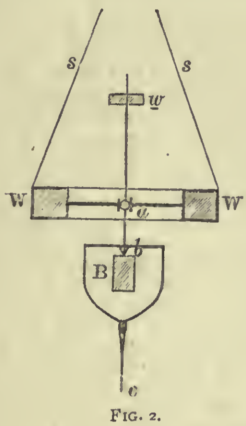


FIG. 2.

in which W represents a lead ring about 7 inches in diameter, with a small tube, *a*, fixed in a plate at its centre. W is supported by three strings or wires, *s*. The indicating pointer is *w a b c*, prolonged downwards, at the lower end there being a needle as a writing-point sliding in a small tube. *w a b* is a light steel rod with a ball forming a universal joint on the tube at *a*, and a point, *b*, pivoting in the fixed steel bar B. The stability of the system is readily altered by raising or lowering the *small* weight *w*. For small displacements neutrality is obtained when $\frac{w}{W} = \frac{p^2}{Ll}$ where $p = ab$, L the length of the main pendulum, and l the length of the inverted pendulum.

The whole is carried on a tripod about 2 feet 3 inches high, stiffened in the centre by a small transverse table which carries the bar B. W is so suspended that it can be readily shifted laterally or vertically. Below there is a small shaft which carries the smoked plate. By means of a wedge this can be raised or lowered, and the plate brought to any degree of contact with the sliding pointer. This portion of the apparatus is so simple that a record-receiving surface is instantly adjusted or removed by the movement of a handle connected with the wedge. The instrument is an outcome of instruments which have preceded it, and it may be regarded as a modification of an old type where *a b c* has been prolonged upwards and the balance load placed above *a* instead of being between *a* and *b*. Its chief recommendations are: (1) its smallness; (2) the simplicity and fewness of its parts; (3) the ease with which it may be used; (4) its large range of motion; (5) the accuracy of its diagrams. The test for accuracy has been made by placing the instrument upon a specially designed shaking table, the absolute movements of which are recorded by a multiplying lever.

Comparing the diagrams given by the machine with those given by the table, it is found that for all small displacements, whether in right lines or complicated curves, the diagrams, 20 or 30 mm. in length, are practically identical. For diagrams 50 mm. in their greatest dimensions, composed of a complication of curves if anything greater in complexity than those yielded by ordinary earthquakes, some differences occur, the extent of which may be judged of by the accompanying diagram, Fig. 3. Figs. 4

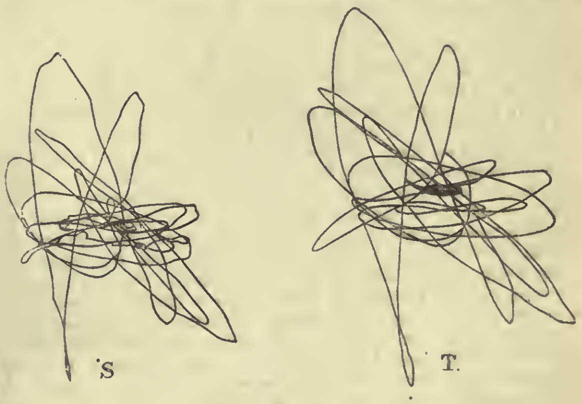


FIG. 3.

and 5 are examples of the diagrams obtained for small displacements. These diagrams are fair specimens, but have not been selected as particularly good examples. The multiplication of the table diagram, marked T, is 6·3, while that of the seismograph, marked S, is slightly over 6.

Diagrams of the old type of seismograph with the weight on *a b* have also compared favourably with the table motion. I regret, however, to say that the diagrams given by one of Prof.

FIG. 4.

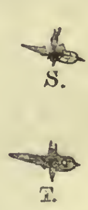


FIG. 5.

Ewing's duplex pendulums, with the exception of their amplitude, in no way resembled the table motion. This instrument was adjusted to have extremely feeble stability. With a second of Prof. Ewing's instruments, which was adjusted by Prof. Sekiya's assistant, who understood the machine, the distortion was not so great, but the diagrams were complicated by the swinging of the pendulum after the shaking had ceased. The pendulum in this instance had a period of about two seconds, which was much too short.

JOHN MILNE.

THE CULTIVATION OF OYSTERS.

A REPORT from the British Consul at Baltimore on the oyster-fisheries of Maryland, which has just been laid before Parliament, contains much interesting information respecting the cultivation of oysters. The method of farming most successful in America consists in depositing clean oyster-shells upon the bottom, just before the spawning-season, to which the young attach themselves, and then placing among the shells a few mature oysters to furnish eggs and young. As soon as the young oysters caught in this manner are large enough to handle, they are distributed over the bottom. Another system is by artificial propagation, properly so called—that is, by producing the seed-oyster itself, or procuring it by methods less simple than the shell-sowing process. This method is due to a discovery by Dr. W. K. Brooks that the *Ostrea virginiana*, or American oyster, is not, like the *Ostrea edulis*, or oyster of Northern Europe, hermaphrodite, but is exclusively male or exclusively female. The eggs of the European oyster are fertilized within the valves of the parent, while in the case of the American oyster, fertilization takes place in the broad and open waters. By experiment Dr. Brooks discovered how artificial fertilization could be procured, and the next great step of finding a simple and practical method of rearing the young oysters which have been hatched artificially was the work of M. Bouchon Brandslé, the French naturalist, who experimented with Portuguese oysters, which, like the American variety, are of distinct sexes. He succeeded in rearing many seed-oysters fit for planting. Another highly important industry which is springing up in the United States, and which also owes its existence to a careful study of the habits of the bivalve, is that of "muzzling" oysters, by which they can be sent long distances in their shells with perfect safety. Until recently, the general practice was to pack the raw oysters in ice, but a sudden rise of temperature is liable to render a whole week's supply useless. Oysters feed twice a day; and always at the still moment preceding the turn of the tide, and at no other time, except when feeding, do they open their shells. When taken out of their natural element, they attempt to feed at regular intervals, and so soon as the shells open, the liquor they contain is all lost, the air takes its place, and the oyster is covered with a thick coating of slime, which is the first stage of decomposition. As long as the shells are closed, the oyster is fit to eat; it feeds on the liquor in the shell, and will thus keep in good condition for a considerable time. To secure the keeping of the shells closed, a method has been invented of tying them with stout wire, which can be done with great rapidity, and now arrangements are being made for despatching American oysters in their natural condition all over the civilized world.

SCIENTIFIC SERIALS.

The Quarterly Journal of Microscopical Science, February, 1888, contains:—On the Photospheria of *Nyctiphanes norvegica*, G. O. Sars, by Rupert Vallentin and J. T. Cunningham (Plate 23). The authors give an account of their examination of the luminous organs of this little crustacean; it is a distinctly northern form, being absent from the Mediterranean and the warmer parts of the Atlantic. It is abundant on the west coast of Norway; the adults seem to live on the bottom and never swim far from the ground, while the young, up to half or three-quarters the size of the adult, occur abundantly at the very surface, and at all intermediate depths. Mr. Murray found swarms in the Faroe Channel, and it seems common in the the Clyde sea-area; the authors took it in abundance off Brodick Bay. The histological details of the luminous organs are given in detail, and agree for the most part with those of G. C. Sars.—On the early stages of the development of a South American species of Peripatus, by W. L. Sclater (Plate 24). These details are worked out from a species found by Mr. Sclater in Demerara, and called by him *P. imthurni*; the early stages present great differences when compared with those described by M. Sedgwick in *P. capensis*.—On the anatomy of *Allurus tetraëdrus* (Eisen), by Frank E. Beddard (Plate 25). The specimen described came from Teneriffe; there are several structural differences between this genus and Allolobophora.—On the development of the Cape species of Peripatus; Part iv. the changes from the G stage to birth, by Adam Sedgwick, F.R.S. (Plates 26-29).—On the occurrence of numerous Nephridia in the same segment of certain earthworms, and on the relationship between the excretory system in the Annelida and in the Platyhelminths, by Frank E. Beddard (Plates 30 and 31).—On the anatomy of the Madreporia, iv., by Dr. G. Herbert Fowler (Plates 32 and 33). The author gives the result of his investigations of the species of seven more genera of the Madrepores, which, among other important results, seem to establish a relationship between the external body-wall and the corallum, which depending on the presence or absence of cœnenchyma may yield a distinctive morphological character. In all those genera in which a cœnenchyma is found, whether they belong to the Perforata or Imperforata, the body-wall rests on the little spikes or echinulations which stud the surface of the corallum. A new species of Seriatopora is described as *S. tenuicornis*; it was found by Dr. S. J. Hickson at the Celebes; it comes near *S. caliendrum*.

Transactions and Proceedings of the New Zealand Institute for 1886, vol. xix. (Wellington, May 1887).—The principal contents of this volume, edited as usual by Sir James Hector, are as follow:—*Zoology*: E. Meyrick, monograph of New Zealand Noctuina, describes sixty-three species.—W. M. Maskell, on the "honeydew" of Coccidæ, and the Fungus accompanying these insects; Further notes on New Zealand Coccidæ; On the freshwater Infusoria of the Wellington district. In the second paper a new genus and two new species are described; in the last many new species are described, and several well-known British forms are recorded; all these papers are illustrated.—G. V. Hudson, on New Zealand glow-worms.—T. W. Kirk, notes, double earth-worm; New species of Ixodes; *Zootoca vivipara*, in New Zealand; New species of Alpheus.—A. Purdie, *Pasiphila lichenodes* sp. nov., and descriptions of larvæ of three species of the genus.—A. T. Urquhart, on new species of Araneidea; On the work of earth-worms.—W. W. Smith, notes on New Zealand earth-worms, gives some very interesting details.—W. Colenso, deformed bill of a Huia; New species of Hemideina; Gestation of a species of Naultinus.—T. Jeffery Parker, on *Palinurus lalandii* and *P. edwardsii*, decides that there are constant though slight differences between the two species; *P. edwardsii*, Hutton, being the New Zealand form, the other being the Cape of Good Hope form.—C. W. Robson, new giant cuttle-fish (*Architeuthis kirkii*).—J. A. Newell, anatomy of *Patinella radians*.—T. F. Cheeseman, Mollusca of the vicinity of Auckland.—J. Adams, land Mollusca of the Thames gold-fields.—A. Reischek, Hauturu Island and its birds; Ornithological notes.—S. Weetman, Moa remains on the Great Barrier Island.—R. Haeusler, Foraminifera from Hauraki Gulf.—P. Goyen, descriptions of new spiders.—*Botany*: J. Buchanan, new native plants; *Hemitelia smithii*, a branching specimen.—T. F. Cheeseman, on the New Zealand species of Coprosma.—W. Colenso, on tree ferns; On some new Phænogamic plants; On some new Cryptogamic plants; Fungi recently discovered in New Zealand.—Catherine Alexander, on the glands in the stem and leaf of *Myoporum lætum*.—T. W. Rowe, on the development of the flower of *Coriaria ruscifolia*.—J. Baber, medicinal properties of some New Zealand plants.—D. Petrie, descriptions of new native plants.—*Geology*: J. Park, ascent of Ruapehu, the exact height was not apparently determined, "about 9000 feet high."—There is a series of important papers on the eruption of Tarawera Mountain and Rotomahana, by J. A. Pond and S. Percy Smith, Major Mair, L. Cussen, Archdeacon Williams, E. P. Dumerque, and H. Hill.—Prof. F. W. Hutton, on the geology of the Trelissick or Broken River Basin, Selwyn County; On the so-called gabbro of Dun Mountain; On the geology of the country between Oamaru and Moeraki; On the geology of the Valley of the Waihao in South Canterbury.—A. McKay, the Waihao greensands and their relation to the Ototara limestone.—Sir J. von Haast, notes on the age and subdivisions of the sedimentary rocks in the Canterbury Mountains, based upon the palæontological researches of Baron von Ettingshausen.—W. S. Hamilton, notes on the geology of the Bluff District.—J. Goodall, on the formation of Timaru Downs.

Reale Istituto Lombardo, March 8.—This number is mainly occupied with E. G. Celoria's determination of some new orbits of the double stars OΣ 298 in the constellation of Boötes and β Delphini. The results of thirty-nine distinct observations are tabulated, and compared with previous more or less approximate determinations of these orbits by Burnham, Dawes, Dembowski, Dunér, Engelmann, Asaph Hall, Perrotin, Schiaparelli, Seabroke, Struve, and Wilson.

Rivista Scientifico-Industriale, March 31.—Influence of magnetism on the electric resistance of solid conductors, by Dr. Faè. In this paper the author explains the conclusions already announced for cobalt and antimony, and describes his further researches on other bodies in connection with the influence of magnetism on their electric resistance. He concludes generally that the resistance of the principal solid conductors undergoes modifications in the magnetic field, such modifications being perceptible enough in the highly magnetic or diamagnetic metals, but most conspicuous in bismuth. In all other metals it is very slight, and at times quite inappreciable. Under like conditions the resistance in the direction of the lines of force increases both for the magnetic and diamagnetic metals, while in the direction normal to the lines of force it diminishes in the first and increases in the second, although under special conditions iron and steel behave exceptionally. These variations of resistance make it probable that Hall's phenomenon depends in effect on a transitory change produced by the magnetism in the structure of the metals, and causing a rotatory variation in the electric resistance.—Dr. Luigi Fritsch describes an industrial product of the nitrate of ethyl.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 2.—"On the Voltaic Circles producible by the mutual Neutralization of Acid and Alkaline Fluids, and on various related Forms of Electromotors." By C. R. Alder Wright, D.Sc., F.R.S., Lecturer on Chemistry and Physics, and C. Thompson, F.I.C., F.C.S., Demonstrator of Chemistry in St. Mary's Hospital Medical School.

About the beginning of the present century it was noticed that when platinum plates are immersed respectively in an acid and an alkaline fluid (*e.g.* diluted sulphuric acid and caustic potash solution), and connected with a galvanometer, a much stronger current flows at first than after passing awhile: which circumstance may be explained by supposing that in virtue of the chemical action taking place between the two fluids a current is generated, the flowing of which necessarily causes electrolysis of the liquids, so that the plates become "polarized" by the evolution thereon of hydrogen and oxygen respectively, whereby an inverse E.M.F. is set up, gas battery fashion. It was shown subsequently by Becquerel that when *nitric* acid is thus used in conjunction with caustic potash a much more powerful continuous current can be generated, the passage of which is accompanied by a continuous evolution of oxygen from the plate immersed in the alkali, whilst the nitric acid is simultaneously reduced, forming lower oxides of nitrogen: whence the term "pile à oxygène" applied to the combination. In this arrangement the hydrogen supposed to be formed electrolytically can never actually make its appearance in the free state, being oxidized whilst nascent by the nitric acid; so that as the gas battery inverse E.M.F. is not developed, the continuous current passing is not so much weakened; the oxygen set free by electrolysis consequently passes off continuously at the other plate.

It occurred to the authors that, if this reasoning be correct, firstly, other oxidizing acid liquids besides nitric acid should be able to act in the same way, causing continuous oxygen evolution at the plate immersed in the alkali. Secondly, by parity of reasoning, if ordinary dilute sulphuric acid be used on the one side opposed to an alkaline fluid also containing some readily oxidizable substance dissolved therein, continuous *hydrogen* evolution should, under favourable circumstances, be produced at the plate in the acid, the oxygen evolved at the other plate being acted upon while nascent by the oxidizable substance present, so as to be suppressed just as the hydrogen is in Becquerel's "pile à oxygène." Thirdly, whether oxygen or hydrogen be continuously evolved, the quantity liberated should be proportionate to the current passing; so that, if a silver voltameter be included in the circuit, for every milligramme-equivalent (108 mgrms.) of silver deposited 1 mgrm.-equivalent of gas should be liberated; *i.e.* 8 mgrms. of hydrogen occupying at 0° and 760 mm. 5·6 c.c.; or 1 mgrm. of hydrogen occupying 11·2 c.c.

A number of cells were arranged, consisting of two porcelain basins or beakers, one containing the acid and the other the alkaline fluid united by a siphon tube, or by a thick wick, containing or wetted with a strong solution of the salt formed by the union of the acid and alkali (*e.g.* sulphate of soda when sulphuric acid and caustic soda were used, and so on). A plate of platinum foil was placed in each fluid attached to a platinum wire, and arranged under an inverted graduated tube filled with the liquid pertaining to that side of the cell, so that any evolved gas could be collected and measured, loss of gas from evolution at the surface of the wire outside the tube being avoided by coating the wire with gutta percha or paraffin wax. A small silver voltameter with a gold plate as negative electrode was always included in the circuit, so as to permit of the deposited silver being determined. Numerous experiments thus made are described, the results of which were always in sensible accordance with the above previsions, a considerable variety of acid oxidizing fluids and alkaline oxidizable solutions being employed.

These results render it probable that, if, instead of a platinum plate and an oxidizable substance in solution, there be used simple caustic soda or ammonia, and an oxidizable metal, the oxide of which is soluble in the alkaline fluid, continuous currents might be set up (in certain cases at least), even though the metal used have of itself no visible action on the alkaline fluid, apart from its absorbing oxygen dissolved therein or in contact therewith; for instance, metallic tin or lead in contact with caustic soda, or copper immersed in ammonia solution. On trying such experiments, continuous evolution of hydrogen from the surface of the platinum plate immersed in the acid was found in many instances to be readily brought about, the amount evolved being (as might *a priori* be anticipated) proportionate to the current passing, *i.e.* to the quantity of silver deposited in a silver voltameter included in the circuit. By employing an alkaline solution of potassium cyanide, it was found easy to produce the same result when certain metals of the non-oxidizable class (gold, silver, palladium, and mercury, but *not* platinum) were used instead of really oxidizable ones.

In most cases the quantity of metal taken into solution in the alkaline fluid was practically identical with that equivalent to the current passing, calculated on the assumption that the nascent oxygen due to the electrolysis combined with the metal *to form the lowest oxide* thereof, in the various cases respectively. In some few instances a slight excess of metal was dissolved, obviously due either to local action or the effect of small quantities of dissolved air. Two well-marked exceptions to the general rule, however, were noticed: one was *tin*, which when dissolved in caustic soda invariably went into solution to an appreciably less extent than corresponded with SnO; instead of fifty-nine parts of tin being dissolved for every 108 of silver deposited in the volameter, only quantities amounting to 93 to 97 per cent. of that amount were dissolved, indicating that some little quantity of SnO_2 was formed as well as SnO, although the latter largely predominated. The other exception was *mercury*, which in contact with potassium cyanide dissolved to only half the extent due to formation of Hg_2O, *mercuric* potassio-cyanide being produced. Copper, whether in contact with ammonia or with potassium cyanide, on the other hand, always dissolved in proportions corresponding with Cu_2O, a little excess instead of deficiency being usually noticeable through the secondary action of dissolved air.

In all these experiments, the results obtained are precisely those due to electrolysis of the salt formed by the neutralization of the acid and alkali in accordance with the scheme (for sulphuric acid and soda)—

$$\left\{\begin{array}{l} H_2SO_4 \mid Na_2SO_4 \mid Na_2SO_4 \mid 2NaOH \\ H_2 \mid SO_4Na_2 \mid SO_4Na_2 \mid SO_4Na_2 \mid H_2O + O\,; \end{array}\right.$$

where either the hydrogen or the oxygen is suppressed, whilst nascent, by combination with the fluid in contact with which it is evolved, or with the metal in the case of oxygen in the cells last described.

Accordingly it might be expected that in all actions of this kind a quantity of acid on the one hand, and of alkali on the other, proportionate to the current passing, will disappear as such on account of the mutual neutralization thus indirectly brought about. The authors have made a number of titration experiments with a view to obtaining numerical evidence on this point, with the general result of finding that such neutralization always takes place. It may be noticed that if cells be constructed with platinum electrodes immersed respectively in an alkaline fluid containing an oxidizable substance dissolved therein, and an acid fluid containing an oxidizing agent (*e.g.* caustic soda solution of pyrogallol, and sulphuric acid solution of chromic anhydride), continuous currents of very considerable power may be obtained when the internal resistance is diminished sufficiently by using cells of considerable magnitude; *e.g.* when made of

the stoneware and inner porous vessels usually employed for Grove's cells, the porous vessel being cemented into the outer stoneware vessel (by paraffin wax or other unattacked material) in such a fashion as to divide it into three compartments separated one from the other by porous dividing walls; the acid and alkaline fluids being placed in the two outermost compartments, and the innermost one being filled with a solution of a neutral salt, *e.g.* sodium sulphate.

March 1.—"On Electrical Excitation of the Occipital Lobe and adjacent Parts of the Monkey's Brain." By E. A. Schäfer, F.R.S., Jodrell Professor of Physiology in University College, London.

The following are the results of my own observations:—Electrical excitation of the posterior limb of the angular gyrus, of the upper end of the middle temporal gyrus[1] (which is continuous with it), of the whole cortex of the occipital lobe, inclusive of its mesial and under aspects and of the quadrate lobule, causes conjugate deviation of the eyes to the opposite side. The movement is not, however, in all cases a simple lateral deviation, but the lateral movement may be combined with an upward or downward inclination according to the part stimulated. Thus—

(1) Excitation of a superior zone which comprises on the external surface the posterior limb of the angular gyrus, the upper (posterior) end of the middle temporal gyrus, and the part of the occipital lobe immediately behind the external parieto-occipital fissure and on the mesial surface the quadrate lobule immediately in front of the upper end of the internal parieto-occipital fissure and the occipital lobe for a short distance behind the upper end of that fissure, produces, besides the lateral deviation, a downward inclination of the visual axes which is sometimes—especially when the stimulation is applied at or near the mesial surface—so marked as greatly to obscure the lateral deviation.

(2) Excitation of an inferior zone comprising the whole of the inferior surface of the lobe, the lower part of the mesial surface, and the posterior or lowermost part of the convex or external surface, produces, besides the lateral deviation, an upward inclination of the visual axes, which, like the downward movement resulting from stimulation of the superior zone, may be so marked as partly to obscure the lateral deviation.

(3) Excitation of an intermediate zone which comprises the greater part of the external surface (where it gradually broadens out laterally) and extends over the margin of the great longitudinal fissure to include a narrow portion of the mesial surface, produces neither upward nor downward inclination of the visual axes, but a simple lateral movement.

If, as is highly probable, the movements of the eyes, which occur on excitation of the occipital lobe and adjacent parts, are the result of the production of subjective visual sensations, these effects of excitation of the several parts of that lobe and the adjoining portions of the brain would appear to indicate—

1. A connection of the whole visual area of each hemisphere with the corresponding lateral half of each retina. (This has already been ascertained to be the case from the result of removing the whole of the area on one side, bilateral homonymous hemianopsia being thereby produced.)

(2) A connection of the superior zone with the superior part of the corresponding lateral half of each retina.

(3) A connection of the inferior zone with the inferior part of the corresponding lateral half of each retina.

(4) A connection of the intermediate zone with the middle part of the corresponding lateral half of each retina.

"A Comparison of the Latency Periods of the Ocular Muscles on Excitation of the Frontal and Occipito-Temporal Regions of the Brain." By E. A. Schäfer, F.R.S., Jodrell Professor of Physiology in University College, London.

Conjugate deviation of the eyes to the opposite side is produced by excitation of entirely different regions of the cerebral cortex.

Of these parts, excitation of which produces this result (conjugate deviation of the eyes to the opposite side), one, viz. the frontal area, is distinguished from the rest by the fact that its removal produces paralysis of that movement. This fact has been seized upon by Ferrier as indicating an important functional difference, the movements in the one case being probably caused by the direct action of this part of the cortex upon the centre of origin of the nerves to the ocular muscles; but in all other cases by indirect action, the movement when, *e.g.*, the visual or auditory region is stimulated being the result of visual or auditory impressions (subjective sensations) being provoked in the brain by the excitation, and these impressions producing indirectly the action in question. Others have supported the view that in all cases the movement is the result of the setting up of subjective sensations, but that in the case of the frontal area these are tactile or are connected with the muscular sense.

It seemed to me that light would be thrown upon the question if the period of latent stimulation of the ocular muscles were accurately determined under exactly the same conditions for the frontal and posterior (temporal and occipital) areas respectively. The result of this determination, which I have made in a number of monkeys, is to show that the latent period is longer by some hundredths of a second in the case of stimulation of the occipital lobe, or of the superior temporal gyrus than when the frontal area is stimulated; thus indicating that in the former case the nervous impulses must be transmitted through at least one more nerve centre than in the latter.

[1] Excitation of the upper end of the superior temporal gyrus gives a similar result. Since this is commonly accompanied by a movement of the opposite ear, it is usually considered that subjective auditory sensations have been called up by the excitation.

Geological Society, March 28.—Dr. W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—On some eroded agate pebbles from the Soudan, by Prof. V. Ball, F.R.S. The majority of the pebbles in a collection made by Surgeon-Major Greene in the Soudan, and presented by him to the Science and Art Museum in Dublin, are of very similar character to the agate and jasper pebbles derived from the basalts of India. It may be concluded inferentially that they came originally from a region in which basaltic rocks occur to a considerable extent. A certain number of them are eroded in a manner unlike anything noticed in India, though it is probable that similar eroded pebbles will eventually be found there. Throughout India, wherever there is a deficient subsoil-drainage or excessive evaporation and limited rainfall, salts are apparent either in supersaturated subsoil-solutions or as crystallizations in the soil. They are most abundant in basaltic regions, and in a lake occupying a hollow in the basalt in Berar carbonate of soda is deposited in abundance from the water, which becomes supersaturated during the summer. The author commented on the efficacy of such a liquid as a solvent of silica, and noticed the selective action of the agent which had affected the Soudan pebbles and had corroded some layers more than others; he suggested that, while this might be to some extent due to differences in composition, it was more probably owing to differences of nodular constitution. He considered it unnecessary to refer to the action of humic acid, because, while the salt to which the solvent action is attributed would be capable of doing such work, and would be probably abundant in the region referred to, we could not expect any great amount of humic acid in the same area. This paper gave rise to a discussion, in the course of which remarks were made by the President, Mr. Whitaker, Mr. Irving, Mr. De Rance, and Sir Warington Smith.—On the probable mode of transport of the fragments of granite and other rocks which are found embedded in the Carboniferous Limestone of the neighbourhood of Dublin, by Prof. V. Ball, F.R.S.—The Upper Eocene, comprising the Barton and Upper Bagshot formations, by J. Starkie Gardner and Henry Keeping, with an appendix by H. W. Monckton.

Royal Microscopical Society, March 14.—Dr. R. Braithwaite, Vice-President, in the chair.—The Rev. A. H. Cooke exhibited a number of photomicrographs of the odontophores of Mollusca, as an attempt to illustrate this group of objects by photography; he also referred to the valuable results obtained in the definition of species by the application of the method.—Mr. E. M. Nelson exhibited and described a new form of mechanical stage, in which two points were moved by milled heads in rectangular directions, carrying the slide with them, the slide being pressed against them, when they were withdrawn, by the hand.—Mr. C. L. Curties exhibited a new combination condenser, which, in addition to the condenser, also contained an iris diaphragm, a spot lens, and a polarizing prism.—Mr. Crisp exhibited a Collins's aquarium microscope which could be fixed by suction to the glass side of the tank; also Klonne and Müller's aquarium microscope for examining objects in a small aquarium or trough specially constructed for the purpose, and fitted with movable diaphragm slides; also a new form of Thury's 5-tube microscope for class purposes, having a reflecting prism made to rotate, so as to exhibit the object upon the stage alternately to

each of five observers.—Mr. G. Massee read a paper on the type of a new order of Fungi, Matuleæ.—Mr. J. Rattray gave a *résumé* of his paper, "A Monograph of the genus *Aulacodiscus*," the subject being illustrated by diagrams, and by a tabulated list of groups of allied species.—The Chairman announced that the date of the next *conversazione* had been fixed for April 25.

Entomological Society, April 4.—Dr. D. Sharp, President, in the chair.—Mr. H. Goss exhibited a large number of insects lately received from Baron Ferdinand von Mueller, F.R.S., of Melbourne, which had been collected by Mr. Sayer on Mount Obree, and the adjoining ranges in New Guinea, during Mr. Cuthbertson's recent expedition there under the direction of the Royal Geographical Society of Australia. The collection comprised Coleoptera, Lepidoptera, Hemiptera, Diptera, Hymenoptera, and Orthoptera. The Lepidoptera included twenty species of butterflies belonging to the genera *Calliplæa, Chanapa, Hamadryas, Melanitis, Mycalesis, Hypocysta, Tenaris, Hypolimnas, Cyrestis, Neptis, Acræa, Danis, Pithicops, Appias, Ornithoptera*, and *Eurycus*.—Mr. Osbert Salvin, F.R.S., exhibited, and made remarks on, about sixty specimens—no two of which were alike—of a species of butterfly belonging to the genus *Hypolimnas*, all of which had been caught by Mr. Woodford near Suva, Fiji, on one patch of Zinnias.—Mr. H. T. Stainton, F.R.S., exhibited, on behalf of Mr. G. C. Bignell, cases of *Thyridopteryx ephemeræformis*, collected near Charleston, U.S.A. Mr. Stainton said he hoped Mr. Bignell would not introduce this pest into England.—Mr. W. F. Kirby exhibited, and read notes on, about twenty species of South African dragon-flies lately received from Mr. Roland Trimen, F.R.S., of Cape Town. The collection included some new species.—Mr. Goss read a letter from Mr. Bignell, correcting a statement made by Mr. Poulton at the March meeting of the Society, to the effect that the variety *Valezina* of the female of *Argynnis paphia* did not occur in Devonshire. Mr. Bignell said that the variety *Valezina* was included in Mr. Reading's "Catalogue of Devonshire Lepidoptera"; and he had himself taken specimens of this variety in Bickleigh Vale, Devon.—Mr. Waterhouse read a paper entitled "Additional Observations on the Tea-bugs (*Helopeltis*) of Java," and exhibited a number of specimens of these insects. He said that the species infesting the Cinchona in Java was supposed to have been introduced from Ceylon in tea, but that he had discovered that the species on the tea and on Cinchona in Java were distinct, and that both species were distinct from *Helopeltis antonii* of Ceylon.—Herr Jacoby read a paper entitled "New, or little-known, species of Phytophagous Coleoptera from Africa and Madagascar."—A letter was read from Mr. E. C. Cotes, of the Indian Museum, Calcutta, asking for the assistance of British entomologists in working out certain groups of Coleoptera, Neuroptera, Orthoptera, Diptera, and Hymenoptera in the Indian Museum. A discussion ensued, in which Mr. McLachlan, F.R.S., Dr. Sharp, Mr. Waterhouse, Herr Jacoby, and Mr. Distant took part.

PARIS.

Academy of Sciences, April 3.—M. Janssen, President, in the chair.—A new theory of the equatorial *coudé* and of equatorials in general (continued), by MM. Lœwy and P. Puiseux. In the present paper the authors deal with the new processes for determining the position of the polar axis, concluding with some remarks on the bend of the arm. Six distinct methods are given for determining the constant n, and five for λ.—Results of comparisons of the standard Peruvian unit of measure and the international metre made by M. Benoit, presented by M. Wolf. From these comparisons, which have been made at the International Bureau of Weights and Measures, it appears that the Peruvian standard is substantially in the same condition as when it was constructed by Langlois in 1735. But it is also made evident that the Peruvian arc, measured with this standard, has been hitherto incorrectly compared with the other terrestrial arcs. In fact it is somewhat shorter than was supposed, and in a future paper the author will point out the consequences to be drawn from this error as affecting the form of the globe.—On the relations of atmospheric nitrogen with vegetable soil, by M. Th. Schlœsing. The author here deals with an objection that might be raised against the results of his previous experiments. The objection is based on the consideration that vegetable humus, like all dead organic matter, is a prey to two different kinds of microbes, one working in the absence, the other in the presence, of oxygen. But the conclusions previously arrived at do not appear to be materially affected by this circumstance.—On the blizzard of March 11 and 12 in the United States, by M. H. Faye. Comparing the public reports with the remarks of Dr. G. Hinrichs, Director of the Iowa Weather Service, the author concludes that a blizzard is a local snowstorm accompanied by an extremely sudden fall of temperature, and controlled by a general cyclonic movement passing over regions subject to great extremes of climate. The phenomenon is analogous to such atmospheric disturbances as the Russian *bora* or *buran*, the *khamsin* or sandstorms of the Sahara, the *föhn* of the Alps, all of which are modified by the different local conditions.—Remarks accompanying the presentation of a work on the Elasmotherium, by M. Albert Gaudry. From the specimens obtained from Russia a more correct idea can now be formed of this huge pachyderm than was hitherto possible. It flourished in the Quaternary epoch, and, notwithstanding several aberrant features, appears on the whole to have somewhat closely resembled the rhinoceros. Surviving till the close of the Glacial period, it became gradually modified, like the elephants and ruminants, to the altered climatic conditions, under which a subtropical vegetation was replaced by herbaceous plants.—On a disposition, by means of which powerful objectives may be employed in meridian observations, by M. G. Bigourdan. By the arrangement here described the great meridian instruments, such as those of Greenwich and Paris, which at present can scarcely observe stars beyond the twelfth magnitude, may be placed on a level with the equatorials.—Observations of the Sawerthal Comet made at the Observatory of Nice with the 0·38 m. Gautier equatorial, by M. Charlois. These observations, covering the period from March 14 to March 21, give the right ascension, polar distance, and other data for the comet and three comparison-stars.—On the velocity of sound, by MM. J. Violle and Th. Vautier. From the experiments here described it is placed beyond doubt that the velocity of the sound-wave diminishes with its intensity; also that the pitch of the sound has no influence whatever on the velocity of its propagation. The slight differences observed appear to be due solely to the different intensities of the sound-wave in the respective cases.—Photographic experiments on the penetration of light in the waters of the Lake of Geneva, by M. F. A. Forel. Comparing his present researches with those of previous years, the author finds that for the chloride of silver the limits of absolute darkness range from 45 metres in July to 110 in March; that the variations in these limits run parallel with those of the limits of visibility; and that the water of the lake is much more limpid in winter than in summer, the difference being mainly due to the greater abundance of organic matter held in suspension during the latter season.—On the latent heats of vaporization for some extremely volatile substances, by M. James Chappuis. The author points out that his own previously announced conclusions have been substantially confirmed by those recently announced by MM. Cailletet and Mathias.—On the laws of chemical equilibrium, by M. H. Le Chatelier. In connection with the discussion on the theory of the thermodynamic potentials, the author here shows how, starting with the hypothesis of MM. Gibbs and Duhem, and employing the same methods, the general formula indicated by M. Van t' Hoff may be established in an extremely simple way.—On the active crystallized matter of the poisoned arrows used by the Somali people, by M. Arnaud. This is an extract from the Wabaio plant, a species of *Carissa*, the poisonous extract from which (wabain) is shown by analysis to be a compound of carbon, hydrogen, barium, and oxygen, with the formula $C_{30}H_{46}O_{12}$.—On the adulteration of olive oils, by M. R. Brullé. A mixture of ordinary nitric acid and the albumen of jerked beef is shown to be an excellent chemical reagent for rapidly detecting the presence of one or more vegetable oils in the olive-oil of commerce.—On a simple and practical method of detecting and analyzing the impurities contained in the alcohols of commerce, by M. L. Godefroy. The reaction here described is extremely sensitive and accurate, detecting a millionth part, or 1 c.c. of impurities in 1000 litres of alcohol.—M. Eugène Dupuy describes some interesting experiments on dogs, cats, and rabbits, in connection with the motor functions of the brain. The results seem to be at variance with the theory usually advanced to explain the mode of production of movements or paralysis originating in the brain.

Astronomical Society, March 7.—M. Flammarion, President, in the chair.—M. Valderrama sent a drawing of a sunspot with white spots in its interior on January 15. M. Schmoll showed a drawing of the same on January 14. According to M.

Trouvelot, this appearance may be explained by a bridge crossing the spot, and sufficiently thin in some parts to escape detection.—MM. Giovannozzi, at Florence, and Bruquière, at Marseilles, sent some observations on the zodiacal light, which has been very bright; M. Gourdet, observations on 66 Ceti; and M. Guiot on Mira Ceti and ν Leporis.—M. Dumenil, at Yebleron, observed a meteor on February 19 whose trace remained visible for five or six minutes.—Observation of a fine meteor at Paris on February 24 by M. Mabire at 7 p.m.—M. de la Fresnaye submitted a plan of binocular telescope with total reflecting prisms to bring the two oculars within convenient distance for the two eyes.—M. Armelin, writing upon the calendar reform, said that it was perhaps entering on a practical phase.—The meeting thanked Mr. Holmes for his letter published in the *English Mechanic*. His observation of the *comes* to Polaris with a 1½-inch is thought remarkable.—M. Flammarion read a paper on a probable connection between the movements of our sun and those of α Centauri.—General Parmentier, reading a paper on the asteroids, remarked that the new planets discovered do not fill up the gaps to which he formerly called attention.—Various communications: on the lunar eclipse of January 28, by M. Moussette; a drawing of Plato, by M. Schmoll; observations on the aurora borealis, by M. Trouvelot; on a natural classification of double and multiple stars, by M. Flammarion; Vogel's chart of stellar spectra, presented by M. Secretan.

BERLIN.

Physical Society, March 16.—Prof. von Helmholtz, President, in the chair.—Doctor Koepsel demonstrated two energy-meters constructed on different principles by Messrs. Siemens and Halske, and explained the arrangements of the same.—Prof. Lampe spoke on a deficiency in elementary text-books of mechanics—namely, that they do not employ the elliptic functions so fully treated of by Gauss and Schellbach. The speaker then showed by a series of examples how easy it is to solve a number of mechanical problems by the use of these functions.—Prof. Helmholtz next showed how the nature of elliptic functions can be made clear to persons unacquainted with them by means of the movement of a pendulum.—He then briefly communicated the results of an investigation by Prof. Töpler, of Dresden, which he had yesterday laid before the Academy of Sciences: it contains a new method for the measurement of the magnetism and diamagnetism of gases. An index drop of petroleum is placed in a glass tube bent at a very obtuse angle; on one side of the index is the gas which is to be investigated and on the other side is atmospheric air. When placed between the poles of a powerful electro-magnet, the index is moved according as the gas is more or less strongly attracted than the air: the amount of displacement is measured by a microscope. The delicacy of the method is extremely great. It was in this way observed that oxygen is most magnetic, then come air and nitric oxide; nitrogen, hydrogen, carbonic oxide, carbonic acid gas, and nitrous oxide, on the other hand, are diamagnetic. The method employed in the above research can also be applied to the solution of various other problems, as, for instance, the determination of the pressure of small columns of gases.

Physiological Society, March 23.—Prof. Munk, President, in the chair.—Dr. Benda spoke on the structure of ganglion-cells, demonstrating at the same time, by means of specimens, his method of hardening the brain and spinal cord, which consists in treating them with nitric acid and potassium chromate. His further communication dealt with certain differences, now largely reconciled, in the results obtained by the speaker and by Prof. Flesch, of Bern, who was present at the meeting. The two observers now agree that certain ganglion-cells readily take up colouring-matter, while others do not, and to these Prof. Flesch attributes a difference in physiological function. Both observers further admit the existence of dark granulations in the protoplasm of the cells, but opinions differ as to the significance of the same.—Dr. Claude du Bois Reymond stated that he had long ago planned an investigation of the pupil when in darkness, and that this intention had only become realizable since the introduction of instantaneous photography by means of the momentary illumination produced by magnesium. Miethe, the discoverer of the method of momentary illumination with magnesium, has in this way taken a photograph of his own pupil after it had been exposed to complete darkness for forty minutes. As shown by the two photographs which were exhibited the result was most surprising: the diameter of the pupil was 9 to 10 mm., while the iris was at the same time reduced to a width of 1½ to 2 mm.—Prof. Gad gave an account of experiments which had been made by Sawyer, at his suggestion, with a view to determining whether the separation of irritability and conducting power, which is so often observed by neuropathologists, has any real physiological existence. When a part of the sciatic nerve, in accordance with Grünhagen's method, was exposed for some time to the action of a current of carbonic acid gas, it was found to be capable of conveying impulses generated by stimuli applied to parts of the nerve more centrally situated, but was itself insensitive to electrical stimulation applied directly to it, as Grünhagen had already found. When that part of the nerve inclosed in the chamber was exposed to the action of diluted vapour of alcohol, the result was exactly the opposite, the conducting power being lost but the irritability retained.—Prof. Gad demonstrated on prepared skulls and on living animals, the curious and scarcely known movements of chewing which may be observed in rats. The incisors of the lower jaw are capable of lateral movement in two halves united together by ligaments, and the larger part of the work done in gnawing is carried on by means of the scissor-like movement of the two incisors. The above has recently been very fully brought to notice by Künstler.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Reports of Geological Explorations during 1885-86-87 (New Zealand).—Système Silurien du Centre de la Bohême, vol. vii. Part 1, Cystidées: J. Barrande (Prague).—Watt's Dictionary of Chemistry, vol. i.: Morley and Muir (Longmans).—A Treatise on Electricity and Magnetism, vol. ii.: Mascart and Joubert, translated (De La Rue).—Elementary Chemistry: W. S. Furneaux (Longmans).—Natural Laws and Gospel Teachings: Rev. H. W. Morris (R.T.S.).—Early Prose and Poetical Works of Taylor, the Water Poet (Hamilton).—Noctes Ambrosianæ: Prof. J. Wilson (Hamilton).—First Lessons in Geometry, 2nd edition: B. H. Rau (Madras).—Abhandlungen der k. b. Gesellschaft der Wissenschaften Math. Naturw. Classe, vii. Folge, 1 Band (Prag).—Perforated Stones from California: H. W. Henshaw (Washington).—Work in Mound Exploration of the Bureau of Ethnology: C. Thomas (Washington).—Education in Bavaria: Sir P. Magnus (New York).

CONTENTS.

THURSDAY, APRIL 19, 1888.

SCIENTIFIC PROGRESS IN ELEMENTARY SCHOOLS.

A VERY remarkable Report has been received by the London School Board from a Special Committee appointed by it a year ago "to consider the present subjects and modes of instruction in the Board schools, and to report whether such changes can be made as shall secure that children leaving school shall be more fitted than they now are to perform the duties and work of life before them." [1]

The Committee, of which Mr. William Bousfield was chairman, was a strong one, representing well the various sections of the London Board. It has produced a Report of twenty-one folio pages, including no less than thirty-one recommendations, and followed by voluminous minutes of evidence given by scientific men and others, who have paid attention to elementary instruction, teachers of special subjects, inspectors, *employés* of the Board, working-men representatives, and others.

This important document is the outcome of several movements. The London Board has, throughout its existence, endeavoured to promote the teaching of science by means of systematic object-lessons; and has made several attempts to give a more practical turn to the instruction. In December 1884, a previous Special Committee reported on technical education, affirming the principle that it was not the duty of the Board to attempt to teach any particular trades, but that it was its duty so to direct the education of its scholars that they could easily take up any special work afterwards, and suggesting various ways by which this might be promoted. Since then the conviction has rapidly grown in the public mind that the teaching is too bookish; the supremacy of the three R's has been rudely assailed; and many people have asserted that other things, such as Lord Reay's three DR's (drill, drawing, and 'droitness), are equally important.

The Report—starting with this definition of education: "the harmonious development of all the faculties, bodily and mental, with which the child is endowed by Nature," —points out the deficiencies of the present curriculum. It has an earnest paragraph on moral education, and makes various remarks upon the present teaching of history, geography, social economy, and art. But its main criticism is "that the physical or bodily side of education, including the development of muscular strength, of the accuracy and sense of colour and proportion of the eye, and of the pliancy and dexterity of the hand, is almost entirely neglected; and that the mental or brain work, which occupies the great bulk of the time in schools of all kinds, is composed far too much of appeals to the memory only, resulting, at the best, in the retention in the child's mind of a mass of undigested facts, and far too little of the cultivation of intelligence." The Kindergarten principle is strongly approved of, and the first recommendation is: "That the methods of Kindergarten teaching in infant schools be developed for senior scholars throughout the standards in schools, so as to supply a graduated course of manual training in connection with science teaching and object-lessons."

These, then, are the two main directions of progress that are indicated—the knowledge of Nature, and the power of work; the development of the perceptive faculties, and the education of the senses—and these two are to go hand in hand.

Object-lessons are common in elementary schools, but much is said, both in the Report itself, and in the evidence of Sir John Lubbock and other witnesses, in regard to their improvement, and the importance of good collections of objects. Yet it appears from the appendix that only about forty minutes per week on an average are actually given to these lessons in boys' and girls' schools, and we know from the Annual Reports of the British Association on the teaching of science in such schools that the present regulations of the Government Code are actually diminishing the amount of the teaching of geography and elementary science. The Special Committee, therefore, very properly recommend that application be made to the Education Department to grant more freedom of choice in the selection of class-subjects; and that the provision for object-lessons, and lessons on natural phenomena, should be taken into account in boys' and girls' schools in assessing the merit grant, as is the case at present with infant schools. The Scotch Code has within the last few weeks allowed that either elementary science or English may be taken as the first class-subject, which is a hopeful sign of progress. The favourite scientific subjects taught at present in the London schools are animal physiology and algebra; but the Special Committee favour the teaching of mechanics and the fundamental notions of physical science by means of special teachers on the peripatetic plan; and they recommend "that the teaching of all subjects be accompanied, where possible, by experiments and ocular demonstration, and that the necessary apparatus be supplied to the schools."

As to manual instruction, it exists in infant schools wherever Kindergarten exercises are practised, but in boys' schools there is often no practice of the kind except in writing. In London, and perhaps in most large towns, drawing is generally taught, and it is universally allowed that this is at the very foundation of technical instruction. The Committee recommend "that all manual instruction should be given in connection with the scientific principles underlying the work, and with suitable drawing and geometry." Drawing to scale is invaluable for teaching accuracy in work. But drawing does not give the best idea of form, and there is a conventional element about it which puzzles little children. Hence modelling in clay is also recommended. The Board started a class for the use of tools in carpentry at Beethoven Street School, Kensal, but the outlay was disallowed by the Public Auditor. Six such classes, however, are being carried on at the expense of the City Guilds Technical Institute. There is little doubt that the present disability will be shortly removed, and that eventually a work-room or laboratory will become an essential part of every large Board school. How best to give manual instruction is still a matter of discussion and experiment. Good observations about it will be found in

[1] "School Board for London. Report of the Special Committee on the Subjects and Modes of Instruction in the Board's Schools, with Appendices." (Hazell, Watson, and Viney, 52 Long Acre.)

the evidence of Mr. Henry Cunynghame, Mr. Davis, of Birmingham, and Profs. Unwin and Perry. Mr. Ricks, one of the Board inspectors, has drawn out an elaborate scheme for the development of the Kindergarten system throughout all the standards of a school in the directions spoken of.

Girls are more fortunate than boys in the matter of manual instruction. They are taught needlework universally, and very often cookery. The latter may be considerably extended. Domestic economy also in its various branches should be taught, through practical work, and with reference to scientific principles—as in washing, laying fires, and ventilating rooms.

But how is time to be obtained for the introduction of this perceptive and practical instruction? On that point the Committee are very distinct, and there is a singular unanimity among the witnesses that the attention now paid to spelling and grammar is excessive, if not educationally worthless. There is a curious table, too, in the appendix, which gives the results of inquiry as to the subjects of instruction most or least preferred in the various schools. Grammar is so unpopular with both boys and girls that it almost always attains that bad pre-eminence. Spelling or dictation comes second. In fact there is no doubt that the children dislike what they feel does not add either to their pleasure, or their real knowledge. It is proposed "that the time now given to spelling, parsing, and grammar generally, be reduced."

There are two points on which we should have liked to see some recommendations of a more vigorous character. The one refers to the teaching of arithmetic, which as laid down by the Code is thoroughly unscientific. The other point is this: there are recommendations in regard to evening classes, the more extended use of the pupil-teachers' schools, and the grouping together of the upper standards of several schools in poor neighbourhoods; but this might have been carried much further, and have included the establishment of such valuable institutions as the central schools which are doing such good work in many of the provincial towns, especially in the North of England.

Nevertheless, these recommendations, if they are all allowed to take effect, will mark an era in education. The Special Committee are happily able to add: "It is significant that these changes are demanded alike by educational theorists, teachers, men of science, leaders of industry, and statesmen, and it rests with the Board to carry them into actual fact." The Bill of Sir Henry Roscoe, and that on technical education which is promised by the Government, must also have an important bearing on the scientific development of elementary instruction. We await the results of the discussions that must ensue with the deepest interest.

THE NERVOUS SYSTEM AND THE MIND.

The Nervous System and the Mind: a Treatise on the Dynamics of the Human Organism. By Charles Mercier, M.B. (London: Macmillan and Co., 1888.)

THE time may come when the psychological historian will be required to trace the genealogy and career of such terms as "molecular movement," "discharge," "explosion," "unstable matter," as applied to mental operations, as well as the familiar expression "environment." Whoever else may have contributed to their use, they will be traced back in the main to Herbert Spencer. When once the brain was recognized as the organ of mind in a special sense, chiefly through phrenological observations in which Mr. Spencer was himself at one time engaged (he was, if we mistake not, a member of the London Phrenological Society), the physical basis of mind was naturally described in terms applied to material bodies and employed in physics. The combination of atoms forming molecules being regarded as the fundamental element of the substance of the nervous system, molecular movements were correlated with mental operations. Every corpuscle in the gray matter of the convolutions of the brain was regarded as "a reservoir of molecular motion." It followed that the destructive molecular changes of which the granular protoplasm in the corpuscles is the seat were accompanied by a disengagement or discharge of motion. For the purpose of decomposition or waste, the amount of which is the measure of the force evolved, the remarkable supply of blood received by the cerebral convolutions was seen to be necessary; as also for the recomposition or repair which succeeds waste. Spencer drew some of his analogies from chemical explosions, taking for instance the explosion of the percussion cap and powder in a pistol to symbolize the setting up of decomposition in an adjacent ganglion-cell by (in the case of the retina) a disturbed retinal element. He showed that a partially-decomposed ganglion-cell propagates a shock through the afferent nerve to a large deposit of "unstable matter" in the optic centre, "where an immense amount of molecular motion is thereupon disengaged." The transmission of waves of molecular motion through nerve-fibres is compared by Spencer to "a row of bricks on end, so placed that each in falling knocks over its neighbour. . . . Each brick, besides the motion it receives, will pass on to the next the motion it has itself gained in falling."

These and similar propositions have for long become household words. The terms referred to have become a part of psychological, and to a large extent medical, language. One well-known outcome of Spencerian teaching has been its elaborate application to the study of epilepsy, by Dr. Hughlings Jackson, who has been always anxious to acknowledge the source from which he drew his inspiration. Dr. Mercier's book is another stream from the same source. He makes an acknowledgment of similar indebtedness in his preface. We do not think he is justified in his complaint that "the classical works on Mind ignore altogether its association with the body, and study it from a stand-point so purely introspective as to offer no obvious advantage to the alienist, to whom the concomitant disorders of body are so conspicuous and so important." Holding this opinion it became "absolutely necessary" for Dr. Mercier to prepare the present volume. The writings of Bain, Laycock, and Maudsley, no less than Spencer, are nothing if they do not insist upon the association of mind and body. The very last charge that can be fairly brought against these classical works is that they altogether ignore their correlation. The best evidence of the direction and complexion of the teaching of authors of modern works on psychology is contained in Dr. Mercier's statement that "everyone

nowadays admits that the evolution of mind and the evolution of the nervous system proceeded *pari passu*, and indeed are but two aspects of the same process." It is hardly consistent with a further statement that this way of regarding them is not only neglected but "derided and scouted." Dr. Mercier asks for our sympathy for having been for the last ten years as "the voice of one crying in the wilderness." Other voices, however, have for long been heard there, if indeed that can be called a wilderness which is peopled by the number who admit the above-mentioned proposition in regard to the evolution of the mind and the nervous system.

This work expounds Spencerian doctrines with much fulness of diction, and in a style which is forcible, not to say somewhat dogmatic. We find Spencer's illustration of molecular movements from bricks on end reproduced, and we may quote the following passage as a fair example of the author's style :—

"Imagine a brick set up on end. To do this requires the expenditure of force. Now, if the ground is shaken the brick falls, and liberates in falling a force equal to that expended in raising it. Again, imagine a brick set on end with another brick placed across the top of it. The upper brick can now be knocked off the lower, and the force which raised it be liberated, while the lower brick is left standing, with the force that raised it still in store. It is evident that a brick balanced on the top of another one will be displaced by a gentler shake than is required to knock down the single brick. . . . Now suppose more and more bricks are added until we have quite a complicated structure composed of loose bricks. It is easy to see how readily a top brick could be knocked off. . . . Now if we imagine these bricks to be connected to the pile by elastic bases, so that when they have been knocked down they will slowly rise again, with perhaps a little help, to their erect position, we shall have a diagram which will represent very roughly what we suppose to be the mechanism of the nervous discharge" (p. 23).

Dr. Mercier's copious vocabulary clothes an idea in many folds of attire. It is, we think, sometimes overloaded and too diffuse. Endowed with a large organ of comparison, he illustrates his theme with a redundant variety of illustrations or makes one illustration do duty in many forms. Great facility of expression enables him to enforce his views, though it may be at the risk of producing weariness by excessive iteration. He revels in the description of molecules—their form, their relative position, their polarity, their life, their behaviour, and their destiny. M. Renan has been said to know more about St. Paul than the apostle knew himself. Similarly Dr. Mercier would, we are quite sure, be found to know more about the molecules of the brain than, were they gifted with consciousness, they would know themselves. He might write a charming story entitled "The Biography of a Cerebral Molecule." The author divides his subject into three sections: Nervous Process, Conduct, and Mind, the first underlying the other two. In treating of "nervous discharge" he argues that the building up of a molecule implies force ; this remains latent, stored up in the gray matter. It is liberated at intervals—that is, during functional activity. The rearrangement of atoms in the molecule may be called "decompounding," while the process of destruction is more properly termed "decomposition." Thus, then, the former, together with the liberation of force accompanying it, is the "discharge." It tends to spread. How is the liberated force replaced? Dr. Mercier cannot tell. All that can be said is that it is a part of the general system of bodily nutrition. Passing on to "nervous resistance," it is assumed that there is a balance of tension and resistance in the gray matter of the brain. The subject is worked out ingeniously, and as fully as it admits of. Necessarily much is altogether inferential. The hypothetical nature of the doctrines taught by the Spencerian school no doubt deters not a few from adopting them. Such persons say that they are not scientifically proved, and they challenge those who insist on their importance to show that they can practically help the physician in his treatment of mental affections. A homely simile illustrates the doctrine of continuous resistance. A charged soda-water bottle resembles the tension of a charged nerve-cell. Withdraw the cork, and the resistance of the narrow neck causes an intermittent escape. The contents "come blobbing out in a succession of intermittent bursts," and so, according to the author, the narrow necks of nerve-cells—the fibres which proceed from them—cause analogous results.

One chapter is devoted to the co-ordination and inhibition of muscular action. Nervous discharges are regarded in terms of the latter. The discharge of an area of gray matter occasions normal movements. The simultaneous beginning, duration, and ending of muscular action depend upon the simultaneous issue of a current of force to each muscle under its influence (p. 67). The nerves of muscles connnect them with the cells of the gray matter of the brain, and muscular force depends upon the amount of nerve-discharge. Co-ordinated movements are secured by the group of nerve-elements called a nerve-centre. The initiatory impulse may come directly from the outside world—the environment. In some instances, however, this action is indirect and distant, as, when reflection ends in acts set going by "currents starting from the highest centres." To terminate the action set up, another stimulus is necessary, unless exhaustion itself terminates it. Here comes in the element of control or inhibition to which all nerve-centres are presumed to be subject, and by which they are retained in a condition of mobile equilibrium as surely as the planets in their orbits by the opposition of attraction to their own inertia. It is forcibly argued that this influence is derived from centres having other functions, and not from one exclusively set apart for this purpose. Inhibition is, in short, a higher degree or power of the resistance which causes the intermittent escape of nervous force. A wide question is here raised, and there is not as yet a consensus of opinion among physiologists in regard to it. "Movements" are dealt with in much detail. The section on the co-ordination of movements is an elaborate study of the subject. In the discussion of the nervous mechanism of co-ordination and inhibition, occasion is taken to give a minute description of Jacksonian epilepsy. In inhibition the centres which supply the impulse to start and accelerate, supply also the impulse which arrests and retards. In walking, for instance, the centres which actuate and regulate it are so arranged that they control those below, they themselves being under the control of still higher centres. If the action of the head-centre is suspended, the local and vegetative functions

are still performed. There is no paralysis. The early stage of drunkenness is a good example. There is the uncontrolled action of the centres usually subordinate to the highest controlling, but now non-functioning, centre. There is in such an instance, "the withdrawal of the stimulus of frequent positive impulses." In later stages there is something more than temporary suspension or inhibition; there is destruction of the highest centres and actual paralysis. Under "Conduct" Dr. Mercier considers the human organism and the environment along with the adjustment of the former to the latter. He carefully follows the lines of Spencer, and points out that the study of mind belongs neither to the first nor to the second, but only to their adjustment.

Our space does not admit of our following the author in his study of the "Constitution of Mind," in which he endeavours, with, we think, imperfect success, to prove that the feelings cannot be disordered without disorderofthe intelligence. Were this theoretically true, so far from having a "practical bearing of great moment," it would be an instance of an abstract theoretical proposition being, strictly speaking, true, while for all practical purposes experienced alienists tell us they find it necessary to admit a moral insanity with an average amount of intelligence. Alienists will no doubt discuss this and other conclusions stated in the third part of this work, and we leave the task in their hands. The most original portion of the work is that in which Dr. Mercier classifies cognitions and feelings, dissenting as he does from the classification of Spencer in several important particulars, upon which we cannot enter.

There will, of course, be the same criticism on the position taken by the author as is frequently offered to that of his master. True, there is no denial of mind; on the contrary, emphasis is laid upon the absolute distinction between mind and matter; but the complaint will be made that all the functions of life are explained by the brain's molecular and molar action in adjustment to environment, without taking into account the influence of what is admitted to be "mind"—in other words, thought and feeling. The circle, it will be objected, is completed without allowing for the action of at least one important factor. Dr. Mercier, for instance, writes to this effect:—"He who gets himself vaccinated is procuring a change in his constitution adapted to the existence in the outside world of the contagium of small-pox. He is procuring the adjustment of his organism to a set of conditions in his environment." What, asks the objector, is the position of the "he"? In what relation does this personal pronoun stand to the organism? How is it possible to ignore it in the explanation of mental manifestations, or what is called the adjustment of organism to environment?

Dr. Mercier advances no further than his predecessors in enabling us to answer these questions. It does not help us to insist upon the "fathomless abyss" that separates mind from matter. We get no further under his guidance than the "rearrangement of molecules in the gray matter of the superior regions of the nervous system." We are told that no process of change in the latter can cause a change of consciousness. To Dr. Mercier's mind this is "unthinkable"; therefore it is not to be thought of. Equally unthinkable is the proposition that a change in consciousness can cause a change in molecular arrangement. The two changes are, it is said, invariably simultaneous. No doubt, as the author says, the student who grasps these notions has half his difficulties surmounted. To overcome difficulties, however, by evading them and confessing our ignorance is somewhat dispiriting, and some would think pusillanimous. Dr. Mercier, while granting the existence of mind on the one hand, and movements on the other, will neither allow of such expressions as "psycho-motor" nor give us an equivalent; for the terms he himself employs exclude the mental factor altogether, although he is forward to admit its existence. There is a break in the circuit, and yet the latter is presented to us as if it were complete. On the remaining links of the chain, the work before us is a painstaking and connected, and therefore valuable, dissertation. If this missing link can only be postulated, and cannot be brought within the range of practical psychology, it must be acknowledged that the science is defective in a most essential particular. If the relation between mind and matter is unthinkable, it is not alleged that mind is so, and therefore there does not seem to be any scientific objection to the employment of terms which recognize some causal relation between mind and matter. Still less do we find in the employment of such terms the "blasphemy" which so painfully grates upon the psychologically tender conscience and rigid orthodoxy of our author Seeing, moreover, that there is an appreciable lapse of time between an idea or willing, motion, the whole of which need not be occupied in the transmission through nerve-fibres, there seems at least as much justification for using the term "ideo-motor" as many terms which describe or imply a theory which, like Dr. Mercier's own hypothesis, is confessedly inferential. And further, the expression may be, like a host of others, defective in comprehensiveness and precision, and yet be the most easily understood.

In conclusion, we would say that Dr. Mercier's work may be read with profit by the class for which it is intended, so long as it is understood that it treats of only one aspect of the relations between mind and body, and so long as it does not obscure the recognition of those great truths of cerebral physiology and mental pathology, which are not in dispute, and the teaching of which will continue to enlighten the student of psychology, when the theories of the philosophers are exploded or forgotten.

POPULAR METEOROLOGY.

L'Atmosphère—Météorologie Populaire. By Camille Flammarion. (Paris: Librairie Hatchette et Cie., 1888.)

THIS is a re-issue of a popular work that first appeared in the year 1872, and which has been enlarged and brought up to date.

Of all the subjects which are interesting not only to men of science but to people in general, there are few more important than that of the atmosphere, since, without its aërial envelope, our planet would pursue its path round the sun in silence and without life, as is the case with our moon, which bears evidence on its surface of nothing but death and desolation. The existence of an atmosphere makes all the difference in the world to a

cooled star; for, with one, its life is stirred up by millions of beings of various kinds which are always changing, by trees and shrubs and different kinds of plants which adorn its surface and supply man and beast with the food and nourishment necessary to sustain life.

Again, when we consider that, of the myriads of meteorites which people space, twenty millions, with weights varying from tons to the minutest possible specks, are met with by the earth every twenty-four hours, a new function of the atmosphere is revealed, for owing to its buffer-like action they fall harmless, and indeed almost unperceived, on the surface of our planet.

Since a knowledge of the atmosphere and its laws must be of more *service*—though not of greater interest—to those whose lot it is to sail the ocean than to those who sit at home at ease, no country should foster meteorology with more gladness than England, so many of whose subjects are under the influence of the "vital fluid," which the author gives as a definition of the atmosphere.

The volume is divided into six books, each of which contains about eight chapters. In the first book is described the atmospheric envelope, the method of determining its height and chemical composition, finishing with a chapter on sound and the history of the invention and development of the balloon which was tried at Paris in 1884, with some most interesting descriptions of ascents which from time to time have been made, including a table of the highest inhabited places, highest mountains, and the distribution of the various species of birds as regards the height to which they fly.

Light and the optical phenomena of the air are next dealt with. Reflection and refraction are first discussed, followed by the beauties of sunrise and sunset, the grandeur and magnificence of which it is almost impossible to describe. We find that not enough mention is made of the absorptive power of the atmosphere which produces at those times all the most beautiful colours. A very interesting phenomenon is here referred to—a total eclipse of the moon with the sun still above the horizon, which is brought about solely by the refraction of the atmosphere.

Then follow illustrations and descriptions of all the various kinds of rainbows, halos, and mirages which have been seen both on land and at sea.

The third book, which is by far the largest and most important of all, is on temperature. When we come to consider the enormous amount of solar heat that is poured on to the earth's surface, we may have some idea of the work which our atmosphere is continually doing for us. The atmosphere, as the author says, is in truth a huge machine, on whose action everything on our planet which has life is dependent. There are in this machine neither wheelwork, pistons, nor cogs, nevertheless it does the work of several millions of horses, and this work has for its end and effect the preservation of life.

Next we come to the waters of the earth, which play one of the greatest parts in the working of the atmosphere. All day long, and every day, water is being carried away from the earth's surface in the form of vapour, and it is chiefly in this way that the action of the sun's rays on the face of our planet is reduced. The amount of water evaporated each year, as the author states, amounts to 721 billions of cubic metres. The enormous quantity of heat which has produced this effect could melt per year eleven thousand millions of cubic metres of iron, a mass whose volume would exceed several times that of the Alps. Following this are some very interesting chapters on the seasons, containing a great many tables of the highest and lowest barometric and thermometric readings which have been taken at various places, concluding with an account of the distribution of the temperature on the surface of the globe.

The wind and general circulation of the atmosphere are the subjects of the next book, including a chapter on ocean currents, showing how the wind is influenced by them. The course of the Gulf Stream, which plays such an active part with the climate of various places, and is the most important of all currents, is here described, with a map showing its course and that of various other currents. The atmosphere, as we know, is threaded with winds, as the sea is with currents, some of which are more or less constant, others variable. But there are still other kinds of winds, especially those that characterize certain countries and certain parts of the ocean, which are more violent and destructive than the ordinary kind, such as cyclones, the simoom, &c.

In the next book the various forms and kinds of clouds are described, and illustrated by splendid coloured plates, which give very good ideas of their form, with the results obtained by M. N. Ekholm, of Hagström, of the heights of the various forms of clouds. This will be read with great interest in connection with Mr. Ralph Abercromby's latest observations.

Electricity and the various forms and ways in which it appears in our air are discussed in the sixth and last book; the aurora, the most curious and most beautiful of all forms which are assumed by it, being fully treated. These wonderful displays, which are seen to perfection in the Polar regions, and which during the long winter there tend to change its monotony by shooting forth brilliant rays of light, and illuminating a region which would otherwise be in darkness, are here described in a graphic manner, woodcuts and coloured plates illustrating the various forms they assume.

The concluding chapter is on the prediction of weather—a subject which at the present day is carried on to such a great extent, and which to a country such as ours is invaluable in giving us warning of storms that would otherwise come upon us and do much destruction.

The volume is thoroughly well written. It is profusely illustrated throughout, and there are fifteen plates printed in chromotypography and two hand-coloured plates. No pains seem to have been spared to make it an intellectual and enjoyable book; the object having been to produce a work giving a broad outline of the various causes of every-day occurrences in the atmosphere.

W. L.

OUR BOOK SHELF.

Life in Corea. By W. R. Carles, F.R.G.S. With Illustrations and Map. (London: Macmillan and Co., 1888.)

THIS is a valuable and interesting account of a country about which little definite knowledge has hitherto been accessible. There are some aspects of his subject with which Mr. Carles does not profess to deal. Apart from such incidents as happened before his own eyes, he has

nothing to tell us about the system of government, or the relations between the king and his nobles, the people and the serfs, in Corea. On the other hand, he gives a full and sometimes a very vivid account of everything he himself had opportunities of directly and carefully studying, and his book is worthy of serious attention, mainly because it consists of the results of his own personal observation. Mr. Carles went to Corea for the first time in 1883, when he not only visited the capital, Soul, but undertook, with some friends, an interesting journey inland. The object of this excursion was the inspection of a silver working, which proved to be very unworthy of its reputation. The scenery, Mr. Carles says, never failed to charm, and the people were invariably civil. At Soul he had some difficulty in obtaining anything really characteristic of native taste and skill. In the curio shops the only distinctly native article seemed to be a kind of iron casket inlaid with silver, the pattern of which was sometimes very delicate. In the spring of 1884, Mr. Carles took up his quarters at Chemulpo as H.M. Vice-Consul in Corea; and one of the best chapters in the book is that in which he sums up his impressions of Chemulpo and the neighbourhood, bringing together various facts of scientific interest, and indicating problems as to tidal and other phenomena about which he is still uncertain. In this chapter Mr. Carles offers a suggestion which is certainly worthy of the attention of men of science. He says:—

"With so much of interest on all sides, I could not help regretting that no information was ever asked for by the outside world on points which the opening of Corea would be likely to clear up. It seemed impossible that one could not be of use to some science in collecting facts which had hitherto been unattainable; but apparently consular officers are not consulted except on commercial questions. If scientific men would follow the example set by Chambers of Commerce, and ask for information which they expect to be within the reach of out-of-the-way posts, they would generally confer a boon on the officer by giving him a new special interest, and they might sometimes learn what they sought for."

Early in September 1884, Mr. Carles received instructions to undertake a journey along the high road from Soul to China, as far as the frontier town of Wi-ju; then from Wi-ju eastwards across the mountains to Kong-ge; thence south to Gensan, on the east coast; and from Gensan to Soul. In the course of this journey he watched closely for any indication of conditions favourable to industry and trade, but his observation did not lead him to take a sanguine view of the immediate future of the Coreans. Displaying little enterprise, they are extremely poor, and the prevailing opinion among them seems to be that the Government alone is capable of doing anything for the improvement of their circumstances. At Song-do, the old capital, admirable pottery used to be made, but when the seat of the government was transferred to Soul, the trade fell off, "and the workmen, refusing to follow the Court, gradually abandoned their industry, the knowledge of which has now been forgotten." Speaking of the religion of the Coreans, Mr. Carles says that, although Buddhism has been under a ban during the supremacy of the present dynasty, there is hardly a mountain valley off the main roads in which there is not a Buddhist temple; and often he came across figures of Buddha carved in relief on rocks. Fetichism still survives, and is manifested, among other ways, in the presentation of offerings to particularly fine trees. Mr. Carles gives an account of a conspiracy which caused serious trouble at Soul in 1884; and in a concluding chapter there are some careful notes on the Corean language. The interest of the book is greatly increased by the illustrations, which are mostly reproductions of some paintings in sepia by a Corean artist at Gensan.

Navigation and Nautical Astronomy. Compiled by Staff-Commander W. R. Martin, R.N. (London: Longmans, Green, and Co., 1888.)

THIS book, which has been accepted by the Lords Commissioners of the Admiralty as a text-book for the Royal Navy, is one that has been wanted for some time, as it contains the whole theory and practice of nautical astronomy in one part. The method of arranging the various problems is very good. The theory of a problem is always proved first, then the problem is worked in a theoretical manner, and lastly in the manner used by navigators, so that one gets everything to do with any one problem in two or three pages, whereas most books on this subject are divided into two parts, a theoretical and a practical. The method adopted by Staff-Commander Martin ought to prove a great advantage to all persons using his book, more especially beginners. The work is divided into two parts, the first being devoted to the various methods of fixing ships' positions by the land, and of navigating a ship by what is known as "dead reckoning." In this part also the various methods of chart construction are very fully explained, and it ought to be mentioned, for the information of naval officers, that the examples relating to charts are as much as possible arranged to be used with the "Officers' Atlas," which is supplied to each man-of-war. The examples ought therefore to be of great service to junior officers. The second part treats of the theory and practice of nautical astronomy; the method of arrangement we have already described. The volume is accompanied by the requisite charts and diagrams.

H. C. L.

A. Johnston's Botanical Plates. (Edinburgh: A. Johnston, 1888.)

THESE are coloured plates, 35 × 25 inches in size, intended for use in elementary schools. In the first instalment of nine plates, members of the following natural orders are shown: Ranunculaceæ, Papaveraceæ, Linaceæ, Acerineæ, Solanaceæ (two examples), Scrophulariaceæ, Corylaceæ, and Liliaceæ. The plants already illustrated appear to have been chosen at random, but when the series is completed a fair representation of the more important orders will no doubt be provided. The plates are well executed and boldly coloured, so that the chief external characters of the plants shown will be sufficiently obvious to the class. Some details of the structure of the flower have also been given, but these figures are rather meagre. Still, this is not a serious objection, as the chief aim of botanical teaching in elementary schools must always be to teach children to know plants by sight. For this purpose these plates, judging from the few already published, seem admirably adapted.

D. H. S.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Injuries caused by Lightning in Africa.

IN a copy of NATURE published on December 11, 1884 (vol. xxxi. p. 127), I noticed a statement by Herr von Danckelman that in all the publications relating to Africa, accounts of injuries caused by lightning are so rare that he scarcely found any literature concerning the use of lightning-conductors or the frequency of accidents caused by lightning in the tropics. After an unbroken residence of twelve years in the Egyptian Equatorial Province, I can give to your meteorological readers a little information on the subject in question, and I venture to submit

the following list of injuries which occurred during the years 1878–86. It must, however, be understood that this is not to be regarded as a complete list of the accidents which occurred, for during the years 1878–80 I was nearly always travelling about, and was therefore unable to collect information of a satisfactory character.

Year.	Name of station.	Lat. north.	Description of accident.
1884 ...	Bor	... 6 12 ...	Man killed; house burnt.
1880 ...	Lado	... 5 1 ...	Tree felled.
1882 ...	,,	... ,, ...	Man killed.
1886 ...	,,	... ,, ...	Two empty houses burnt.
1883 ...	Redjaf	... 4 44 ...	Two men badly burnt.
1886 ...	,,	... ,, ...	Woman killed.
1883 ...	Wandi	... 4 46 ...	Flagstaff felled; man bruised.
1882 ...	,,	... ,, ...	House burnt.
1880 ...	Kabajendi	... 4 37 ...	Storehouse burnt.
1883 ...	Muggi	... 4 8 ...	Two girls killed.
1885 ...	,,	... ,, ...	House burnt; girl paralyzed.
1881 ...	Labore	... 3 55 ...	Two boys killed.
1879 ...	Chor Aju	... 3 48 ...	Tree felled.
1881 ...	Dufilé	... 3 34 ...	House, with sheep, burnt.
1883 ...	,,	... ,, ...	Tree felled.
1883 ...	Wadelai	... 2 37 ...	House burnt.
1883 ...	,,	... ,, ...	House, with calves, burnt.
1878 ...	Magungo	... 2 14 ...	Woman killed.
1880 ...	Mahagi	... 2 2 ...	Flagstaff felled.
1878 ...	Kiroto	... 2 ...	Tree felled.

Captain Casati reports—

1881 ...	Neolopo (Monbuttu)...	Man badly burnt.
1886 ...	Djuaia (Unyoro) ...	Woman killed.
1886 ...	,, ,, ...	One man killed; one burnt—died in two days.
1886 ...	,, ,, ...	Tree felled near observer's house.

These cases were all observed in our stations, with the exception of two—one occurring in Wandi, in December 1882, and one in Redjaf, in November 1886. They all took place in the rainy, that is to say, in the cooler season. From the list you will see that two or three times every year we suffer some damage from lightning-strokes. It therefore does not appear that these accidents are so rare as Herr von Danckelman supposes, at least in this part of Africa, and if travellers do not report such accidents, it is probably because of their short stay in definite places. I noticed, in Schweinfurth's "Im Herzen von Africa," i. pp. 345–46, that six women were killed by a single flash of lightning. I may be also permitted to remark that in Unyoro and Uganda, countries which have a greater elevation than our own, the frequency of destructive lightning-strokes is much greater. Uganda is the only country boasting of a lightning-conductor. Mr. A. M. Mackay has erected one there, to protect King Mwanga's palace. Monbuttu, too, although having a lower elevation than Uganda and Unyoro, is celebrated for the frequency of accidents caused by lightning.

In more northern latitudes—namely, at Fashoda, Khartum, and Berber—destructive lightning-strokes are nearly unheard of, whilst in Sennar they are not altogether infrequent.

It is curious to notice that the Sudan Arabs have firmly conceived the idea that with every flash of lightning a piece of meteoric iron is thrown to the earth. They believe that whoever is able to secure such a piece of iron has gained a great treasure, because they think that swords and knives made out of it can never be surpassed in quality, and that their possession gives immunity from danger in battle, and affords protection against lightning-strokes. Sheik Nasr, who is the Chief of the Takkala Mountains, is said to have resisted all Egyptian attacks, and preserved his and his people's independence, on account of his possession of such a sword. Another superstition is, that fire kindled by a flash of lightning cannot be extinguished until a small quantity of milk has been poured over it. There seems to be some connection between these beliefs, inasmuch as water is believed to spoil meteoric iron: when working it, the blacksmith uses milk instead of water.

EMIN PASHA.

An "Instructive" Bibliography of the Foraminifera.

UNDER the title of "The (!) Bibliography of the Foraminifera," a remarkable production was published by Mr. Anthony Woodward in the *Fourteenth Annual Report of the Geographical and Natural History Survey of Minnesota* (St. Paul, 1886), and the work has lately been followed by a supplement—one of a series—in the *Journal of the New York Microscopical Society* for January 1888. Had the compiler not issued this supplement, previous criticisms might have sufficed, but, as he has again produced an extraordinary and untrustworthy list, it is incumbent on us to bring the work and its demerits before the notice of those who may be tempted to expect good results from using it.

"The" Bibliography, as it first appeared, occupied some 120 pages of the Minnesota Report, and it was hailed with some satisfaction. When put to the test, however, it was found to be absolutely untrustworthy—dates, pages, volumes, and other important details being incorrect. It therefore became necessary for the worker to see and examine every unknown or new entry, and to correct when necessary. The result fully justified this labour, for the whole thing was soon found to be comparatively useless in its uncorrected condition. To begin with, it was evident from the number and nature of the typographical errors that the proofs had not been corrected. Apart from this, however, there are more serious defects, for which excuse must be difficult. The compiler uses freely Mr. Brady's excellent bibliography appended to the *Challenger* Report on the Foraminifera, but he does more—he reproduces in his lists precisely the same printers' errors that appeared in Brady! It is therefore evident that the American compiler neither saw the books he entered in his lists, nor troubled to verify the entries. Here are a few examples:—

P. 251.—Terquem's papers on the Foraminifera of the Oolitic series. "Pt. 1, in the *Bulletin de la Soc. d'Histoire Nat. du Dép. de la Moselle*, 1868; the remainder published by the author." Some of these "remainder" were published in the *Mém. Ac. Imp. Metz*, and it is so stated on the separate copies.

P. 271.—Neugeboren, J. L. Bericht zu den in den Jahrg. LII. und LIII., &c. This is nonsense, it was a printer's error for I., II., and III. In the same entry Brady gives a wrong volume; Woodward does the same!

P. 224.—Reade, J. B. Mr. Brady gives a wrong date; Mr. Woodward copies him, and does the same.

The names of authors form another stumbling-block. Some of these are positively offensive.

P. 196.—Karrer, F., L. F. Pourtales, &c. Two papers, both on the same page in Brady. The paper quoted should read Pourtales, L. F.

P. 218.—King, Wm. This is true, but if the paper had been consulted, the compiler would have found that the Foraminifera were described by Jones, T. R., in King, Wm., &c.

P. 225.—Seguenza. This paper was written by Brady on Seguenza's genus!

P. 226.—Stewardson, G. This author is probably Dr. George Stewardson Brady, F.R.S.!

P. 227.—Thompson, W. Sir C. Wyville Thomson is confused with a somewhat obscure author of fifty years ago.

P. 266.—Jozsef-tol, S. Can one believe that this is the compiler's serious attempt at Dr. Jozsef Szabo, of Budapest!

In all these cases, and numerous others besides, it would have been to the compiler's credit if he had placed "[not seen]" at the end of his entry, for it is surely far worse to acknowledge acquaintance with papers, and to quote them thus carelessly, than to have never seen them at all.

The next offence is the frequent duplication of entries. The following examples will suffice:—

Pp. 228, 229.—Wallich on the Radiolaria as an order of the Protozoa. A reference to the *Pop. Sci. Rev.* would have shown this.

P. 244.—Leymerie, Mém. sur le terrain à Nummulites, &c. Again a reference would have shown these entries to be the same.

P. 268.—Koch, Ueber einigen, &c. The same remark applies to this as to the last, and indeed to all similar carelessnesses.

The quotation of *notices* of papers from various scientific journals, unaccompanied by references to the original places of

publication is a frequent and a grossly careless error, for in every case the information is forthcoming. Examples are :—

P. 225.—Siddall, NATURE, vol. xv.—instead of *Annual Report Chester Soc. Nat. Sci.*

P. 230.—Williamson, NATURE, vol. xvii.—instead of *Proc. Manchester Lit. Phil. Soc.*

P. 250.—Suess, *Quart. Journ. Geol. Soc.*, xxvi.—instead of *Verh. k. k. geol. Reichs.*

Moreover, the hopeless nature of his published errata may indicate that the author was somewhat ashamed of his work, and it is difficult to understand why the book was not stopped and reprinted, before it was allowed to pass into circulation.

Enough has now been said of the original work—that is, the first attempted list ; we will now pass on to the supplement I. In this, at least, we might have hoped that the compiler would have profited by experience, and used more care. There is certainly a difference in the proportion of typographical errors, but such details as volumes are still rather wild (*Bull. Soc. Géol. France*, for 1886, is quoted here and there as vol. x., xiv., &c.). We gather from the perusal of his supplement many things we could not understand in the original work. We recognize that the compiler is neither a born bibliographer, nor acquainted with scientific literature. We observe with satisfaction that the words "[not seen]" occur more frequently than in the earlier work, but can it be possible that the author has seen a copy of Silvestri's paper noted on p. 62? It is exceedingly rare, it does not exist in English libraries, and the writer of this has only seen two copies, both of which were sent to him from Italy. It would have been interesting to learn the pagination of so scarce a paper : the title as it at present stands is strongly suggestive of a bookseller's catalogue. And surely it was worth the compiler's while to quote Ehrenberg properly (p. 65) while the book was presumably lying open before him? The book also is known as "*Monatsbericht*" not "*Verhandlungen*," that is a secondary title. A very careless error is seen on p. 57, where *Orbitolina conoidea*, Alb., and *O. discoides*, Alb., are quoted. The original gives Albin Gras as the authority, whose paper on the subject, moreover, is well known. There should not have been confusion here. On pp. 64, 65, 71, 72, and 74, the same careless duplication of entries occurs as seen in the first attempted bibliography. But worse than all, perhaps, is the rendering of different versions of the title of one publication. A good instance of this is seen on pp. 66, 67, where six variants of *Verh. k. k. geol. Reichs.* are used, some (*Ver. K. K. Geol.*) being quite unintelligible to the uninitiated. On p. 72 we see two versions of *Ann. Soc. Belge Microsc.*, and only those familiar with the book would recognize readily "*Vierteljahrsschrift d. Zür. Natur. Gesellsch.*" (p. 74), with its chief word abbreviated. The compiler should remember that there is no necessity to quote, but, if he quotes, he should quote correctly.

It is needless to waste space on such clumsinesses as Prof. Wm. King, S.C.D. (? D.Sc.) (p. 1), or *Jahrbuch. Geol. Reicht.* It is also advisable to have some method even in printing. The compiler of this list uses roman and *italics* indiscriminately for titles of works (p. 71, Steinmann—where more prominence is thus given to the review than to the original work), while on p. 63, in the entry Alth, the word *Rozprawy* begins the title of the book, and has nothing whatever to do with the title of Dr. Alth's paper.

Many of these errors and defects might have been avoided had the compiler been accustomed to public libraries, or even endeavoured to find out the common books of reference, always at hand in these places. No bibliographer should ever think of working in scientific literature without his Carus and Englemann, his Scudder, and his Bolton, and for an American to omit to do so is sinful. No greater mistake was ever made by a writer than that made by the compiler, when he wrote in his preface that he had enjoyed facilities not enjoyed by many scientific students, those facilities afforded by the great public libraries of New York. We know what the resources of those libraries are, and the production which calls for this letter does not shake our faith in them. "Instructive" this bibliography certainly is, but not in the sense intended by its compiler.

CHAS. DAVIES SHERBORN.

Density and Specific Gravity.

MAY I ventilate a point in mechanical definition which has perplexed students within my experience—the use of the words density and specific gravity?

We are usually told that the quantity of matter in a body—as it is now called, the mass of the body—is proportional to the volume and density conjointly. This is Newton's definition of density (see also Thomson and Tait's "Natural Philosophy," § 208). Thus, if M be the mass, V the volume, and ρ the density of a body, we have—

$$M = \rho V \quad . \quad . \quad . \quad . \quad . \quad (1)$$

if the unit of mass be taken as the unit of volume of a substance of standard density.

Again, we are told that specific gravity is the ratio of the weight of the given body to the weight of an equal volume of some standard substance (Besant's "Hydrostatics and Hydrodynamics," § 13). Since weights are simply proportional to masses, it follows that the numerical values of specific gravities and densities are exactly the same. It would seem better, under these circumstances, to use one word only to express the one physical property. Accordingly, we find that specific gravity is disappearing from many of our best books (I think from Thomson and Tait's "Natural Philosophy," for example), though it still holds its place to puzzle students in examinations, and therefore teachers are compelled to make the best of it they can.

But this is not the whole evil. The definition of specific gravity is usually followed by the equation—

$$W = sV \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where W is the weight, s the specific gravity, and V the volume of the body. This equation is, no doubt, usually accompanied by the caution that the unit of weight chosen is not the unit of force proper to other dynamical equations, and for this reason the equation

$$W = \rho V g \quad . \quad . \quad . \quad . \quad . \quad (3)$$

is far to be preferred.

If equation (2) is of practical value, would it not be as well to define specific gravity in accordance with it, and say that specific gravity is the weight of unit volume of the substance? Thus, the specific gravity of water would be expressed by 62·5 lbs. avoirdupois in British units, or by 1 gramme in C.G.S. units. I believe this would have the advantage of conveying a perfectly definite idea to minds which dislike such abstractions as mass and density.

L. CUMMING.

Rugby, March 31.

"Coral Formations."

MR. MELLARD READE last week (April 5, p. 535) pointed out an error in my calculations which I had myself discovered when too late, and had intended to correct in sending you a further note on some experiments which are now in progress.

Mr. Reade seems to make use of my arithmetical blunder, and apparently attempts to discredit my experiments, and the new views as to coral-reef formations ; but I leave the matter to those who have a practical knowledge of the subject.

The corals experimented upon were of the class known as hard corals, and consequently the amount dissolved must be much smaller, I imagine, than that dissolved from the softer varieties, such as Porites. The first experiment (p. 462) gives the highest result, but I have no reason to doubt that the rate of solution deduced therefrom is far below that actually taking place in the tropical areas of the Pacific and Indian Oceans.

I do not consider that Mr. Reade has given an answer to Mr. Irvine's pertinent question, though he would have it appear that an answer is patent to everyone, and he must not take up your space with such a trivial matter.

Mr. Murray, speaking of his tow-net experiments in his Royal Institution lectures, says :—"I give this calculation more to indicate a method than to give even the roughest approximation to a rate of accumulation of deposits. The experiments were too few to warrant any definite deductions" ; and he is evidently satisfied that we have no knowledge, other than relative, as to the rate of accumulation of calcareous deposits.

It is at once evident to all who have used the tow-net, that Mr. Murray's experiments afford a very slender basis for calculations. Probably not more than one-fourth of the water in the track of the tow-nets actually passed through the nets, and not more than one-half of the organisms that entered them were retained ; the Coccospheres, Rhabdospheres, and small Foraminifera, for instance, passing through and escaping with the

water. Then, Mr. Reade supposes all the organisms in the bulk of water taken to die and fall to the bottom each day. Mr. Murray, in his calculations, supposes only one-sixteenth part to die each day. From the same data the former makes out a rate of accumulation of deposit of 1 inch in 29 years, the latter a rate of 1 inch in 470 years. Dana estimates the growth of a reef at not greater than one-sixteenth of an inch in one year, *i.e.* 1 inch in 16 years. Yet it will be admitted that a reef must grow much more rapidly than a deep-sea deposit. What then would justify us in accepting these figures as in any way representing what is now taking place in Nature? The fact is we much want definite information on the rate of growth of these calcareous deposits, and if Mr. Reade has the information his language would warrant, he should make it known for the benefit of science.

We know that these deposits do accumulate to hundreds of feet in thickness in some places, notwithstanding solution; and it seems to me that, as we can imitate in the laboratory the conditions of solution while we cannot those of secretion by organisms, then by experiments in this direction we may at least arrive at a knowledge of the minimum rate of accumulation of oceanic calcareous deposits. JAMES G. ROSS.

14 Argyll Place, Edinburgh, April 14.

Bernicle Geese on Coniston Lake.

THIS afternoon while walking by this lake I saw four large birds flying overhead. These birds, after making several circuits in the air, pitched on the lake. I had with me an excellent pair of field-glasses, and as I succeeded in approaching within 20 yards of them, I was enabled to examine them with sufficient accuracy to convince me that they were Bernicle geese (*Anser leucopsis*, Yarrell). What struck me as most worthy of remark was their extreme tameness, as they allowed me, first on land, and then in a boat, to approach within 20 yards of them. They were in excellent plumage, and seemed in good condition. After remaining about three hours swimming about on the lake, they rose, and after circling round once or twice, flew off in a northerly direction.

May I ask if this is a rare bird to see in the Lake District at this time of year? I have inquired in the neighbourhood, and do not think they could have come from any private water. Several people who have been here for many years assure me they have never seen this bird on the lake before, and this has certainly been my own experience. Is it possible their extreme tameness was due to fatigue? WILLIAM R. MELLY.

Tent Lodge, Coniston Lake, Lancashire, April 8.

The Muzzling of Oysters.

THIS practice, described in the current number of NATURE (p. 572) as owing "its existence to a careful study of the habits of the bivalve," is by no means new, though probably original on the part of the American naturalists. Our London fishmongers have muzzled oysters on a large scale from a time that is immemorial among them. Barrelled oysters are all very carefully muzzled, but without wires, as anybody may learn by watching an expert in the process of barrelling. It will be seen that he lays the oysters one by one carefully in tiers up to the top of the barrel, and then lays another tier rising *above* the level of the top. Having done this, he places the lid of the barrel on this exuberant tier, and thumps and rattles the barrel on a stone pavement or other solid ground until, by close packing of the whole, it descends to the level of the barrel top. The mass of oysters being thus compressed so as to render the slightest gaping of any one quite impossible, he firmly nails down the head of the barrel.

Experience has proved that oysters thus effectively muzzled may take long slow journeys (as they did in the old coaching days) and be kept fresh and without loss of flavour for two or three weeks, provided the barrels are unopened. If, however, they are loosely barrelled, a few days are too many. In some old country houses the barrels, unopened, were placed in salt water, and thus kept until required, but whether this was advantageous I cannot say.

W. MATTIEU WILLIAMS.

The Grange, Neasden, April 13.

SUGGESTIONS ON THE CLASSIFICATION OF THE VARIOUS SPECIES OF HEAVENLY BODIES.[1]

I.

I.—PROBABLE ORIGIN OF SOME OF THE GROUPS.

I. NEBULÆ.

IN a paper communicated to the Royal Society on November 15, 1887, I showed that the nebulæ are composed of sparse meteorites, the collisions of which bring about a rise of temperature sufficient to render luminous one of their chief constituents—magnesium. This conclusion was arrived at from the facts that the chief nebula lines are coincident in position with the fluting and lines visible in the bunsen burner when magnesium is introduced, and that the fluting is far brighter at that temperature than almost any other spectral line or fluting of any element whatever.

I suggested that the association or non-association of hydrogen lines with the lines due to the olivine constituents of the meteorites might be an indication of the greater or less sparseness of the swarm, the greatest sparseness being the condition defining fewest collisions, and therefore one least likely to show hydrogen. This suggestion was made because observations of comets and laboratory work have abundantly shown that great liability to collision in the one case, and increase of temperature in the other, are accompanied by the appearance of the carbon spectrum instead of the hydrogen spectrum.

The now demonstrated meteoric origin of these celestial bodies renders it needful to discuss the question in somewhat greater detail, with a view to classification; and to do this thoroughly it is requisite that we should study the rich store of facts which chiefly Sir William Herschel's labours have placed before us regarding the various forms of nebulæ, with the view of ascertaining what light, if any, the new view throws on their development.

To do this the treatment must be vastly different from that—the only one we can pursue—utilized in the case of the stars, the images of all, or nearly all, of which appear to us as points of light more or less minute, while, in the case of the nebulæ, forms of the most definite and, in many cases, of the most fantastic kind, have been long recognized as among their chief characteristics.

It will at once be evident that since the luminosity of the meteorites depends upon collisions, the light from them, and from the glow of the gases produced from them, can only come from those parts of a meteor-swarm in which collisions are going on. Visibility is not the only criterion of the existence of matter in space; dark bodies may exist in all parts of space, but visibility in any part of the heavens means, not only matter, but collisions, or the radiation of a mass of vapour produced at some time or other by collisions. The appearances which these bodies present to us may bear little relation to their actual form, but may represent merely surfaces, or loci of disturbances.

It seemed proper, then, that I should seek to determine whether the view I have put forward explains the phenomena as satisfactorily as they have been explained on the old ones, and whether, indeed, it can go further and make some points clear which before were dark.

To do this it is not necessary in the present paper to dwell at any great length either on those appearances which were termed *nebulosities* by Sir William Herschel or on irregular nebulæ generally; but it must be remarked that the very great extension of the former—which there is little reason to doubt will be vastly increased by increase of optical power and improvement in observing conditions and stations—may be held to strengthen the view that space is really a meteoritic plenum, while the forms indicate motions and crossings and interpenetra-

[1] The Bakerian Lecture, delivered at the Royal Society on April 12, by J. Norman Lockyer, F.R.S.

tions of streams or sheets, the brighter portions being due to a greater number of collisions per unit volume.

When we come to the more regular forms we find that they may be generalized into three groups, according as the formative action seems working towards a centre, round a centre in a plane, or nearly so, or in one direction only; as a result we have globular, spheroidal, and cometic nebulæ. I propose to deal with each in turn.

Globular Nebulæ.

The remarkable appearance presented by the so-called planetary nebulæ requires that I should refer to them in some detail. Sir William Herschel does not describe them at any great length, but in his paper on "Nebulous Stars" he alludes to the planetary nebulosity which in many cases is accompanied by a star in the centre, and finally comes to the conclusion that "the nebulosity about the star is not of a starry nature" (Phil. Trans., vol. lxxxi. p. 73, 1791).

Sir John Herschel, in his valuable memoir published in Phil. Trans., 1833, describes them as "hollow shells" (p. 500). It was so difficult to explain anything like their appearance by ordinary ideas of stellar condensation that Arago, as quoted by Nichol ("Architecture of the Heavens," p. 86), abandoning altogether the idea that they represented clusters of stars or partook in any wise of a stellar constitution, imagined them as hollow spherical envelopes, in substance cloudy and opaque, or rather semi-transparent; a brilliant body invisible in the centre illuminating this spherical film, so that it was made visible by virtue of light coming through it and scattered by reflection from its atoms or molecules. The mystery was explained to a certain extent by Lord Rosse, who (Phil. Trans., 1850, vol. cxl. p. 507) states that nearly all the planetary nebulæ which he had observed with his colossal instruments up to that time had been found to be perforated. In only one case was a perforation not detected, but in this ansæ were observed, introducing into the subject for the first time the idea of nebulous bodies resembling to a certain extent the planet Saturn. But Lord Rosse, although he thus disposed of the idea of Arago, still considered that the annular nebulæ were really hollow shells, the perforation indicating an apparently transparent centre.

Huggins and Miller subsequently suggested that the phenomena represented by the planetary nebulæ might be explained without reference to the supposition of a shell (or of a flat disk) if we consider them to be masses of glowing gas, the whole mass of the gas being incandescent, so that only a luminous surface would be visible (Phil. Trans., vol. cliv. p. 442, 1864).

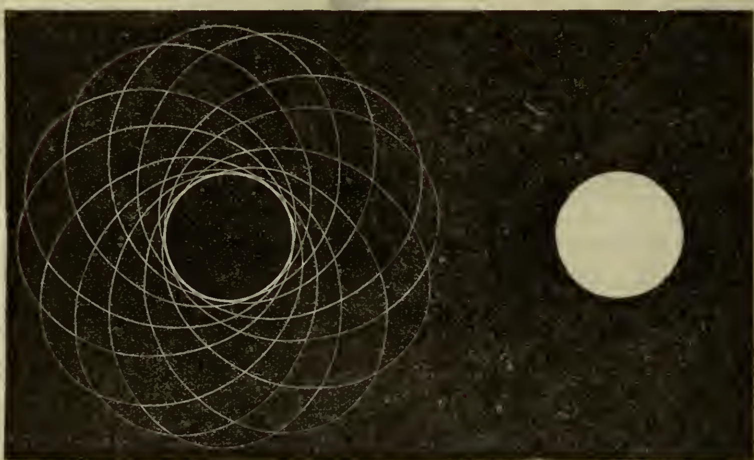


FIG. 1.—Suggested origin of the appearance presented by a planetary nebula. The luminosity is due to the collisions occurring along the sphere of intersection of the elliptic orbits of the meteorites. The left-hand diagram is a cross-section of the meteoric system, and the right-hand one shows the appearance of the collision shell as seen from a point outside.

It will be seen that all these hypotheses are mutually destructive; but it is right that I should state, in referring to the last one, that the demonstration that these bodies are not masses of glowing gas merely has been rendered possible by observations of spectra which were not available to Messrs. Huggins and Miller when their important discovery of the bright-line spectrum of nebulæ was given to the world.

It remains, then, to see whether the meteoritic hypothesis can explain these appearances when it is acknowledged that all the prior ones have broken down. Let us for the sake of the greatest simplicity consider a swarm of meteorites at rest, and then assume that others from without approach it from all directions, their previous paths being deflected. There will be at some distance from the centre of the swarm a region in which collisions will be most valid. Meteorites arrested here will begin to move in almost circular orbits round the common centre of gravity.

The major axes of these orbits may be assumed to be not very diverse, and we may further assume that, to begin with, one set will preponderate over the rest. Their elliptic paths may throw the periastron passage to a considerable distance from the common centre of gravity; and if we assume that the meteorites with this common mean distance are moving in all planes, and that some are direct and some retrograde, there will be a shell in which more collisions will take place than elsewhere. *Now, this collision surface will be practically the only thing visible, and will present to us the exact and hitherto unexplained appearance of a planetary nebula—a body of the same intensity of luminosity at its edge and centre—thus putting on an almost phosphorescent appearance.*

Such a collision surface, as I use the term, is presented to us during a meteoric display by the upper part of our atmosphere.

I append a diagram, Fig. 1, which shows how, if we thus assume movement round a common centre of gravity in a mass of meteorites, one of the conditions of movement being that the periastron distance shall be somewhat considerable, the mechanism which produces the appearance of a planetary nebula is at once made

apparent. The diagram shows the appearance on the supposition that the conditions of all the orbits with reference to the major axis shall be nearly identical, but the appearances would not be very greatly altered if we take the more probable case in which there will be plus and minus values.

Globular Nebulæ showing Condensations until finally a Nebulous Star is reached.

If we grant the initial condition of the formation of a collision-shell, we can not only explain the appearances put on by planetary nebulæ, but a continuation of the same line of thought readily explains those various other classes to which Herschel has referred, in which condensations are brought about, either by a gradual condensation towards the centre, or by what may be termed successive jumps, showing that they are among the earliest stages of nebular development.

To explain these forms we have only to consider what will happen to the meteorites which undergo collision in the first shell. They will necessarily start in new orbits,

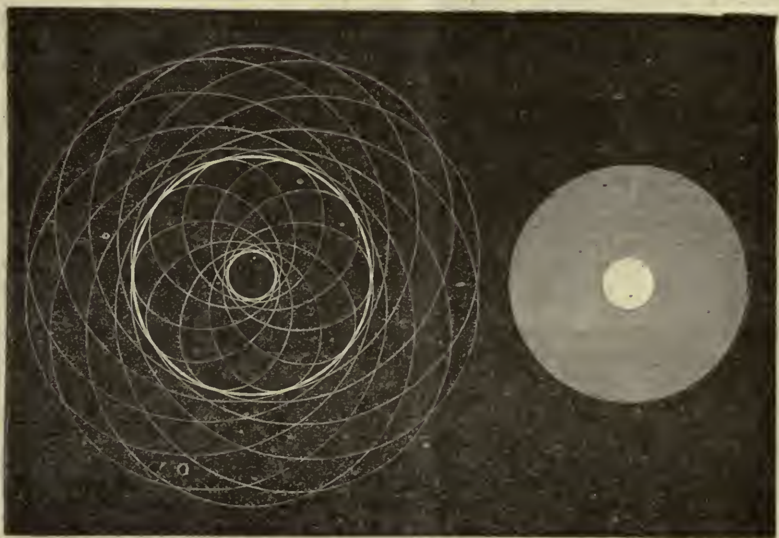


Fig. 2.—Suggestion as to the origin of a globular nebula with a brighter central portion. As in the former case, the luminosity of the fainter portion is due to the collisions which occur along the sphere of intersection represented by the larger circle. After collision the meteorites will travel in new orbits, and there will be an additional sphere of intersection, represented by the smaller circle. The left-hand diagram is a cross-section, and the right-hand one represents the appearance of the two collision shells as seen from a point outside.

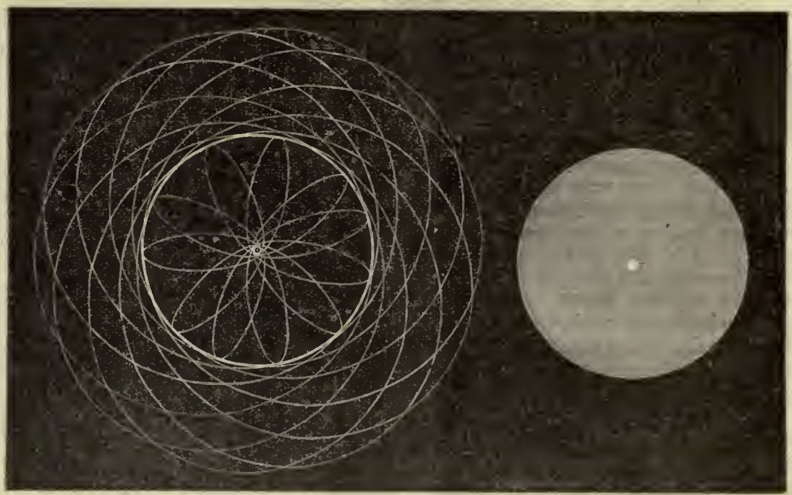


Fig. 3.—Suggestion as to the origin of a nebulous star. The orbits of the inner set of meteorites are very elliptic, so that the shell of intersection appears almost as a point. As in the previous cases, the left-hand diagram represents the meteoric systems in section, and the right-hand one the appearance from a point outside.

and it is suggested that an interior collision-shell will in this way be formed.

In consequence of the collisions the orbits will have a tendency to get more and more elliptic, while the pericentric distance will at the same time be reduced; the swarm will, in consequence of this action, gradually brighten towards the centre through collisions being possible nearer the centre, and ultimately we shall have nebulæ with a distinct nucleus, the nucleus then representing the *locus* of most collisions. This brightness may be sudden in places, or quite gradual, according to the collision conditions in each swarm. The final stage will be a nebulous star.

Effects of Subsequent Rotation.—Spheroidal Nebulæ.

In such meteor-swarms as those we have considered, it must be that rotation is sooner or later set up. Otherwise it would be impossible to account for the spheroidal

nebulæ at all. I am aware that in Newton's opinion the cause of this rotation was not mechanical, but the moment we assume a meteoric origin of these globular clusters it is straining the facts to assume that the intake will be exactly the same at all points, and the moment the bombardment is more or less localized, rotation must follow sooner or later. Sir William Herschel, in his paper of 1811 (p. 319), says: "If we consider this matter in a general light, it appears that every figure which is not already globular must have eccentric nebulous matter, which, in its endeavour to come to the centre, will either dislodge some nebulosity which is already deposited, or slide upon it sideways, and in both cases produce a circular motion; so that, in fact, we can hardly suppose a possible production of a globular form without a subsequent revolution of nebulous matter, which in the end may settle in a regular rotation about some fixed axis."

Given, then, a globular swarm with a rotation around an axis, we have to discuss the phenomena produced by collisions under a new set of circumstances.

Here at once we have to account for the fact that the nearly spherical forms are very short-lived, for they are very rare; we seem to jump, as it were, from globes to very extended spheroids.

If it be conceded that from the above considerations we are justified in supposing that the elliptic and other spheroidal nebulæ really represent a higher stage of evolution than those presented to us in the globular form, it is clear that on the meteoritic hypothesis the greater part of the phenomena will represent to us what happens to such a system under the condition of a continuous bombardment of meteorites from without.

So soon as we have a minor axis, there will at first be more collisions parallel to it; the result of this will be that the equatorial plane will be intensified, and then, later on, if we conceive the system as a very extended spheroid, it is obvious that meteorites approaching it in directions parallel to its minor axis will now have fewer chances of collisions than those which approach it, from whatever azimuth, in what we may term the equatorial plane. These evidently, at all events if they enter the system in any quantity, will do for the equatorial plane exactly what their fellows were supposed to do for the section in Fig. 1, and we shall have on the general background of the symmetrically rotating nebula, which may almost be invisible in consequence of its constituent meteorites all travelling the same way and with nearly equal velocities, curves indicating the regions along which the entrance of the new swarm is interfering with the movements of the old one; if they enter in excess from any direction, we shall have broken rings or spirals.

This was suggested in my last paper. Various rings will indicate the regions where most collisions are possible, and the absence of luminosity in the centre by no means demonstrates the absence of meteorites there.

Researches by Lord Rosse and others have given us forms of nebulæ which may be termed sigmoid and Saturnine, and these suggest that they and the elliptical nebulæ themselves are really produced by the rotation of what was at first a globular rotating swarm of meteorites, and that in these later revelations we pick up those forms which are produced by the continued flattening of the sphere into a spheroid under the meteoric conditions stated. It is worthy of remark that all the forms taken on by the so-called elliptic nebulæ described by the two Herschels, and by the spiral, sigmoid, and Saturnine forms which have been added to them by the labours of Lord Rosse and others, are recalled in the most striking manner by the ball of oil in Plateau's experiment, when rotations of different velocities are imparted to it. It is my intention to repeat Plateau's experiments, and to take instantaneous photographs of the various phenomena presented, and to place them side by side with the drawings of nebulæ, of which they are almost the exact counterparts.

The Saturnine form may, indeed, in some cases represent either the first or last stages in this period of the evolutionary process. I say *may* represent, in consequence of the extreme difficulty in making the observations, so that in the early stages a spherical nebula, beginning to change into a spheroid, may have its real spheroidal figure cloaked by various conditions of illumination.

The true Saturnine form must, as in the case of Saturn itself, represent one of the latest forms in the meteor-swarm, because, if it be not continually fed from without, collisions must sooner or later bring all the members of the swarm to the centre of figure.

Cometic Nebulæ.

I do not know that any explanation has, so far, been suggested as to the origin of these curious forms, which were first figured by Sir William Herschel, and of which a number have recently been observed in the southern hemisphere ("Melbourne Observations"). It is clear that in them the conditions are widely different from those hitherto considered in this paper. I think that the meteoritic hypothesis satisfactorily explains them, on the supposition that we have either a very condensed swarm moving at a very high velocity through a sheet of meteorites at rest, or the swarm at rest surrounded by a sheet all moving in the same direction. It is a question of relative velocity.

If we consider the former case, it is clear that the collision region will be in the rear of the swarm, that the collision will be due to the convergence of the members of the sheet due to the gravity of the swarm, and that the collision region will spread out like a fan behind the swarm.

The angle of the fan, and the distance to which the collisions are valid, will depend upon the velocity of the condensed swarm.

Nebulous Origin of some Bodies which appear as Stars.

From this point of view it is also possible that many stars, instead of being true condensed swarms due to the nebulous development to which we have referred, are simply appearances produced by the intersection of streams of meteorites. They are, then, simply produced by an intensification of the conditions which gave rise to the brighter appearances recorded by Herschel here and there in his diffused nebulosities. The nebulous appendages sometimes seen in connection with stars strengthen this view.

II. STARS WITH BRIGHT LINES OR FLUTINGS.

I pointed out in my last paper that those stars in the spectra of which bright lines had been observed were in all probability the first result of nebulous condensation, both their continuous spectrum and that of the surrounding vapour being produced by a slightly higher temperature than that observed in nebulæ in which similar though not identical phenomena are observed.

I have recently continued my inquiries on this point: and I may say that all I have recently learned has confirmed the conclusions I drew in my last paper, while many of the difficulties have disappeared. Before I refer to these inquiries, however, it is necessary to clear the ground by referring to the old view regarding the origin of bright lines in stellar spectra, and to the question of hydrogen.

Reference to the Old View by which it was supposed some of the Bright-line Phenomena might be accounted for.

In the views which, some years ago, were advanced by myself and others, to account for the bright lines seen

in some of the "stars" to which reference has been made, the analogy on which they were based was founded on solar phenomena; the "stars" in question being supposed to be represented in structure by our central luminary. The main constituent of the solar atmosphere outside the photosphere is hydrogen, and it was precisely this substance which was chiefly revealed by these stellar observations and in the Novas, in which cases it was sometimes predominant. A tremendous development of an atmosphere like that of the sun seemed to supply the explanation of the phenomena.

Acting on this view in 1878,[1] I attempted to catch these chromospheric lines in *α* Lyræ, abandoning the use of a cylindrical lens in front of the slit with this object in view.

Further, it was quite clear that if such gigantic supra-photospheric atmospheres existed, their bright lines might much modify their real absorption-spectra; even "worlds without hydrogen" might be thus explained without supposing a *lusus naturæ*, and so I explained them.

That this view is untenable, as I now believe, and that it is unnecessary, will, I think, be seen from what follows. A long series of newly described phenomena, which are absolutely incomprehensible while it is applied to them, find, I think, a simple and sufficient explanation. I must hold that the view is untenable, because how a body constituted in any way like the sun could change its magnitude from the thirteenth to the sixth every year or so, or change its hydrogen lines from bright to dark once a week, passes comprehension; and the more closely a "star" resembles the sun the less likely are such changes to happen. Even the minor evolutionary changes are inexplicable on this hypothesis, chiefly because in a completely condensed mass the temperature must be very high and constant, while I have shown that the spectrocopic phenomena are those of a specially low temperature; and I may now add that many of the objects are extremely variable in the quantity and quality of the light they emit.

Another cause of the appearance of the hydrogen lines has been suggested by Mr. Johnstone Stoney (Proc. Roy. Soc., vol. xvii. p. 54). He considers it due to the clashing together of the atmospheres of two stars, the outer constituent of the atmosphere—hydrogen—alone being raised by the friction to brilliant incandescence.

Another objection we can urge against the old view is that all bodies in the universe cannot be finished suns in the ordinary sense, and that it leaves out of account all possible processes of manufacture, not only of single stars, but of double and multiple systems, at all stages between nebula and sun; while the new one, by simply changing the unit from the star to each individual constituent, it is hardly too much to say, explains everything, though it is perfectly true that in some of the steps a considerable acquaintance with spectroscopic phenomena is necessary to realize the beauty and the stringency of the solutions.

[1] ". . . The sun which we see, the sun which sends us the majority of the light we receive, is but a small kernel in a gigantic nut, so that the diameter of the real sun may be, say, two million miles. Suppose then that some stars have very large coronal atmospheres; if the area of the coronal atmosphere is small compared with the area of the section of the true disk of the sun, of course we shall get an ordinary spectrum of the star; that is to say, we shall get the indications of absorption which make us class the stars apart; we shall get a continuous spectrum barred by dark lines. But suppose that the area of the coronal atmosphere is something very considerable indeed, let us assume that it has an area, say fifty times greater than the section of the kernel of the star itself; now, although each unit of surface of that coronal atmosphere may be much less luminous than an equal unit of surface of the true star at the centre, yet, if the area be very large, the spectroscopic writing of that large area will become visible side by side with the dark lines due to the brilliant region in the centre where we can study absorption; other lines (bright ones) proceeding from the exterior portion of that star will be visible in the spectrum of the apparent *point* we call a star. Now it is difficult to say whether such a body as that is a star or a nebula. We may look upon it as a nebula in a certain stage of condensation; we may look upon it as a star at a certain stage of growth."—Proc. R.S. 1878, No. 185, p. 49.

The Question of Hydrogen in the Case of Bright-Line Stars.

It may be convenient also that I should summarize the various conditions under which the lines of hydrogen are observed in the meteorite swarms we are now considering.

In the "nebulæ" we begin with the widest interspaces. Future investigation may show that, as I have suggested, those in which the hydrogen lines are absent are the most widely spaced of all. Be this as it may, it is a matter of common knowledge that with the brighter nebulæ, such as that of Orion, to take an instance, we have hydrogen associated with the low-temperature radiation of olivine. That the hydrogen is electrically excited to produce this glow is proved by the fact that the temperature of the meteorites themselves must be very low; otherwise the magnesium would not show itself without the manganese and iron constituents, and the continuous spectrum would be much brighter and longer than it is.

In the former paper I showed that in my laboratory experiments, when the pressure was slightly increased in a tube containing gases obtained from meteorites, the carbon bands began to be visible. We should expect this to happen therefore in a meteor swarm at some point at which the mean interstitial space was smaller than that accompanied by the appearance of the hydrogen lines; and it would be natural that both should be seen together at an early stage and both feeble, by which I mean not strongly developed, as hydrogen is not strongly developed even in the nebula of Orion, none of the ultra-violet lines being visible in a photograph, while the magnesium line is.

The association of the low-temperature lines of hydrogen with the flutings of carbon is therefore to be expected, and I shall subsequently show that we have such an association in the so-called bright-line stars; and even at a further stage of development, in stars like *α* Orionis, the hydrogen is still associated with the carbon.

The Cometic Nature of Stars with Bright Lines in their Spectra.

Seeing that the hypothesis I am working on demands that the luminosity in stars and the bright lines in their spectra are produced by the collisions of meteorites, the spectra of those bodies must in part resemble those of comets, in which bodies by common consent the luminosity is now acknowledged to be produced by collisions of meteorites.

We must, however, first consider the vast difference in the way in which the phenomena of distant and near meteoric groups are necessarily presented to us; and, further, we must bear in mind that in the case of comets, however it may arise, there is an action which drives the vapours produced by impacts outward from the swarm in a direction opposite to that of the sun.

It must be a very small comet which, when examined spectroscopically in the usual manner, does not in consequence of the size of the image on the slit enable us to differentiate between the spectra of the nucleus and envelopes. The spectrum of the latter is usually so obvious, and the importance of observing it so great, that the details of the continuous spectrum of the nucleus, however bright it may be, are almost overlooked.

A moment's consideration, however, will show that if the same comet were so far away that its whole image would be reduced to a point on the slit-plate of the instrument, the differentiation of the spectra would be lost; we should have an integrated spectrum in which the brightest edges of the carbon bands, or some of them, would or would not be seen superposed on a continuous spectrum.

The conditions of observations of comets and stars being so different, any comparison is really very difficult; but the best way of proceeding is to begin with the spectrum of comets in which, in most cases, for the reason given, the phenomena are much more easily and accurately recorded.

But even in the nucleus of a comet as in a star it is much more easy to be certain of the existence of bright lines than to record their exact positions,[1] and as a matter of fact bright lines have been recorded, notably in Comet Wells and in the great comet of 1882.

The main conclusion to which my researches have led me is that the stars now under consideration are almost identical in constitution with comets between that condition in which, as in those of 1866 and 1867, they give us the absolute spectrum of a nebula and that put on by the great comet of 1882.

I am aware that this conclusion is a startling one, but a little consideration will show its high probability, and a summary of all the facts proves it, I think, beyond all question.

While we have bright lines in comets, it can be shown that some of them are the remnants of flutings. Thus in Comet III. of 1881, as the carbon lines died away the chief manganese fluting at 558 became conspicuously visible; it had really been recorded before then. The individual observations are being mapped in order that the exact facts may be shown. It may probably be asked how it happened that the fluting of magnesium at 500 was not also visible. Its absence, however, can be accounted for: it was *masked* by the brightest carbon fluting at 517, whereas the carbon fluting which under other circumstances might mask the manganese fluting at 558 is always among the last to appear very bright and the first to disappear.

In the great comet of 1882, which was most carefully mapped by Copeland, very many lines were seen, and indeed many were recorded, and it looks as if a complete study of this map will put us in possession of many of the lines recorded by Sherman in the spectrum of γ Cassiopeiæ. We have then three marked species of non-revolving swarms going on all fours with three marked species of revolving ones, and in this we have an additional argument for the fact that the absence in the former of certain flutings which we should expect to find may have their absence attributed to masking by the carbon flutings.

We have next, then, to show that there are carbon bands in the bright-line stars.

There is evidence of this. Among the bright lines recorded is the brightest carbon fluting at 517. This is associated with those lines of magnesium and manganese and iron visible at a low temperature which have been seen in comets.

But we have still more evidence of the existence of carbon. In a whole group of bright-line stars there is a bright band recorded at about 470, while, less refrangible than it, there appears a broad absorption band. I regard it as extremely probable that we have here the bright carbon band 467–474, and that the appearance of an absorption band is due to the fact that the continuous spectrum of the meteorites extends only a short distance into the blue.

If we consider such a body as Wells's comet, or the great comet of 1882, as so great a distance from us that only an integrated spectrum would reach us, in these cases the spectrum would appear to extend very far, and more or less continuously, into the blue; but this appearance would be brought about, not by the continuous spectra of the meteorites themselves, but by the addition of the hydrocarbon fluting at 431 to the other hot and cold carbon bands in that part of the spectrum.

There are other grounds which may be brought forward to demonstrate that the difference between comets and the stars now under discussion is more instrumental than physical.

Supposing that the cometic nature of these bodies be conceded, the laboratory work will show us which flutings and lines will be added to the nebula spectrum upon each rise of temperature; and the discussion, so far as it has gone, seems to show that such lines and flutings have actually been observed.

The difficulties of the stellar observations must always be borne in mind. It will also be abundantly clear that a bright fluting added to a continuous spectrum may produce the idea of a bright line at the sharpest edge to one observer, while to another the same edge will appear to be preceded by an absorption band.

III. Stars with Bright Flutings accompanied by Dark Flutings.

I also showed in the paper to which reference has been made that the so-called "stars" of Class III.*a* of Vogel's classification are not masses of vapour like our sun, but really swarms of meteorites; the spectrum being a compound one, due to the radiation of vapour in the interspaces and the absorption of the light of the red- or white-hot meteorites by vapours volatilized out of them by the heat produced by collisions. The radiation is that of carbon vapour, and some of the absorption, I stated, was produced by the chief flutings of manganese.

These conclusions were arrived at by comparing the wave-lengths of the details of spectra recorded in my former paper with those of the bands given by Dunér in his admirable observations on these bodies.[1]

The discovery of the cometic nature of the bright-line stars greatly strengthens the view I then put forward, not only with regard to the presence of the bright flutings of carbon, but with regard to the actual chemical substances driven into vapour. From the planetary nebulæ there is an undoubted orderly sequence of phenomena through the bright-line stars to those now under consideration, if successive stages of condensation are conceded.

I shall return to these bodies at a later part of this memoir.

IV. Stars in which Absorption Phenomena predominate.

I do not suppose that there will be any difficulty in recognizing, that if the nebulæ, stars with bright lines, and stars of the present Class III.*a* are constituted as I state them, all the bodies more closely resembling the sun in structure, as well as those more cooled down, must find places on a temperature curve pretty much as I have placed them; the origin of these groups being, first still further condensation, then the condition of maximum temperature, and then the formation of a photosphere and crust.

We shall be in a better position to discuss these later stages when the classifications hitherto adopted have been considered.

(*To be continued.*)

THE HITTITES, WITH SPECIAL REFERENCE TO VERY RECENT DISCOVERIES.[2]

IV.

THOSE who have attempted to decipher the Hittite inscriptions have not always regarded a fact which may be discerned with tolerable facility. The inscriptions from Hamath, and those from Jerablûs or Carchemish, though no doubt deriving their origin from a common source, yet present, as we know them, two distinct types. Symbols usual and frequently repeated on the Jerablûs monuments are wholly absent from those of

[1] "*Observations of Comet III.*, 1881, *June* 25.—The spectrum of the nucleus is continuous; that of the coma shows the usual bands. With a narrow slit there are indications of many lines just beyond the verge of distinct visibility."—Copeland, *Copernicus*, vol. ii. p. 226.

[1] "Les Étoiles à spectres de la troisième classe," *Kongl. Svenska Vitenskaps-Akademiens Handlingar*, Bandet 21, No. 2, 1885.

[2] Based on Lectures delivered by Mr. Thomas Tyler at the British Museum in January 1888. Continued from p. 562.

Hamath. Other symbols, not difficult to identify as essentially the same, yet assume a form more or less changed. The difference is altogether so considerable that in ancient times the ability to read and fully understand the one type may quite possibly not have involved a facility of perfectly comprehending the other. The difference might be spoken of as one of *dialect*, if that word could be, in this case, appropriately employed. Then, so far as the more considerable monuments in the Museum from Jerablûs or Carchemish are concerned, there is clearly between them a difference in age, and the difference may possibly be very great. As evidence in support of this assertion, I may adduce a symbol which was intended apparently to denote an agricultural implement. When this symbol was given as in Fig. M (1), though probably drawn out of perspective and perhaps already somewhat conventionalized, yet its relation to the actual object would seem to have been not very distant. But when the symbol has become changed in the manner that appears

1 2 3

Fig. M.—Symbols of agricultural implements: 1 and 2, from Jerablûs monuments; 3, from incised bowl.

in (2), there is no difficulty in recognizing that a considerable interval must have elapsed. In (3), on an incised bowl, at present deposited in the British Museum, the same symbol has assumed something of a hieratic form. Though the bowl was found at the site of Babylon, the inscription cut into it obviously belongs to the Carchemish type. Possibly the bowl had been brought from Carchemish as a trophy.

It is conceivable that (1) might denote a kind of harrow, but more probably the vertical portion represents the end of a threshing-sledge, with teeth of stone or iron projecting therefrom. It appears to me very doubtful whether this symbol (which is not found on the inscriptions from Hamath) is ever used with any direct reference to agricultural operations. It is rather to be understood figuratively of severity in warfare and of the devastation of an enemy's country. This is in accordance with the usage of the Biblical books, which, on account of local contiguity, have, in relation to the Hittite inscriptions, peculiar importance. Thus we find, in Amos i. 3, "For three transgressions of Damascus, and for four, I will not turn away [the punishment] thereof; because they have threshed Gilead with threshing-instruments of iron"; and there are other similar passages. Moreover, such metaphorical or figurative employment of material symbols is in accordance with what we know of the use of picture-writing by the American Indians. I ought, perhaps, to add that on the Carchemish inscriptions the threshing-sledge is usually accompanied by what is probably the representation of the more essential parts of a plough somewhat conventionalized. Between the pole (or handle) and the

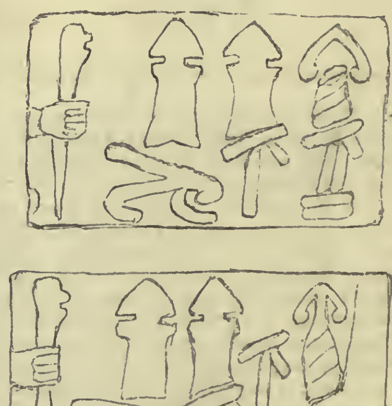
Fig. N.—Probable symbol of plough.

share or tooth, wedges would seem to have been inserted to keep the tooth firm in its place. By an easy metonymy a plough would denote land tilled and cultivated. Fig. N gives this symbol as accompanying Fig. M (2).

The difficulty of explaining the characters of the Hittite inscriptions may result in part from the objects originally depicted being such as are no longer known to us. But probably a much more serious cause of difficulty is to be found in conventionalization and the changes made to facilitate rapid execution. And we must take into account, in addition, the necessity which would arise in some cases for the lateral compression of the representation, if I may so speak, in order that the symbol might be conveniently given in the same line and in association with other symbols. This last remark applies particularly to a symbol which, there is strong reason to believe, represents the *shadoof*, or instrument for raising water, still used in the East. It would have been inconvenient to represent at full length the lever at top, with a weight at one end, and a bucket, suspended by a cord or

Fig. O.—*Shadoof* symbol, from Jerablûs inscriptions.

chain, from the other. Consequently we have the instrument represented with modification, and with the lever shortened. Here again in all probability the symbol is used for the most part figuratively, and not in general with reference to the raising of water or the irrigation of land. People familiar with the swinging up and down of the lever, and of bringing up the bucket of water, might use the symbol of "raising" in a wider sense, or generally of active and efficient operation. It is probably with this latter meaning that it is employed in three out of the five Hamath inscriptions, and in a combination of symbols which is exceedingly interesting and instructive. Two of the three are represented in Fig. P. As to the general subject, the presence of the hand grasping warlike weapons can scarcely leave a doubt; and in accordance with this indication is the spear-head, however ornamented, at the other end of the figure. The two triangular-topped symbols between, probably denote actual conflict. The idea represented conventionally



1

2

Fig. P.—Groups of symbols ending two Hamath inscriptions.

may be that of a mass of warriors who have closed together in deadly combat, or a mass of spears seen together. Under the first of these triangular-topped figures is a symbol which has been supposed to represent an insect. The two symbols together may be taken as meaning "war commencing." In the second place, we have a combination with the *shadoof*, and we may interpret,

"war in active operation." In the third combination the change in both the symbols is to be noted. That at the top may refer to a custom of enwreathing or adorning the arms of warriors to denote success in war, and to celebrate victory; and the change in the *shadoof* corresponds therewith. In (1) the vertical bar is doubled, and two short horizontal bars are added beneath; in (2) we may take it that the same end is attained by lengthening the vertical bar, while, as before, the two short horizontal bars are added, and the ornamentation of the spear is lengthened. This third combination manifestly marks the climax; but it can only indicate this, if, in accordance with what I have already said, the inscriptions are to be read "with the faces." And very important additional evidence is also furnished by these groups as to the ideographic character of the inscriptions.

That a comparatively primitive people, employing the *shadoof*, the plough, and the threshing-sledge, should use figures of these instruments to represent ideas more or less abstract can scarcely excite surprise. Probably, too, a paucity of symbols might lead to those employed being used to denote a plurality of somewhat diverse significations.

A symbol, with regard to the meaning of which the evidence is especially clear, is the symbol of deity or divinity on the Jerablûs monuments. This symbol consists of a straight stroke and a crescent, denoting in all probability Asherah, "the straight," and the goddess

Fig. Q.—The symbol of deity, with various figures on Jerablûs monuments.

Ashtoreth. Such a combination would accord with the close relation between Asherah and Ashtoreth in the Old Testament.[1] But, whether this explanation is admitted or not, that the symbol denotes deity or sacredness can scarcely admit of question. In the first place the symbol occurs invariably *at the top of the line* on the Jerablûs monuments. This fact is itself significant. Then, three times on the "doorway inscription" what are evidently hands, though somewhat conventionalized, are held up towards the symbol in worship, as with the palm upward (1), according to the widely-spread custom, and also, as it would seem, in giving thanks (Fig. T). The symbol is to be seen also above a sacred tree (3), and above a rudely-shaped idol (2), from a fragment found at Jerablûs. This idol may have been a *lusus naturæ*, presenting a distant resemblance to the human face, and but slightly modified by art. And on the rounded pillar from Jerablûs, which bears the most modern, comparatively, of the three considerable inscriptions obtained from this site, we find the same symbol over very curious figures which, as it seems to me, were intended to represent spiritual beings or disembodied souls (4). They are insubstantial εἴδωλα, mere masks as it would appear, and with tail-like prolongations instead of bodies. They are horned, however, and the horn was a sign of dignity and power. On the whole, the evidence of the value of the straight stroke and crescent is, in my judgment, entirely conclusive. I ought to add that this symbol is not found on the inscriptions from Hamath; and thus in all probability is indicated a difference of religious cult.[1]

[1] Asherah was probably a phallic symbol. This accords with the view of Movers ("Die Phönizier," vol. i. p. 560 *sqq.*), and with that of the Rabbins (cf. I. Kings xv. 13, and the commentaries thereon).

An interesting question presents itself as to whether the names of Hamath and Carchemish can be detected on the inscriptions. In reply it may be stated that the name or symbol of the ancient city on the site of Jerablûs may be pointed out with a good deal of confidence.

Fig. R—Name of ancient city on Jerablûs monument.

The oval symbol, which appears at the top in Fig. R, in its origin was intended, no doubt, as a plan of a city. A similar oval form, both of the military camp and of the city (Layard's "Monuments of Nineveh," pl. 77), is to be seen depicted on the Assyrian monuments. And, with regard to the Hittite symbol, it is also worthy of note that not only on the Egyptian monuments is there an analogous circular symbol of "city" or "place," but that a similar symbol, with the like meaning, was found in Mexico, both of circular form, and, as it would seem, also oval. For the latter see Brasseur de Bourgbourg, "Études sur le Système graphique et la Langue des Mayas," Paris, 1869, vol. i. p. 150. From the Assyrian monuments it appears that fortresses were not uncommonly of angular and quadrilateral form. I therefore take the lozenge-shaped figure to denote the idea of "fortress." Like the "city" symbol it has what we may regard as a road or street crossing it; and it has markings indicating, in all probability, gates, at the other corners. Then, as to the eagle, a question of great interest, if of some difficulty, presents itself. The ancient city on the site of Jerablûs we have identified with Carchemish. As already stated, the name Carchemish has been looked upon as denoting "the fortress of Chemosh." The question then occurs, If the lozenge-shaped figure denotes "fortress," does the eagle denote Chemosh? Whatever may be the etymology of "Chemosh," it is sufficiently probable that Chemosh was, like Baal and Moloch, a solar deity. This, indeed, has been previously suggested. And the sun might very well be represented by the eagle, the bird of the sun. Moreover such a view is not purely hypothetical. As is well known, in ancient Egypt, Horus, the god of the rising sun, was represented by the hawk. Then there is reason to think that, in connection with the solar cult, the eagle was worshipped or regarded as a sacred bird at places in or near the Hittite country, and not very far distant from Carchemish.

Looking, then, upon the eagle and upon the second part of the name "Car-chemish" as both representing Chemosh, there remains no difficulty about the first part of the name, as we find, in Assyrian, *caru*, a fortress—a word found also, with comparatively slight modification, in Hebrew (*kir*).

[1] There is another sign, **IL**, which, though less frequent, yet appears as if a variant of the sign of deity usual in inscriptions of the Jerablûs type. This sign somewhat puzzled me till, on the coinage of Mallus, in Cilicia, I found the right angle together with the straight stroke, or *asherah*, the equilateral triangle, and the cone. All these were, no doubt, connected with the goddess Astarte, to whose service Mallus seems to have been especially devoted. It is, in all probability, this deity who appears in winged form on the obverse of the coin, which Mr. Barclay V. Head, the eminent numismatist, assigns to a date earlier than 400 B.C. I ought to add that the sign with the right angle, which probably denotes a different aspect or function of the goddess, occurs apparently in the Hamath inscriptions

With regard to Hamath, though the evidence is weaker, yet probably the city is indicated by a symbol consisting of the vase or receptacle (Fig. S, 1), with the oval character "city" above and the feminine sign below. The word "Hamath" comes very near to one used in Hebrew for a bottle or bulging receptacle.

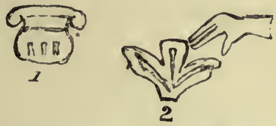


FIG. S.—Symbols on "doorway inscription" from Jerablûs: 1, vase or receptacle; 2, hand seizing vegetation.

The receptacle in the figure, having three vertical marks, and one or two horizontal marks, is a common symbol on the Jerablûs monuments. Probably, as in the bag previously spoken of (and see Fig. T), the three vertical marks denote objects within the receptacle; we may suppose, pieces of metal used as uncoined money. It seems most likely, however, that the difference in external shape of the receptacles indicates a difference in the nature and value of the contents. The symbol of seizing vegetation is another example of the use of ideograph or picture-writing in these inscriptions. That the thing seized is a plant or herb is sufficiently obvious. And from the accompanying symbols there is reason to think that one of the food-producing cereals, when ripe, is intended.

Treated in accordance with the principles which have guided us, and the conclusions previously expressed, the group of symbols concluding the "doorway inscription" in the British Museum will be found to yield probable and consistent results. Beginning from the reader's left, we have a symbol which, probably deriving its origin from the chase, bears some resemblance to the leg of an animal repeated, but inverted. The inverted position would appropriately represent the total defeat of an enemy,

FIG. T.—End of "doorway inscription" from Jerablûs, in the British Museum.

while the repetition or doubling may be regarded as implying plurality, and perhaps flight. Then follow symbols denoting probably repeated thanksgivings to the gods (notice the doubling of the sign of deity). Next comes the bag of treasure with the hand beneath pointing towards the king. Under the king's head is a hand in the attitude of acceptance. Here is essentially what we find on the Yuzgât seal, but the object being accepted is not identical with the bag behind the king. Perhaps it denotes the tribute the payment of which was imposed on the conquered people.[1] Then follows the *shadoof* symbol, which here may well imply the vigorous prosecution of agriculture on the restoration of peace. At the end is the plant with four strokes above it, which may be regarded as signifying that the earth brought forth abundantly, or fourfold.[2] But whether the interpretation I have thus given is accepted or not, the ideographic character of the group is altogether unmistakable.

Allusion was previously made to the name "Zu-zu," or "Su-su" (see p. 539, note) as possibly occurring on the shortest (excepting mere fragments) of the inscriptions from Jerablûs in the Museum. In the first line of the inscription is the most important of the places where the name would be thus read in accordance with the conclusions arrived at with regard to the Tarkutimme inscription. In the group there are two smaller cones and one larger, all crossed by horizontal lines. The two smaller cones will represent, as on the Tarkutimme inscription, a people or nation. This coincidence with the Tarkutimme inscription may give credibility to the supposition of still further agreement. The animal's head resting on the double cone will denote the name of the people. The taller cone would probably denote a king or possibly kings, crossed as it is by horizontal lines. A curve passes from the top down near the side of the taller cone, and above are the two strokes repeated and placed at an angle, which would be read "Zu-zu" or

FIG. U.—Group of symbols from Jerablûs monument in the British Museum.

"Su-su." Connected with the curve is an appendage passing to the head of an animal. Across this appendage (and the curve also after the two have become united) pass horizontal lines, probably lines of plurality. The animal's head, with the appendage, may give the name of a royal dynasty or possibly of a subordinate people.[1] But the chief interest attaches to the larger animal's head. In accordance with what was before said, we may regard it as tolerably certain that the name of the people is repeated. If "Zu-zu" is the correct reading of the strokes above the tall cone, the name of the animal whose head rests upon the double cone ought to be essentially the same. To solve the problem, if we are to be consistent, we must have recourse to the Semitic dialects, and preferably to Hebrew. Here we find a rare word, *ziz* (from a root *zuz* or *ziz*), used of an animal browsing sometimes on the vine (Psalm lxxx. 13, A.V.). It would be difficult to determine what particular species of animal is intended either in the Psalm or on the inscription; but it would seem not unsuitable to suppose that young wild cattle are intended in both. It will be in accordance with what has been said to identify "Zu-zu" with the Zuzim described in Genesis xiv. 5, as dwelling in the country east of the Jordan. And indeed, from an ancient city on the site of Jerablûs, a hostile raid on Bashan, Gilead, and the adjacent country was likely enough to be undertaken. Assyriological research has tended to show that Chedorlaomer and some other names in Genesis xiv. are genuine. It would not be very wonderful if the Hittite monuments should show that this is the case also with the name Zuzim.[2]

The results in decipherment thus set forth are, it may be said, but scanty and imperfect, and, in some cases, as based on slight evidence, may be liable to fall away when a wider induction is attainable. But "all science," it has been said, "is provisional"; and in relation to such a subject as that with which these articles are concerned, it may be sufficient if we should succeed in setting forth just principles, and in making even a slight extension of the boundaries of knowledge.

(To be continued.)

[1] Having regard to the shape of the symbol, one may be reminded perhaps of the wool which was included in the tribute paid by Mesha of Moab to the king of Israel (II. Kings iii. 4).

[2] Cf. Amos i. 3 *seq.*, and the Biblical use of "four" and "fourfold."

[1] It has been suggested that the smaller animal's head is that of a gazelle. If so, in the plural, the name would be in Hebrew "Tsebaim" or "Zeboûm," a name found in Genesis xiv. 2, but possibly this would only be a curious coincidence.

[2] The city indicated on this monument, for the name of which I have suggested "Bamoth-elah" (*ante*, p. 539), may possibly be identical, judging from the ideograph, with Bamoth-in-the-valley of Numbers xxi. 20.

ASA GRAY.

THE following, as yet unpublished, words, almost the last spoken publicly by Asa Gray, have a pathetic interest for all those who knew and loved him. They were uttered in the Free Trade Hall, at Manchester, at the opening meeting of the British Association in August last, in seconding the vote of thanks to Sir Henry Roscoe for his address:—

"For the very great honour of being called upon to second the motion for a vote of thanks to your illustrious President, I am mainly indebted to that deference which is naturally accorded to advancing years, a deference which sometimes—as in the present case—takes one unawares.

'In looking back over the list of Corresponding Members of the British Association, I find myself, much to my surprise, nearly, if not quite, the oldest survivor.

"I recognize, therefore, a certain fitness, on this score, in the call upon me to be the spokesman of those, your brethren from other lands, who have been invited to this auspicious gathering, and to the privilege of listening to the very thoughtful, well-timed, and most instructive address of your President.

"As guests, we desire, Mr. Mayor, heartily to thank the city of Manchester and the officers of the Association for inviting us; we wish to thank you, Sir Henry, for the gratification your address has afforded us.

"Convened at Manchester, and coming myself by way of Liverpool, I would say personally that there are two names which memory calls up from the distant past with unusual distinctness; both names familiar to this audience and well known over the world, but which now rise to my mind in a very significant way. For I am old enough to have taken my earliest lessons in chemistry just at the time when the atomic theory of *Dalton* was propounded, and was taught in the text-books as the latest new thing in science.

"Some years earlier, Washington Irving in his "Sketch-book" had hallowed to our youthful minds the name of *Roscoe*, making it the type of all that was liberal, wise, and gracious. And when I came to know something of botany I found that this exemplar, as well as patron, of good learning had, by his illustrations of Monandrian plants, taken rank among the *Patres Conscripti* of the botany of that day.

"The name so highly honoured then we now honour in the grandson. And I am confident that I express the sentiments of your foreign guests, whom I represent, when I simply copy the words of your President in 1842, now reproduced in the opening paragraph of the address of the President of 1887, transferring, as we fitly may, the application from the earlier to the later Manchester chemist: 'Manchester is still the residence of one whose name is uttered with respect wherever science is cultivated, who is here to-night to enjoy the honours due to a long career of persevering devotion to knowledge.'

"I cannot continue the quotation without material change. 'That increase of years to him has been but increase of wisdom' may indeed be said of Roscoe no less than of Dalton; but we are happy to know that we are now contemplating not the diminished strength of the close, but the manly vigour of the mid-course, of a distinguished career. Long and prosperously may it grow from strength to strength.

"In general, praise of the address which we have had the pleasure of hearing would not be particularly becoming from one whose chemistry nearly ended as well as began with the simple atomic theory of Dalton. But there is one topic which I may properly speak of, standing as I do as a representative of those favoured individuals whom your programme—for lack of a better distinguishing word—calls foreigners. I refer to the urgently expressed 'hope that this meeting may be the commencement of an international scientific organization.' For this we thank you, Mr. President, most heartily. This is, indeed, a consummation devoutly to be wished, and confidently to be hoped for, by all of us, especially by those for whom I am speaking. Not only we Americans, who are of British descent, and who never forget that blood is thicker than water, but as well our Continental associates on this platform, of the various strains of blood which interfused have produced this English race and fitted it for its noble issues—we, each and all, I repeat, accept this name of foreigners only in the conventional sense which the imperfection of language imposes. In the forum of science we ignore it altogether. One purpose unifies and animates every scientific mind with 'one divine intent,' and that by no means the 'far-off intent' of which the poet sings, but one very near and pervading. So we took to heart the closing words of your President's most pertinent and timely address. Indeed, we had taken them to heart in anticipation. And we have come to this meeting one hundred strong or more (in place of the ordinary score) fully bent upon making this Manchester meeting international.

"Far back in my youthful days there was a strong-willed President of the United States, of military antecedents, who once drew up and promulgated an official order which somewhat astounded his Cabinet officers. 'Why, Mr. President!' they said, 'you can't do that.' 'Can't do it!' replied General Jackson, 'don't you see that I have done it?' And so we internationals have come and done it. I am the unworthy spokesman of such a numerous, and such a distinguished array of scientific foreigners as have never been assembled before.

"Next year, if you will, you shall have as many more. When you, too, are ready to cross the Channel or the North Sea, we shall compose only a larger scientific brotherhood. And when you cross again the Atlantic, the brotherhood of science will be the more increased, and its usefulness in proportion.

"In behalf of your foreign guests, I heartily second the motion."

NOTES.

FIFTEEN years have passed since the Marshall Hall Fund was instituted with the twofold purpose of commemorating the late Dr. Marshall Hall, and for the encouragement of research in that branch of natural science which he did so much to develop. The Trust provides "that a prize shall be given every fifth year for the best original work done and recorded in the English language during the previous quinquennium, in physiological or pathological researches relating to the nervous system, and that the prize shall consist of the simple interest derived during the preceding five years from the amount of the capital fund." The first award was made to Dr. Hughlings Jackson, the second to Dr. Ferrier, and this year the Council of the Royal Medical and Chirurgical Society, in whose hands the Fund was placed, have awarded the prize to Dr. Walter Holbrook Gaskell, F.R.S., Lecturer in Advanced Physiology in the University of Cambridge. The Council have invited Dr. Gaskell to give some account of his work before the Society, and a special meeting will be convened for this purpose.

MR. MATTHEW ARNOLD, the tidings of whose death excited universal regret, did admirable service to the cause of education in England. No writer of his time pressed more earnestly on the attention of the public the need of thorough educational reform, and in his full and lucid Report on the Universities and secondary schools of the Continent he showed how far, in almost all matters relating to this essential element of the national life, we had allowed ourselves to be outstripped by some of our neighbours and rivals. Although, of course, convinced that

classical studies must be maintained in our schools and Universities, and personally interested chiefly in this aspect of the subject, Mr. Arnold frankly recognized the great place that must necessarily belong to science in any true system of education.

We print elsewhere a letter from Emin Pasha. Other letters from him have lately appeared in the *Times* and the *Scotsman*. His province is evidently once more in working order, and Emin is at peace with his neighbours. The letters took eight months to reach this country, so we need not be alarmed by the fact that no word has come of Mr. Stanley's arrival. Emin tells us that the country through which Mr. Stanley had to pass is of the most difficult character, full of swamps, and with rivers rendered impassable by vegetation; so that the expedition could not reach the Albert Nyanza before November.

So many new garden plants are annually described in various English and foreign periodicals that some are apt to escape the notice of botanists and horticulturists. From 1860 to 1886 a list was regularly published in the Gardener's Year-Book and Almanac; and during the months of January to May 1887 inclusive the *Journal of Horticulture* gave the names of plants up to October 1886. No later list has appeared. Now it has been decided that a list shall henceforth be given as one of the regular issues of the Kew Bulletin of Miscellaneous Information, and the first of the promised series is presented in the April number. It includes the new garden plants and alterations of names recorded between October 1, 1886, and December 31, 1887. To these have been added the names of authors, which did not appear in former lists. The list will be of great service to horticulturists.

On April 8 a beautiful display of the aurora borealis was observed at Throndtjem. The weather was fine, and there was no wind.

On March 12, at about 2 a.m., a faint shock of earthquake, accompanied by subterranean rumbling, was felt at Drammen, in Norway. It went from east to west.

A severe earthquake occurred at Lintthal (Canton Glarus) on April 2, at 9.10 a.m. At Elm the oscillations were so strong that the walls of the houses were cracked.

On the evening of Wednesday, the 11th inst., shocks of earthquake were felt in various parts of North Wales. At the large Baptist Chapel, Llangollen, while service was proceeding, a shock was distinctly felt, and the walls and ground were seen to shake. Shocks were also experienced at many of the residences in the valley, where the crockery and windows quivered in their places. A farmer residing at the Craig said his farmstead shook so much that he expected it to fall. The shocks were also noticed at Corwen, Bala, and Dolgelly.

Some months ago a Conference was held in Manchester with the object of promoting the interests of the silk industries of the United Kingdom. Various papers were read, and it was ultimately resolved that an Association, to be called the Silk Association of Great Britain and Ireland, should be formed. The objects of the Association are to promote and maintain the silk industry of Great Britain and Ireland in all its branches; to encourage the production of raw silk in India and our colonies; to collect and disseminate amongst its members useful information and statistics connected with or affecting manufacture and commerce in silk; and to promote technical, commercial, and linguistic education, and any necessary Parliamentary legislation; and generally to assist in the expansion and development of the trade. The Association was "inaugurated" at Manchester on March 22, and will hold a general meeting in London in June.

It is announced from Lyons that M. de Chardonnet has succeeded in getting by chemical processes a matter having all the appearance of silk. He adds to an etherized solution of nitrated cellulose (the base of gun cotton) a solution of perchloride of iron, and to this mixture a little of a solution of tannic acid in alcohol. The whole is poured, after filtration, into a vertical reservoir having a horizontal sharp nozzle (with fine passage) at its base, debouching in a vessel of water acidulated with nitric acid. The issuing fluid vein at once becomes consistent, and can be drawn off by a uniform movement. It is dried by passage through a dry air space, and then wound. It is of gray or black aspect; but by means of colouring matter put in the etherized solution the colour may be varied *ad lib.* It is further described as supple, transparent, cylindrical or flattened; of silky appearance and touch; the rupturing weight is 25 kilogrammes per square millimetre. The fibre burns without the flame being propagated; it is unattackable by acids and alkalies of mean concentration, by cold or hot water, alcohol, or ether; but it is dissolved in etherized alcohol and acetic ether.

An attempt was made last year to cultivate the cotton-tree in European Russia, in the neighbourhood of Taganrog, on the Don. We learn that the attempt proved successful, the temperature of the Lower Don being not inferior to that of the valleys south of the forty-first degree of latitude, where the cotton-tree is cultivated in Turkey.

The Board of Trade Journal for April contains a paper in which there are some interesting facts about sponge-fisheries. It seems that an industry in artificial sponges is in process of creation. M. Oscar Schmidt, Professor at the University of Gratz, in Styria, has invented a method by which pieces of living sponge are broken off and planted in a favourable spot. From very small cuttings of this kind, Prof. Schmidt has obtained large sponges in the course of three years, and at a very small expense. One of his experiments gave the result that the cultivation of 4000 sponges had not cost more than 225 francs, including the interest for three years on the capital expended. The Austro-Hungarian Government has been so much struck with the importance of these experiments, that it has officially authorized the protection of this new industry on the coast of Dalmatia.

According to *Allen's Indian Mail*, arrangements are now being made by the Meteorological Department of India for the prompt publication of a regular series of cyclone reports, so as to admit of their issue from two to three months after the date of the storm to which they refer. Hitherto, accounts of cyclones have not been published for a year or two after their occurrence.

The *American Meteorological Journal* for March contains the first of a series of articles by Mr. A. L. Rotch on the organization of the Meteorological Services of Europe, based upon a similar series by Dr. Hellmann, about ten years ago, with the addition of subsequent changes. The first country dealt with is Germany. Of the other articles may be specially mentioned one by Mr. W. M. Davis, on a proposed classification of the winds, according to their physical causes and conditions. The characteristics employed are: (1) the source of energy that excites motion (earth, sun, &c.); (2) the contrasted temperatures (equator, poles, &c.); (3) the period of occurrence; (4) the kind of wind (cyclonic, sea-breezes, &c.). By this means, although no claim to novelty is made, except as to the arrangement of the data, the author proposes to bring together what is known into a convenient shape, and so to separate the unsorted material for further critical examination. Prof. H. A. azen continues the controversy between Dr. Hann and himself as to the behaviour of pressure and temperature in high and low barometric areas, at elevated stations; his theory being that

the low temperature in a cyclone at a mountain station is due to the lagging behind of the minimum pressure, and similarly with respect to the high temperature in an anticyclone.

AN interesting paper is contributed to the April number of the Journal of the Chemical Society by Mr. Ward Couldridge on chlorophosphide of nitrogen. This peculiar compound was shown some years ago by Dr. Gladstone to be represented by the empirical formula $PNCl_2$, but vapour-density determinations revealed the curious fact that in the gaseous state its molecule really possesses the constitution $P_3N_3Cl_6$. Mr. Couldridge prepares it by a method somewhat different from that employed by Dr. Gladstone, and one which gives a better yield. Pentachloride of phosphorus is heated with chloride of ammonium in a flask connected with an upright condenser, so that the pentachloride volatilized returns to the seat of action until it becomes completely decomposed, and the chlorophosphide, which would otherwise be carried away by the escaping hydrochloric acid, crystallizes in the condenser. The whole of the chlorophosphide is subsequently purified by distillation in steam. The reaction is found to be as follows: $3PCl_5 + 3NH_3 = P_3N_3Cl_6 + 9HCl$. Chlorophosphide of nitrogen thus prepared dissolves readily in ether, chloroform, or carbon bisulphide, and separates on evaporation in fine rhombic crystals, which have a most remarkable aversion to water, refusing under any circumstances to be wetted by it. When fused and heated above its boiling-point, it emits a singular odour. It has the proud distinction of being unattacked by all the strong acids, hot fuming nitric alone being capable of making any impression upon it. Mr. Couldridge finds that when dry ammonia gas is led through a hot tube containing the melted chlorophosphide, a somewhat violent reaction occurs resulting in the formation of another remarkable compound known as phospham, $P_3H_3N_6$ or $P_3N_3(NH)_3$. Not only does ammonia behave in this way, but all substituted ammonias, such as the amines, form similar compounds; aniline, for instance, gives a white solid, readily crystallizable from glacial acetic acid, of the composition $P_3N_3(NH \cdot C_6H_5)_6$. Phospham itself is both insoluble in water and infusible at a red heat, but fumes in contact with air, owing to slow decomposition and oxidation. One cannot help remarking how singular it is that the introduction of phosphorus, itself a notable combustible, into the terribly explosive compound of chlorine and nitrogen, should result in the formation of a substance so extremely inert as the chlorophosphide; yet such are the vagaries met with by the chemist.

A SECOND edition of Prof. C. M. Tidy's "Hand-book of Modern Chemistry, Inorganic and Organic," for the use of students, has been issued by Messrs. Smith and Elder. As regards general arrangement, the author has adhered to the plan he first adopted. He especially notes that in writing of chemical compounds he has at times not hesitated to use common language. "If," he says, "I have used the word 'potash,' and the body I mean to imply thereby is understood, I am satisfied. I confess that the growing necessity for having a translation at one's side in attempting to understand the modern scientific paper, is in my opinion a circumstance to be deplored. Danger, moreover, is always to be apprehended when a language has to be invented to support a theory or a formula. A party shibboleth has, no doubt, a charm for its special clique. It serves as a bond of union for the initiated, whilst it prevents the interference of outsiders. But, all the same, it is distracting to the independent worker, and can but prove a hindrance to the general cultivation of science."

MESSRS. G. BELL AND SONS will shortly publish "The Building of the British Islands," a study in geographical evolution, by Mr. A. J. Jukes-Browne. The author tries to restore the geography of the British region at successive epochs of geological time, and to describe the gradual formation or evolution of the British Isles. The book will be illustrated by maps.

MESSRS. ROPER AND DROWLEY will publish immediately "Geology for All," by Mr. J. Logan Lobley, Professor of Physiography at the City of London College, and author of several volumes on geological subjects.

THE valedictory address delivered by Mr. J. W. Taylor as President of the Conchological Society has been reprinted from the *Journal of Conchology*, and issued separately. Mr. Taylor brings together some interesting observations bearing on the variation of British land and fresh-water Mollusca.

MR. THOMAS WILSON, of the Smithsonian Institution, calls attention in the *American Naturalist* to the fact that the importance of the subject of criminal anthropology has not hitherto been so thoroughly appreciated in the United States as in Europe. A step in the right direction, however, has been taken by the New York Academy of Anthropology, which lately held a meeting for the consideration of questions connected with criminal anthropology. These questions were classed under two heads, criminal biology and criminal sociology. In the circular summoning the meeting it was contended that the true way of studying crime is to begin with the study of the criminal himself. "It is impossible," said the writer, "to evolve the criminal out of one's inner consciousness. Knowledge of his peculiarities is essential to any rational treatment of him, and this knowledge can only be gained by systematic, intelligent observation of his physical and mental habits, supplemented by an exhaustive analytical comparison of the facts observed, with a view to their right classification and interpretation."

THE Mitchell Library, Glasgow, has now been ten years in existence, and the Committee, in the Annual Report just issued, express the belief that no consulting or reference library has ever made so much progress in so short a time. Speaking of the character of the reading, they say that it continues satisfactory, and bears evidence of a desire on the part of readers to seek solid information from the abundant resources at their disposal. Unfortunately, the Committee have to report that during 1887 twenty-one books were stolen.

THE Royal Microscopical Society will hold a *conversazione* on Wednesday evening, the 25th inst.

AN Aëronautical Exhibition was opened at the Rotunde in the Prater at Vienna on April 1.

THE additions to the Zoological Society's Gardens during the past week include an Arabian Baboon (*Cynocephalus hamadryas* ♀) from Massowah, presented by Mr. D. Wilson-Barker, R.N.R., F.Z.S.; two White-necked Crows (*Corvus scapulatus*), two Spotted Eagle-Owls (*Bubo capensis*) from South Africa, presented by Captain Henry F. Hoste, s.s. *Trojan*; a Muscovy Duck (*Cairina moschata*) from South Africa, presented by Mr. W. Shuter; four Half-collared Doves (*Turtur semitorquatus*) from Africa, presented by Mrs. Wisely; a Slowworm (*Anguis fragilis*), British, presented by Mr. F. W. Pilkington; two Indian Swine (*Sus cristatus* ♂ ♂) from India, a Greater Vasa Parrot (*Coracopsis vasa*) from Madagascar, a Blue-eyed Cockatoo (*Cacatua ophthalmica*) from South Australia, a Goffin's Cockatoo (*Cacatua goffini*) from Queensland, an Asp Viper (*Vipera aspis*) from Italy, deposited; four European Pond Tortoises (*Emys europæa*), European, purchased; two Collared Fruit Bat *Cynonycteris collaris*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

HARVARD COLLEGE OBSERVATORY.—The most interesting item in the forty-second Annual Report of the Director of the Harvard College Observatory is the account of the threefod accession to its resources which it has received during the past

year. This consisted of the funds provided by Mrs. Henry Draper for carrying on the photographic study of stellar spectra as a memorial to her late husband; the fund left by the late Uriah A. Boyden for the establishment of a mountain Observatory; and the large bequest of the late Robert Treat Paine. Prof. Pickering points out, however, that the Observatory still stands in need of further endowment, as its new resources are necessarily largely absorbed in those new lines of research for which they were specially designed, and considerable improvements are required in the principal building; and he adds that it is probable that there has never been a time in the history of the institution when so large a return could be obtained from a given expenditure as at present. The most striking results obtained during the year have been those secured by the use of the Henry Draper Memorial Fund in the photographic study of stellar spectra, and which have been already referred to in these columns. Under the Boyden Fund several instruments have been devised and constructed for the automatic registration of the meteorological conditions and general fitness for observing of sites for Observatories, and these have been carefully tested at various elevated stations. The usual observations have also been kept up, including the observation with the meridian photometer of the magnitudes of stars in zones at intervals of 5° in the region covered by the Southern D.M. This work was about half finished, and would, it was expected, be entirely completed within the present year. The east equatorial had been used in the observation of eclipses of Jupiter's satellites and of comparison-stars for variables. A wedge photometer, arranged in a somewhat modified manner, is employed with this telescope, and is to be used in the investigation of the phases of asteroids and in the observation of zones of D.M. stars. The meridian circle is to be engaged in the observation of one of the zones required in the proposed revision of the Southern D.M.

COMET 1888 *a* (SAWERTHAL).—Dr. L. Becker has computed the following elements and ephemeris from observations made on February 18 at the Cape, March 13 at Palermo, and March 27 at Strasburg. From the outstanding deviation of the middle place it may be inferred that unless there be some considerable error in the observations the true orbit will prove to be elliptical.

$$T = 1888 \text{ March } 16{\cdot}96412 \text{ G.M.T.}$$

$$\left.\begin{aligned} \pi - \Omega &= 359^\circ\ 49'\ 45''{\cdot}1 \\ \Omega &= 245\ 30\ 40{\cdot}2 \\ \iota &= 42\ 17\ 47{\cdot}4 \end{aligned}\right\} \text{Mean Eq. } 1880{\cdot}0.$$

$$\log q = 9{\cdot}844562$$

Error of middle place $(O - C)$.

$$\Delta\alpha \cos\delta = -2{\cdot}61\text{s.} \quad \ldots \quad \Delta\delta = +7''{\cdot}1.$$

Ephemeris for Greenwich, Midnight.

1888	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
April 20	22 57 31	20 22·7 N.	0·1517	9·9912	0·3
22	23 2 48	21 44·3			
24	23 7 58	23 2·3	0·1681	0·0143	0·3
26	23 13 3	24 16·9			
28	23 18 2	25 28·3	0·1835	0·0369	0·2
30	23 22 55	26 36·7			
May 2	23 27 41	27 42·2 N.	0·1980	0·0588	0·2

The brightness at discovery is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 APRIL 22–28.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on April 22

Sun rises, 4h. 50m.; souths, 11h. 58m. 21·1s.; sets, 19h. 6m.: right asc. on meridian, 2h. 2·2m.; decl. 12° 26′ N. Sidereal Time at Sunset, 9h. 11m.

Moon (Full on April 26, 6h.) rises, 14h. 3m.; souths, 21h. 7m.; sets, 3h. 56m.*: right asc. on meridian, 11h. 12·3m.; decl. 8° 39′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury	4 31	10 52	17 13	0 55·3	3 21 N.
Venus	4 21	10 40	16 59	0 44·1	3 2 N.
Mars	17 24	23 1	4 38*	13 7·0	5 12 S.
Jupiter	21 56*	2 10	6 24	16 12·7	20 8 S.
Saturn	10 7	18 5	2 3*	8 9·5	20 44 N.
Uranus	17 11	22 49	4 27*	12 54·3	5 4 S.
Neptune	6 1	13 43	21 25	3 47·6	18 18 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

April.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
22	B.A.C. 3837	6	18 9	19 32	90 173
25	65 Virginis	6	4 19	near approach	17 —
28	χ Ophiuchi	6	5 46	6 44	96 316

April.	h.	
24	22	Mars in conjunction with and 3° 16′ south of the Moon.
28	1	Jupiter in conjunction with and 3° 26′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Apr. 22,	3 21	*m*
U Virginis	12 45·4	6 10 N.	,, 23,		*m*
V Boötis	14 25·3	39 22 N.	,, 27,		*M*
δ Libræ	14 55·0	8 4 S.	,, 24,	22 4	*m*
U Coronæ	15 13·6	32 3 N.	,, 24,	1 14	*m*
S Coronæ	15 16·8	31 46 N.	,, 25,		*M*
U Ophiuchi	17 10·9	1 20 N.	,, 22,	0 38	*m*
			,, 27,	1 24	*m*
β Lyræ	18 46·0	33 14 N.	,, 22,	3 0	*M*
U Capricorni	20 41·9	15 12 S.	,, 26,		*M*
T Vulpeculæ	20 46·7	27 50 N.	,, 23,	2 0	*m*
δ Cephei	22 25·0	57 51 N.	,, 22,	21 0	*m*
S Aquarii	22 51·1	20 56 S.	,, 23,		*M*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

THE death is announced of Nicholas von Miklucho-Maclay, at the age of forty-two years. M. Maclay's name must be familiar to our readers in connection with New Guinea explorations. He was the son of a Russian nobleman, and studied medicine and natural science at St. Petersburg and at several Dutch Universities. In 1866 he accompanied Prof. Haeckel to Madeira; in 1867 he visited the Canary Islands, and, in 1869, Morocco. He then made preparations for an extended exploration among the Pacific Islands, and especially in New Guinea. He went by South America, Tahiti, and Samoa to New Guinea, and remained for over a year, 1871–72, on its north-west coast, afterwards exploring the south-west coast to the south of Geelvink Bay. In 1874–75 he visited Further India, and especially Malacca, where he explored several districts in the interior, and obtained important results. After visiting the Pelew, Admiralty, and other island groups, Maclay again went to New Guinea (1876–78), devoting himself to the north coast, where he was now well known, and was on friendly terms with several Papuan tribes. Maclay then went to Singapore and Sydney to restore his shattered health, but was in New Guinea again in 1879, afterwards visiting several Pacific islands and going on to Sydney once more. He returned to Russia in 1882, bringing with him rich collections in ethnography and in natural history. M. Maclay afterwards resided for some time in Sydney, where he founded a biological station. He recently returned to Russia, and at the time of his death, we understand, was preparing for publication a complete account of his many years' work. At present the records of his travels, with their rich anthropological results, are to be found mainly in the Proceedings of the Batavia Society and the Russian Geographical Society.

WE also learn of the death of Herr Anton Stecker, who has done some good exploring work in Africa. In 1878 he accompanied Rohlfs to Kufra, and in 1880 he went out at the expense

of the German African Society to Tripoli, and thence by Egypt and Abyssinia to Galla Land. Herr Stecker's observations referred largely to natural history, of which he was a student.

LIEUT. WISSMANN, the African traveller, who was obliged to spend the winter at Madeira on account of ill-health, there had an opportunity of writing the report of his second journey to Africa. The book has just been published by Brockhaus. At present Lieut. Wissmann is engaged on an account of his first expedition to the south of the Congo Basin, in company with Dr. Pogge.

DRS. FRIEDERICH KURTZ AND WILHELM BODENBENDER, both Professors at the Cordoba University (Argentine Republic), have started on a scientific expedition to the East Andes.

FORESTRY IN THE CAPE COLONY.

THE Report of Consul Siler, the United States representative at Cape Colony, which has been just issued, contains a full account of the present state of forestry in that country. He says that of the 214,000 square miles which are comprised in Cape Colony, there are something over 350 square miles covered with large forest trees. These forests lie almost all together near the sea, running nearly parallel to the coast, in the temperate regions of the southern mountain chains. Till recent years the system of felling pursued was a most wasteful and unsystematic one. Far from confining the operations of the woodcutters to certain limited sections or areas, the authorities permitted them to roam about at pleasure, and to pick and choose from among the forests what trees they should cut down. This license had its natural effect: only the choicest trees were cut, and even of these only selected portions were taken away, the rejected parts being left to cumber the ground. It has been estimated by those skilled in woodcraft that by this pernicious system 30 cubic feet of wood were wasted to each one utilized; and thus it is that many forests have totally disappeared, and even those that were not so easily accessible have been sadly impoverished. Till 1880 no steps were taken to preserve this natural wealth that was being so shamefully abused. In that year, however, the question was strongly urged on the attention of the Colonial Parliament. One of the chief defects of the system was pointed out—namely, the total absence of skilled caretakers, those then in charge having received no technical education whatever; and to meet this in some measure Parliament at once voted a sum of money to pay a trained superintendent. The choice fell on Count de Vasselot, who had had wide experience in French forestry at Nancy, and he at once proceeded to lay the foundations of the present forest department at the Cape. One of his first steps was to divide the forests into districts, which he again subdivided into sections, and to direct that felling should proceed in sections, the re-growth of the first section being given time to develop into mature trees before the axe was again used in that section. By this system the entire shutting up of any forest for a time is done away with. At present the period for the "revolution" of fellings is fixed at forty years. The tariffs now vary for standing timber from 2 cents to 6 cents per cubic foot of sound wood; with the exception of stinkwood (*Oreodaphne bullata*), which, being very hard and very valuable, was almost threatened with extermination, for which the price is 24 cents per cubic foot. Poles from 6 inches to 10 inches in diameter are sold at the rate of 2 cents per running foot; spars from 4 inches to 6 inches in diameter at 12 cents per 100 running feet.

The Consul illustrates the general system of managing and preserving the forests now followed in the colony by a minute description of that used in Knysua, the most extensive and most valuable of all the Cape forests. The total area of the Knysua may be roughly stated to be 100,000 acres, and of this magnificent forest almost three-fourths have been impoverished and in fact exhausted by the indiscriminate and reckless system of felling pursued in the past. At present the staff to conserve and replant this forest consists of one conservator, three superior grade officers, and six rangers or guards. Each higher grade officer has the superintendence of a tract of woodland varying in extent from 10,000 to 30,000 acres, in which he surveys the large timber, fixes the limits of the blocks or series, and plans out the boundaries of the various sections. No works are sanctioned without the consent of the Superintendent of Woods and Forests, and, if he has given his approval, the sections are surveyed and the trees fit for felling are marked with an official stamp. The duties of the rangers are to ride about their districts and endeavour to discover any breaches of the forest regulations, and in cases of successful prosecution they are rewarded according to the zeal and ability displayed by them. Besides the officers above-named, there are thirteen foresters distributed over the different woods, whose duty it is to plant, and, if necessary, transplant trees, and to take care of young trees. These men are paid at the rate of $20 a month, are provided with free quarters and ten acres of garden land, and are paid a bonus of $2.50 per 1000 for planting nursery plants, $2.50 per 1000 for 1-foot trees in the forest, or for nursery work and transplanting $5 per 1000 trees. This bonus cannot in the case of any individual forester exceed $200 in the year, without special permission. Each forester is expected to raise at least 40,000 young trees annually. So far as can at present be judged, seeing that the system has had but a few years' trial, it has undoubtedly proved a success. To show the amount of work that some of these foresters get through, it may be mentioned that in King William's Town forests in the year 1885 six foresters planted in the course of the year 138,080 plants in the nursery, and transplanted from the nursery into the forests 63,885 young trees. With the object of encouraging these valuable efforts to preserve the forests and to increase the area under timber, the Colonial Government has laid out several large tracts of land into plantations and nurseries, and although these are but of very recent formation they have already proved their utility in the reafforesting of the country. At the Government nurseries there are at the present moment over one million plants flourishing. In the working of these nurseries and plantations, convict labour has been utilized as largely as possible, and by this means the cost of the convict prisons has largely diminished. One other work in this connection of the Colonial Government is worthy of remark. At the plantation of Tokai, on the Table Mountain range, 150 species of extra-tropical trees have been introduced, and from them plants have been raised, with which it is proposed to reafforest the whole Table Mountain slopes, and already, in the short space of two seasons, 1000 acres have been replanted. From all the Government nurseries plants can be purchased at a nominal rate, and this, together with a recent Act whereby public bodies receive Government aid to the extent of one-half their expenditure on replanting, has given a strong stimulus to, and has aroused general interest in, the science of arboriculture among the colonists. Following the example of many American States, their first "arbor day," in 1886, was proclaimed a public holiday; and so great was its success that it is very likely to become a permanent institution. The Consul concludes his Report by saying that it is confidently hoped that with such machinery at work and with a growing interest in the advantages of tree-cultivation, in the future, Cape Colony will be independent of foreign markets for her timber supply; and that it is probable that the presence of forests, by increasing the rainfall, will bring tracts which are at present barren into cultivation.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 22.—"On the Skull, Brain, and Auditory Organ of a new Species of Pterosaurian (*Scaphognathus Purdoni*) from the Upper Lias, near Whitby, Yorkshire." By E. T. Newton, F.G.S., F.Z.S., Geological Survey. Communicated by Dr. Archibald Geikie, F.R.S.

The fossil Pterodactyl skull, which is the subject of this communication, was obtained from the Upper Lias of Lofthouse, near Whitby, by the Rev. D. W. Purdon, of Wolverhampton. It is the first Pterodactyl found in the Yorkshire Lias, and is a new form, allied to the Continental Jurassic species *Scaphognathus* (*Pterodactylus*) *crassirostris* of Goldfuss. The structure of the skull, including the back, base, and palatal regions, is better shown than in any previously discovered specimen; and in addition to this the brain and parts of the auditory organs have been exposed.

In its present condition the skull is about five and a half inches long; but apparently about two inches of the front are wanting. The elongated snout gives the skull a very bird-like appearance; but its most striking features are the five apertures, surrounded by bone, seen on each side. The orbit is the largest of these apertures; in front of this, and next in size, is the ant-orbital fossa; still further forward is the somewhat smaller external

nostril. Behind the orbit is the temporal space, divided by a bony bar into the supra- and infra-temporal fossæ.

On the upper surface of the skull are to be seen the nasals and prefrontals, on each side of the premaxillary process. The frontals form the upper boundaries to the orbits, and are confluent posteriorly with the parietals. Strong buttresses extend outward from the postfrontal and parietal regions to form the supra-temporal bar. There is on each side a large lachrymal bone forming the greater part of the upper and hinder boundary of the ant-orbital fossa. The jugal and quadrate-jugal are of a somewhat unusual form; the former bounding the lower half of the orbit, and the latter inclosing in an open V the greater part of the infra-temporal fossa. The quadrate is a wide but thin plate seen chiefly at the back of the skull. The base of the cranium is remarkable for its depth and extreme antero-posterior flattening; and viewed from behind a pair of long rods are seen extending from its lower margin, one on each side, to the inner angles of the quadrates. These bones are regarded as the homologues of the basi-pterygoid processes of the sphenoid, such as are seen in some lizards and birds, as for example in the Chameleon and Emu.

From the point of junction of the quadrate and basi-pterygoid process a bone runs along the palate, and dividing anteriorly forms the hinder boundary of the internal nostril, its outer portion joining the maxilla and its inner being continuous with a median bone occupying the position of a vomer. This bony bar, it is thought, represents the palatine and pterygoid bones.

The back of the skull is essentially Lacertilian. A large paroccipital bone extends outwards from the sides of the foramen magnum, and its distal end, expanding, embraces the upper part of the quadrate. The relation which the base of the paroccipital bears to the semicircular canals shows that it must be chiefly formed by the opisthotic element, as Prof. W. K. Parker has shown to be the case in lizards, and not by the exoccipital as it is in birds.

By removing the frontal and parietal bones of the left side, a cast of the brain cavity has been exposed, which there can be no doubt represents the form of the brain, just as closely as does that of a bird's cranial cavity. In proportion to the size of the entire skull, the brain of this Pterodactyl is very small, being not more than one-eighth of its length. Each cerebral lobe is oval in shape, and about as thick as it is wide. The olfactory lobe is small. Behind the cerebrum is a pair of large optic lobes, occupying a prominent position on the sides of the brain, and extending upwards well to the upper surface, but not meeting above in the middle line. The region of the cerebellum has been broken away, and its exact form therefore is somewhat uncertain; but, judging from portions which remain, it is tolerably clear that it extended between the optic lobes, and may have reached as far forwards as the cerebrum. Attached to the side of the medulla oblongata is a large flocculus, such as occurs in this position in birds.

It was the finding of the flocculus which led to the discovery of some parts of the auditory apparatus. On clearing away the stone in this region, a small tube filled with matrix was found arching over the pedicle of the flocculus and dipping down between it and the optic lobe. This tube occupies the position of the anterior vertical semicircular canal in the goose. By tracing the canal backwards and downwards it was found to join another similar tube forming an arch behind the flocculus—that is, in just the position of a posterior vertical semicircular canal. By careful excavation below the flocculus, a portion of a third tube was found, arching outwards in a horizontal plane, and this is believed to be the external semicircular canal.

The similarity between the base of the fossil skull and that of the Chameleon led to the inference that the fenestra ovalis would be found to be similarly placed in both, and by clearing away the matrix from the orbit and temporal fossa this inference was proved to be correct. The form and relations of the quadrate bone make it highly probable that this Pterosaurian had no ear-drum.

A comparison of this fossil with the skulls of known Pterosauria leaves no doubt that it is more nearly related to the *Scaphognathus* (*Pterodactylus*) *crassirostris* than to any other species, but as it differs from that form, and is evidently new, it is to be named specifically *Scaphognathus Purdoni*.

The Pterosaurian skull, as exemplified by this Lias fossil, resembles more the Lacertilian than any other type of Reptile skull; and seeing that the skulls of birds and lizards are in many points very similar, one is not surprised to find in this fossil characters which are also found in both these groups. In considering, therefore, the relation which the Pterosaurian skull bears to those of birds and lizards, the characters should be especially noticed which serve to distinguish between the two groups, thus:—

1. In birds the brain-case is larger in proportion to the size of the skull than it is in lizards.

2. The quadrate, pterygoid, and palatine bones are movable on the skull in birds, but more or less fixed in lizards.

3. In birds the hinder end of the palatine and front end of the pterygoid are brought into close relation with the rostrum of the sphenoid. This is not the case with lizards.

4. The orbit is rarely completed by bone in birds, and never by the jugal; in lizards the orbit is surrounded by bone, and the jugal forms part of it.

5. In birds there is no prefrontal bone, while it is always present in lizards.

6. No bird has a supra-temporal bar of bone, but it is always developed in lizards.

7. In lizards the paroccipital process is large and formed by the opisthotic; in birds the paroccipital is small and formed by the exoccipital.

8. In birds the bones of the cranium are early anchylosed; in lizards they nearly always remain separate.

9. Birds have the premaxillæ large and united into one bone; in lizards they are usually small.

10. The ant-orbital fossa which is present in birds is only occasionally present in lizards.

11. In birds there is always a lower temporal bar of bone extending from the maxilla to the quadrate. This bar is incomplete in all lizards except *Sphenodon*, although well developed in other reptiles.

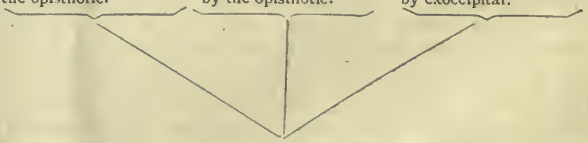
The skull of *Scaphognathus Purdoni* agrees with lizards in the first seven of the above characters; and with birds in those numbered 8, 9, 10. Number 11 need not be considered, as it can scarcely be regarded as distinctive. The greater importance of the first seven characters makes it clear that in the structure of the skull *S. Purdoni* most nearly resembles the Lacertilia.

The brain of *Scaphognathus Purdoni* agrees with that of reptiles in its relatively small size; while the separation of the optic lobes by the cerebellum and the meeting of the latter with the cerebrum, as well as the possession of a distinct flocculus, are important points in which it resembles the brain of the bird. On the other hand, the form of the optic lobes is unlike that of any living bird.

The brain of the American fossil-bird, *Hesperornis*, shows a striking resemblance to that of *Scaphognathus Purdoni*, for not only is it proportionally smaller than in recent birds, but the relation of the cerebellum and cerebrum to the optic lobes is very similar.

The facts above stated seem to show that the Pterosauria are related to the birds in the form of the brain, and to the lizards in the structure of the skull. This, however, does not constitute the Pterosaurian a transitional form between birds and reptiles, in the sense of the Pterosauria having been derived from reptiles, or of the birds having been derived from Pterosauria; but rather points to *Aves*, *Pterosauria*, and *Reptilia* having been derived from some common ancestral type. These relationships may be thus indicated, taking only a few of the characters of each:—

Lizard.	*Pterosaurian.*	*Bird.*
Cerebellum small, optic lobes meeting, paroccipital formed chiefly by the opisthotic.	Cerebellum large and between optic lobes. paroccipital formed chiefly by the opisthotic.	Cerebellum large and between optic lobes. paroccipital formed chiefly by exoccipital.



Ancestral Type.

Cerebellum small, optic lobes meeting, paroccipital small, and formed by both exoccipital and opisthotic.

Mathematical Society, April 12.—Sir J. Cockle, F.R.S. President, in the chair.—The following communications were

made:—Continuation of a former paper on simplicissima, by W. J. C. Sharp.—Synthetical solutions in the conduction of heat, by E. W. Hobson.—Symmetric functions, part ii., by R. Lachlan.—On a law of attraction which might include both gravitation and cohesion, by G. S. Carr.—Messrs. Buchheim, Larmor, and Greenhill spoke upon the various papers.

PARIS.

Academy of Sciences, April 9.—M. Janssen, President, in the chair.—Observations of the minor planets made with the great meridian instrument of the Paris Observatory during the third and fourth quarters of the year 1887, by M. Mouchez. The right ascension, polar distance, and correction of ephemerides are tabulated for thirteen of the minor planets.—On Gompertz and Makeham's laws of mortality, by M. J. Bertrand. Some arguments are advanced to show that, although he does not mention them, Thomas Simpson must have been acquainted with one or both of these laws.—Observations on the fixation of nitrogen by certain vegetable soils, by M. Berthelot. Some remarks are made in connection with the author's previous communications and M. Schlœsing's recent notes on this subject. It is pointed out that M. Schlœsing has not taken sufficient account of the experimental conditions which M. Berthelot has shown to be necessary in dealing with the question of nitrification.—On a new gas-thermometer, by M. L. Cailletet. This instrument, which has been for some time employed by the author, especially in connection with his researches, jointly made with M. Bouty, on the measurement of electric resistances at low temperatures, is described as of an extremely sensitive character, indicating differences of height of 2·36 millimetres for 1° of temperature. Being intended for measuring extremely low temperatures, it is charged with hydrogen as the expanding body.—Report on M. Delauney's astronomical communications, by the Commissioners, MM. Daubrée, Tisserand, and Faye. These communications, which were addressed to the Academy during M. Delauney's residence in Cochin-China, are now resumed in one volume, and are of an extremely varied character. They deal with the distances of the planets from the sun; the distances of the satellites from their respective planets; the distances of certain stellar groups from the central orbs of their systems; the distance of aërolites from the sun, their action on the solar spots, on our volcanoes, on the meteorological phenomena of our atmosphere, and on terrestrial magnetism; formation of the stellar systems, and especially that of Sirius, of which the sun itself, with Procyon, α Centauri, Vega, Arcturus, and others, would appear to be members. These, and other even bolder speculations, seem based on the three laws of distances here formulated by the author.—Observations of Sawerthal's Comet 1888 *a* made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan, and at the Bordeaux Observatory (0·38 m. equatorial), by MM. G. Rayet and Courty. The Paris observations cover the period from March 25 to April 6; those of Bordeaux from April 4-6.—Observations of Palisa's new planet, discovered April 3, 1888, made at the Observatory of Algiers with the 0·50 m. telescope, by MM. Trépied and Sy. These observations, made on April 4, give an estimated magnitude of 12·5 for this planet.—On M. Bertrand's geometrical curves, by M. G. Demartres. These curves are here considered as geodetic lines of ringed surfaces; and the following problem is proposed and discussed: To find the surfaces whose circular generator is inclined at the same angle, *i*, on the same family of geodetic lines, this angle, however, being capable of varying from one generator to the next.—Action of the tetrachloride of carbon on oxygenated mineral compounds free of hydrogen, by M. H. Quantin. It was long ago shown by Geuther that potassa and baryta raised to a red heat in the vapour of the tetrachloride of carbon are transformed to chlorides and carbonates. More recently the experiments of Demarçay and Quantin, since confirmed by Lothar Meyer, have shown that oxides which cannot be attacked by chlorine alone are under the same conditions also transformed to chlorides. In the present paper the author deals more fully with these phenomena, and generalizes the results already obtained.—On the sesquichloride of rhodium, by M. E. Leidié. After examining the processes hitherto employed in the preparation of the anhydrous sesquichloride, the author describes a new method in which the chlorine acts on the alloy of rhodium and tin, $RhSn_2$, described by Debray. He then gives the processes of preparation of some double chlorides formed by the hydrated sesquichloride.—On the passive property of nickel, by M. Ernest Saint-Edme. Having already described the results of his researches on the passivity of steel and iron, the author here deals with some of the conclusions he has obtained from the analogous study of nickel.—Action of the cyanide of zinc on some chlorides, by M. Raoul Varet. The results are described of experiments with the chlorides of mercury and copper, as well as with the alkaline chlorides. The general conclusion is arrived at that the cyanide of zinc does not enter into molecular combination with the chlorides.—Syntheses by means of cyanacetic ether (continued), by M. Alb. Haller. In the present paper the author deals with the higher homologues of acetylcyanacetic ether.—Heat of formation of aniline, by M. P. Petit. The heat of formation of aniline is here determined, both by the wet and dry processes, with fairly uniform results.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Key to the Mysteries of Water, Electricity, and Heat: W. Boggett (Trübner).—Die Catastrophe von Zug, 5 Juli, 1887 (Hofer and Burger, Zürich).—Zrání Oplozeni a Rýhování Vajíča: Fr. Vejdovský (Prag).—Zeitschrift für wissenschaftliche Zoologie, 46 Band, 2 Heft (Leipzig).—Proceedings of the Academy of Natural Sciences of Philadelphia, Part 3, 1887 (Philadelphia).—Journal of Physiology, vol. ix. No. 1 (Cambridge).—Bulletin de la Société Impériale des Naturalistes de Moscou, 1888, No. 1 (Moscou).—Proceedings of the Geologists' Association, No. 87 (Stanford).—Botanische Jahrbücher für Systematik, Pflanzengeschichte, und Pflanzengeographie, Neunter Band, 4 Heft (Williams and Norgate).—Annalen des k. k. Naturhistorischen Hofmuseums, 1887 (Wien).—Journal of Comparative Pathology and Therapeutics, Part 1 (Johnston).—Journal of the Society of Telegraph-Engineers and Electricians, No. 71 (Spon).—Journal of the Asiatic Society of Bengal, vol. xvi. Part 2, Nos. 2 and 3 (Calcutta).—Journal of Anatomy and Physiology, April (Williams and Norgate).—Sitzungsberichte der k. b. Gesellschaft der Wissenschaften. Math. Naturw. Classe. 1886 (Prag.)—Bericht über die Math. und Naturw. Pubn. ii. Heft (Prag).—Geschichte der k. b. Gesellschaft der Wissenschaften, Zweites Heft (Prag).—A Higher Arithmetic and Elementary Mensuration: P. Goyen (Macmillan).—Next of Kin Marriages in Old Irân: D. P. Sunjana (Trübner).—Mechanics and Experimental Science: *Chemistry*, C. Aveling (Chapman and Hall).—The Minerals of New South Wales, &c.: A. Liversidge (Trübner).—Dissolution and Evolution and the Science of Medicine: C. P. Mitchell (Longmans).—Notes from the Leyden Museum, vol. 9, Nos. 1 and 2 (Leyden).—Journal of the Royal Statistical Society, March (Stanford).—Journal of the Chemical Society, April (Gurney and Jackson).—Bulletin de l'Académie Royale des Sciences de Belgique, No. 2 (Bruxelles).—Journal of the Royal Microscopical Society, April (Williams and Norgate.—The Auk, April (New York).—Mittheilungen der Naturforschenden Gesellschaft in Bern, 1887 (Bern).—Verhandlungen der Schweizerischen Naturforschenden Gesellschaft in Frauenfeld, 1886-87 (Frauenfeld).—Quarterly Journal of the Royal Meteorological Society, January (Stanford).

CONTENTS.

THURSDAY, APRIL 26, 1888.

MR. A. C. SMITH'S "BIRDS OF WILTSHIRE."

The Birds of Wiltshire, comprising all the Periodical and Occasional Visitants, as well as those which are indigenous to the County. By the Rev. Alfred Charles Smith, M.A. (London: Porter, 1887.)

BY all ornithologists Wiltshire will be admitted to be a county the birds of which are worthy of a volume; and all ornithologists, who know, even by name and reputation only, Mr. Alfred Charles Smith, will admit that he of all men is the proper author of that volume. Nominally but the Honorary Secretary of the Wiltshire Archæological and Natural History Society, the Rector of Yatesbury has for many years past been its most active officer, and the editor of its organ—the *Wiltshire Magazine*—to say nothing of the various "by-blows" of which he has at times been delivered in the shape of "Tours" in Portugal, Egypt, and Palestine, or of the very laborious and important work on the "British and Roman Antiquities of the North Wiltshire Downs"—that work which so narrowly escaped total destruction—nearly all the copies of the original edition having perished by a disastrous fire while in the binders' hands. Mr. Smith, too, is a Wiltonian of the Wiltonians; not only one of the best-known and most highly-esteemed men in his own county, but one of those who, in these days of universal brotherhood and cosmopolitan sympathies, are year by year becoming rarer. Hence he speaks from the heart when he expresses himself as in his opening paragraphs:—

"The county of Wilts has been sometimes thoughtlessly said to be poor in Ornithology; indeed, I have heard it denounced by superficial observers as exceptionally wanting in the various members of the feathered race; pre-eminent, doubtless, in the remains of antiquity—so these gentlemen are good enough to allow—but in birds a barren field indeed. Against any such verdict I enter a decided protest, and I even maintain, on the contrary, that, taking into consideration that Wiltshire is an inland district, and therefore cannot be expected to abound in birds whose habitat is the sea and the sea-shore, our county will scarcely yield to any other, similarly situated, in the number and variety of the species of birds to be found there; and I now proceed to prove this by statistics.

"Let us first, however, examine the physical aspect of Wiltshire, and we shall see that it is not composed of bleak open downs alone, as its detractors superciliously affirm, but that it can show a great diversity of scenery, and much of it of surpassing beauty. We have, it is true, our broad, open, expanding downs—and what native of Wiltshire does not glory in them and admire them?—but we have at the same time our richly-timbered vales: if we have hill, we have also dale; if we have open plains, we have also large woods and thick forests. Where shall we find more clear and limpid streams, where more green and laughing meadows, than in the valley of the Avon (the northern and southern Avon), the vale of Kennet, or of Pewsey, or of Wily, or of Wardour? Where, again, in all England can we meet with a forest to compare with that of Savernake? And in woods and parks and well-timbered estates, both in the north and south of the county, we are exceptionally rich" (pp. 1, 2).

All who have traversed Wiltshire will readily allow the truth of these words, skilfully put together as they are by our author, in regard to the pleasing variety which its landscape in several parts exhibits, yet it must be confessed that the variety is limited in extent—the same features recurring over and over again, so that one range of downs or one valley repeats another. Both down and valley are alike enjoyable to the utmost, but the contrast between them is mild when compared with that afforded by hill and dale in many another county; and, above all, whatever may be the reason of it, Nature in Wiltshire wears an aspect of sameness, which, after a few days, becomes almost distressing to the stranger, because it is disappointing, though the native may very likely rejoice in the absence of everything that suggests a wild country; and a wild country, it should be needless to observe, gives the hope, if not its realization, of a plentiful crop of birds.

Though we fully admit the strong temptation to which a faunistic writer is exposed of magnifying the area of his field of work, it has been our duty before now in these columns to condemn the inconsiderate yielding to that temptation; and, with the utmost regard for our present author, we are compelled to say that he has fallen—perhaps not so deeply as others—into this besetting sin. We must repeat what we have so often urged before. The real interest (not only scientific, but even sentimental) of a fauna lies in its proper inhabitants—those that are entitled to all the rights and privileges of citizenship—and not in those adventitious aliens,

'Blown from over every main"

—strangers which are the sport of fate, and to whom the offer of letters of naturalization is not only a mockery—for if chance allows they are invariably killed—but an insult to the rightful denizens of the district. However, even on the unprincipled principle—which, by the way, is only admitted in ornithology among all the many branches of natural history—that a species once showing itself in a district should be scored to that district's credit, some proof of the alleged appearance is needed before it be accepted as a fact. Experience proves that there are few compilers of faunas, especially ornithological faunas, who are not ready, we will not say to strain a point, but to receive favourable evidence on easy terms; and indeed a rigid examination of all claims to admission, with a stern rejection of those that cannot be substantiated, is a virtue which has hardly been cultivated until within these later days, and not often even recently.

We have just said that in this respect Mr. Smith is not a grievous sinner; and, after examining his list pretty carefully, we find but sixteen species that we think ought, almost without any doubt, to be excluded on one ground or another—whether the ground be insufficient testimony, manifest importation, or from their proper habitat being so far distant as to render it nearly certain that their recognition within the boundaries of the county was only the accident of an accident. But how much stronger would Mr. Smith's list be if these sixteen species were omitted? and how much stronger still if the (say) forty irregular visitants were also subtracted? Then, and only then, would the ordinary reader know the wealth of Wiltshire ornithology; and, for an inland county, presenting (as we have stated) a not very diversified area, and mainly composed of one geological formation, a very respectable comparison could be made, we are confident, with any other county, however favourably situated. We

are not going to make the calculation—indeed, for comparison's sake, the statistics of many other counties are as yet wanting; but we think it would appear that not many English shires would show a more creditable roll of real inhabitants, whether breeding within its borders, or so regularly visiting it at fixed seasons as to deserve recognition as denizens. Of the former, we think Wiltshire could fairly claim 100, and of the latter 50, making the respectable number of 150, to which might be added 29 for irregular visitants to be legitimately included, after deducting the aforesaid (16 + 40 =) 56 from Mr. Smith's total of 235. Our author may think very hardly of us for thus diminishing the ornithic wealth of his county, but we assure him that he would have little cause to complain of the result were the same rule applied to the so-called "avifaunæ" of other inland shires.

This, however, is a theme we will not pursue. Rather let us speak of the manifest merits of Mr. Smith's volume. One of them stands out pre-eminently in that he has accorded so much space to two species very interesting to all who care for English birds—the Raven and the Heron. Of the former, which in days not so long past had numerous homes in Wilts, an account is given which in years to come will, we are sure, be regarded as of the highest interest, for it is compiled from information obtained by no fewer than 110 correspondents in various parts of the county, and is in itself a proof of well-directed energy. The result is, of course, a mournful one.

"It will be seen that the history of the Ravens of Wiltshire is, alas! rather a history of that which is past and gone than of that which is flourishing to-day; so persecuted, shot down, trapped, and despoiled of their young have these noble birds been at the hands of ruthless gamekeepers and others, who have gone upon the false issue that they are very destructive to game, whereas, with the exception of an occasional raid on a leveret or a rabbit, they do little harm in the preserves, for the Raven cannot bear an inclosed district—he must have plenty of room to disport himself; and as to being 'cabin'd, cribb'd, confin'd' within narrow woods, he eschews them altogether, and only during the breeding-season will he consent to occupy some big tree in the park, generally the highest and most inaccessible he can find, and there he and his mate return, year after year, to occupy their accustomed nursery" (p. 222).

It would seem, from Mr. Smith's information, that out of the twenty-two localities he names, sixteen have wholly ceased to be tenanted by this species, four are doubtful, and in *two* only has the bird certainly still a home. But how many English counties could claim such a distinction as that? Some of the larger landowners, as the Duke of Beaufort, Lords Bath and Pembroke (to their credit be it said), have been disposed to protect this very interesting and (as the writer from his own experience can assert) comparatively harmless species; but gamekeepers' prejudices are almost beyond control, and probably nothing short of a reward given on the hatching-off of a ravenry, combined with dismissal on the murder of a breeding-bird, would insure protection. A scientific man naturally shuns sentiment as such, but curious it is that the owners of historic estates do not perceive the value of all their historic associations; and an ancient Raven-tree, still occupied by the descendants of many a generation, would be no mean adjunct to the glories of Badminton or Bowood, Longleat or Wilton! Where the proprietor does not exert himself, the doom of the species is as certain as that of the Bustard has proved to be.

The Bustard, in popular opinion, is always more associated with the Wiltshire Downs or Salisbury Plain than with any other part of England. But needless to observe that herein, as usual, popular opinion is wrong, and anyone who seeks will find that in reality the association terminated much longer ago than in four or five other counties. Mr. Smith naturally devotes a good deal of space—much of it being, we regret to say, wholly beside the purpose—to this grand bird; and indeed its gilt figure decorates the cover of his volume. We must, however, express ourselves somewhat disappointed at the result, though it is one not unexpected. The statements of the editor of Pennant in 1812, and of Montagu in 1813, are confirmed, and in a small degree supplemented; but, says our author:—

"After this I have no record on which I can rely of any native Wiltshire Bustard; but I have had many statements, to which I listened attentively, from thirty to forty years ago, from old shepherds, farmers, and labourers, several of whom could well recollect seeing these birds on the downs in their early days, but from whom I could obtain no reliable information as to date; for the Wiltshire countryman, good honest soul, is not observant of detail, and as to dates he ignores them altogether—'a long whiles ago' conveniently covering half a century. However, by putting together the information gained from many sources, and by comparing the several statistics which I thought reliable, I arrived at the opinion (perhaps somewhat indistinct and hesitating) that our Wiltshire Bustard lingered on till about the year 1820" (pp. 355, 356.)

This date may be approximately correct; but it is undeniable that for several years later the Bustard inhabited the Wolds of Yorkshire and Lincolnshire, and was not extirpated in Suffolk in 1832, nor in Norfolk until 1838; since which time all the examples that have occurred in England (Wiltshire included) may rightly be regarded as foreign visitors.

Mr. Smith's account of the Heron, before mentioned, is as satisfactory as that which he gives of the Raven; but here it must suffice to say that Wiltshire boasts of seven heronries, besides twenty-two offshoots. Some of the former, however, are but recently established, and fresh colonies are always forming; for in this county, as elsewhere in England, is observable the tendency of these birds to break up and colonize—a fact almost undoubtedly due, as has been pointed out by more than one writer, to the increased difficulty of finding in one spot food for their young, induced by the more complete drainage of the country.

We have left ourselves no space for other matters on which we should like to dwell, as the honest enthusiasm of our author makes us a little blind to his faults—whether of omission or commission—the latter certainly predominating; for in his desire to give information to his readers he says a great deal more than is necessary in a faunistic work, especially as to classification, nomenclature, structure, and so forth—all matters that are best left to experts, and their treatment (which is far from perfect) only swells the volume to an uncomfortable size. We also freely excuse his many old-fashioned ways, which will, however, be no blemish, if they be not a positive blessing, in the eyes of most of his readers. The

most severe critic must admit that the style, without being in the least laboured, is far superior to that of the ordinary writer on natural history, and the book is consequently in the highest degree readable. Many a Wiltshire man, woman, and child will have reason to be grateful to Mr. Alfred Charles Smith.

A HAND-BOOK FOR TRAVELLERS.

Führer für Forschungsreisende. Anleitungen zu Beobachtungen über Gegenstände der physischen Geographie und Geologie. Von Ferdinand Freiherr von Richthofen. (Berlin: Oppenheim, 1886.)

IT is now thirteen years since Dr. Neumayer issued his "Anleitung zu wissenschaftlichen Beobachtungen auf Reisen," a joint production of himself and representatives of various departments of science, the geological section having been contributed by the present author. The volume now under consideration is virtually an enlarged and completely revised edition of that section, which it seemed desirable to publish separately. A re-issue of the complete work is, however, in contemplation. The qualifications which Von Richthofen possesses for the task he has undertaken are of no common order. Himself a traveller of wide experience, whose work on China deservedly ranks as one of the classics of geographical literature, he brings to bear upon his subject a wealth of practical knowledge combined with scientific attainment, in which few are his equals.

In the preface it is explained that the primary object of the work is to enable those travellers whose previous scientific training is not extensive, such as missionaries, merchants, and others, who may be thrown in regions but little explored, to make observations which shall be of permanent value. Under these circumstances, no attempt is made to furnish the reader with references to the literature of the subject which would almost certainly be inaccessible to him, although notice is taken here and there of modern treatises on particular questions. The body of the work opens by an introduction, the scope of which may best be indicated in a general way by stating that it contains such headings as "Outfit," "Modes of Travelling," and "Miscellaneous Practical Hints." These last are especially valuable, and might with advantage be carefully studied by anyone who is starting on a first expedition, on account of their eminently suggestive and practical character. The emphasis laid upon the necessity of noting all observations on the spot, and even upon such minutiæ as having the pencil suspended round the neck so as to be always ready, indicates an experience of the temptations to procrastination which beset travellers in common with humanity at large. Among other divisions of this section may be mentioned "Measuring and Drawing," in which sufficient directions are given for mapping unexplored countries in a preliminary fashion, and also "Climatic and Biological Observations," the latter of which are treated with extreme brevity, as not falling within the author's special province.

The next portion of the book is entitled "Observations upon Externally Modifying Processes," and includes chapters upon rocks and soils, on springs and flowing water. It contains a dissertation of some length on the important subject of glaciers, in which the phenomena accompanying their present existence, as well as the traces of their past action, are carefully described. In another chapter an abstract is given of the present state of our knowledge regarding coral reefs and islands. In addition to the time-honoured theory of Darwin, the most recent researches of Semper, Rein, Murray, and Studer are summarized; one misses, however, the name of Agassiz in this connection, and it is noticeable that, although Dana's soundings off Tahiti are quoted in some detail, no mention is made of the series executed by the *Challenger*, although their results agree well with the author's diagrammatic section of a reef. No one theory is embraced to the exclusion of all others, but stress is laid upon the need for further investigation, and upon the fact that "each reef has its own special history of origin and development." Upraised coral reefs are indicated as being likely to throw light on the question—a suggestion which has been independently carried out by Dr. Guppy in the Solomon Islands with such brilliant results. A few pages give what is known regarding the changes of level of the ocean, and the terms "positive" and "negative displacement" are adopted instead of "sinking" and "upheaval" of the land respectively.

The third section is devoted to "Observations on the Crust of the Earth, on Rocks, and on Mountain Structure." It contains an outline of the principal facts of petrology and of stratigraphical geology.

The author treats his subject in considerable detail; his volume occupies more than 700 pages—that is, a somewhat larger bulk than the whole of Neumayer's original work. Indeed, if a fault is to be found in the book, we should be disposed to say that, considering the fact that only one aspect of Nature is discussed, the amount ot detail is rather excessive. If botany, zoology, anthropology, and all the other matters which have an equal claim upon the traveller's attention, were elaborated in the same fashion, the result would be an encyclopædia of no small dimensions. The work is, however, thoroughly practical in character. There are no lengthened discussions upon abstract questions, but divergent theories regarding unsettled points are summarized in such a way as to indicate how both the traveller who has time at his disposal, and also he who is compelled to hasten through the country, can each make the best use of their respective opportunities.

W. E. H.

OUR BOOK SHELF.

Geometry in Space. Edited by R. C. J. Nixon, M.A. "Clarendon Press Series." (London: Henry Frowde, 1888.)

THIS book is a sequel to "Euclid Revised" by the same author. It consists of one hundred pages, divided into three chapters and an appendix. The first chapter is devoted to the discussion of planes and solid angles, covering much the same ground as Euclid's eleventh book; it contains, besides, some very useful notes on elementary perspective and the drawing of solid figures. This is an excellent feature of the book, and the author might with advantage have given more than a couple of pages to it, for there is no doubt that, to most students, the representation of solid figures, other than the simplest, is a real and often a permanent stumbling-block to the development of the science in their own minds. The second chapter is concerned with polyhedra. It begins with Euler's theorem establishing a linear relation between the numbers of edges, corners, and faces, and Listing's

extension of it. In giving the latter the author speaks of "facets," "sheets," and "interfaces," without having previously defined them, thus leaving a student in some little difficulty as to their precise meaning. Considering the great analytical interest of the algebraical researches of Klein and Cayley in the polyhedral functions and the finite groups of linear substitutions, which represent geometrically the production of congruence of figure by the rotations of the corresponding polyhedra, we think it would add greatly to the interest of the book to show the elementary geometrical relations which interpret the algebraical operations. The mensuration and usual properties of the simple solids are worked out, the method of limits being freely employed. The third chapter is of "Solids of Revolution," and includes Pappus's theorems of mensuration, the extension of the modern geometry of lines and circles to planes and spheres, and an elementary account of surface spherics.

The appendix, which treats of the "Geometrical Theory of Perspective in Space," is from a paper in the *Quarterly Journal of Mathematics* for 1886, by Mr. Alexander Larmor, of Clare College, Cambridge; it contains ten important theorems in the subject.

Throughout the book great brevity of expression is employed with taste and discretion. It bears traces of careful compilation, and is certainly well and suitably printed and illustrated. Interesting theorems and problems are given as exercises at the end of each chapter.

The work may be safely recommended to students and teachers as a clear and precise introduction to the study of solid geometry.

Chambers's Encyclopædia: a Dictionary of Universal Knowledge. (London: William and Robert Chambers, 1888.)

THE process of revising and altering a work of this kind is no easy task. As the publishers tell us, "much has happened during the twenty years it has been before the public which necessitates a different treatment of many articles." This new edition has been thoroughly revised, new articles having been written, and the old ones gone over by eminent authorities, as may be seen from the following list: Alchemy and Atomic Theory, by Prof. Crum-Brown; Ant, by Sir John Lubbock; Alps, by Prof. James Geikie; Arctic, Antarctic, and Atlantic Oceans, by Mr. John Murray; and Atom, by Prof. Tait. While such well-known names as these will command universal respect and confidence, it is to be regretted that some of the subjects, such as that of Astronomy—to take an instance—should leave much to be desired in this particular.

The work is carried out on exactly the same lines as the original edition, the subjects being treated, not in great detail, but so as to afford information interesting to any more or less educated person.

American and colonial subjects are dealt with in this edition more than in former ones, the more important articles on American subjects being written by American authors especially for this re-issue.

The number of maps, both geographical and physical, has been increased, and the illustrations are more numerous, and supersede those of former editions. The printing throughout is excellent.

Messrs. Chambers are to be congratulated upon the issue of a work which, from its merits, deserves to find a place in every home.

Leitfaden der Zoologie für die oberen Classen der Mittelschulen. Von Dr. Vitus Graber. Mit 502 Abbildungen im Texte (darunter 62 farbige) und einem Farbendruckbilde. (Wien: F. Tempsky, 1887.)

EVEN in these days of cheap books, it is surprising to find an octavo volume of nearly 250 pages, with over 500 illustrations, published for the price of less than three shillings of our money. When we add that the information, though of necessity very much condensed, is not only good and exact, but in most cases quite up to date, we have said all that is needed to call our readers' attention to this little volume.

The coloured illustrations in the text are wonderfully effective; one gives a representation of one of Schulze's sections through a Sponge, printed in two colours, in which the horny framework is represented yellow, the pore-canal system blue.

It is interesting to note that at a time when in this country the study of biology is not encouraged in our schools, when it is omitted from the programme of our intermediate education examinations, it should be so taught in the intermediate schools in Austria as to call for the production of such an excellent and cheap introduction to its study.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

"Coral Formations."

I SHOULD be trespassing too much on the kindness of the Editor of NATURE if I were to refer to all the numerous novel and interesting points in Mr. Bourne's description of Diego Garcia. The retrospective character of the account is something new in the instance of an atoll; and it is not often that a naturalist is able to add to his own observations the twenty-five years' experience of an observer like M. Spurs.

I am, however, at a loss to understand why Mr. Bourne is unable to assent to the theory of subsidence. Prof. Dana, who long since referred to the evidence of upheaval in the atoll regions of the Pacific, nevertheless did not regard such evidence as negativing the theory he supported, nor, in fact, did Mr. Darwin himself. The testimony most required to overturn the theory of subsidence is the testimony which the supporters of that view will accept. I do not find such evidence in Mr. Bourne's paper.

I am also in doubt as to the position of the writer of the paper in regard to Mr. Murray's views. In disagreeing with the importance which Mr. Murray attaches to the agency of solution, he makes no attack on the main position of the new explanation, viz. the building up of the foundations of atolls by organic deposits. Does Mr. Bourne accept this view?

H. B. GUPPY.

I HAVE been much interested by the discussion on coral formations which recently appeared in NATURE, and I venture to send you an extract from a journal kept during my stay in Massowah.

"*Massowah, February* 1888.—The whole of the harbour is fringed with coral reefs formed by species of *Madreporaria* (*perforata*), extending in places a considerable distance from the high-water mark (Turtle Island, for example); in other parts the edge of the reef is quite close to land, and in each case there is less water immediately over the edge of the reef than there is a little way in shore. The outer edges of the reefs go down almost perpendicularly to a depth of 4 or 5 fathoms, while towards the shore the water deepens, at first rather quickly to 3 or 4 feet, then gradually becomes shallow to the beach. The bottom, inside the edges of the reefs, is composed of fine grayish mud—composed chiefly of a mixture of disintegrated coral and fine drift alluvial sand which is blown over from the mainland—while the bottom of the harbour is nearly black mud. Here and there, just inside the edges of the reefs, are found pieces of living coral broken off from the outer edges. Every evidence here shows that the land is rising.

"Large masses of coral much altered by the rain are to be found on the plains of Massowah, which extend three or four miles in south-west, west, and north-west directions. They show unmistakable signs of the undermining action of the sea,

which can still be seen going on around the coast and harbour. At Mokullo, at a depth of 20 feet, I observed masses of coral (*Aperosa*) almost perfect in shape, covered up with alluvium. It is probable that the whole coast from the mountains has been reclaimed by the action of coral builders, and that eventually the group of islands outside will be joined to the mainland."

I noticed a similar formation of the coral reefs in Suakim Harbour; while at Key West, Florida, there was no lessening of the depth of the water on the edge of the reefs.

DAVID WILSON-BARKER.

THE following table, showing some of the results of work done in connection with the solubility of carbonate of lime in sea-water will be of interest. The difference in solubility between heavy dense corals and the lighter porous varieties is very marked.

TABLE I.—*Showing Solubility oj Carbonate of Lime, under different forms, in Sea-water, in grammes per litre.*

Material used.	Temperature.	Exposure.	Mean amount of $CaCO_3$ taken up.	Number of determinations made.
	° C.	Hours	Grm.	
Dead coral, Porites	27	12	0·395	3
Coral sand	27	12	0·032	5
Harbour mud, Bermuda	27	12	0·041	2
Isophyllia dipsacea (Dana), Bermuda	27	12	0·041	6
Millepora ramosa (Pallas), Bermuda	27	12	0·036	7
Madrepora aspera (Dana), Mactan Island, Zebu	27	12	0·073	7
Montipora folioso (Pallas), Amboyna	27	12	0·043	7
Gomastræa multilobata (Qualch), Amboyna ...	10	12	0·073	3
Porites clavaria (Lamk.), Bermuda	10	12	0·093	2

TABLE II.

Weathered oyster-shells	10	12	0·331	3
Mussels allowed to rot in sea-water seven days...	27	168	0·384	2
Crystallized carbonate of lime	10	12	0·123	2
a Amorphous carbonate of lime (freshly prepared)	10	—	0·649	2
b Ditto ditto ditto	−1·66	—	0·610	2
Melobesia, Kilbrennan Sound, Scotland ...	10	12	0·089	3

a and *b*. The carbonate of lime was added as long as it dissolved.

The figures in Table II. will give Mr. T. Mellard Reade facts (so far as laboratory experiments may) upon which to found reasonable views. Mr. George Young, who has made all the determinations under my direction, is one of the chemical staff attached to the Marine Station here.

ROBERT IRVINE.

Royston, Granton, near Edinburgh, April 16.

Note on a Problem in Maxima and Minima.

I SUPPOSE most lovers of elementary geometry who read the communication on the above subject from Mr. Chartres in NATURE of February 2 (p. 320) admired the simple investigation he gave of the problem.

I should like, however, to point out—

(1) That it might be made still more elementary by proving $EB + EC = ED$ without the aid of Book VI.

Let E be any point on the arc of the circumcircle of an equilateral triangle BDC on which the angle D stands, and on ED as diameter describe a circle cutting EB, EC in B′, C′.

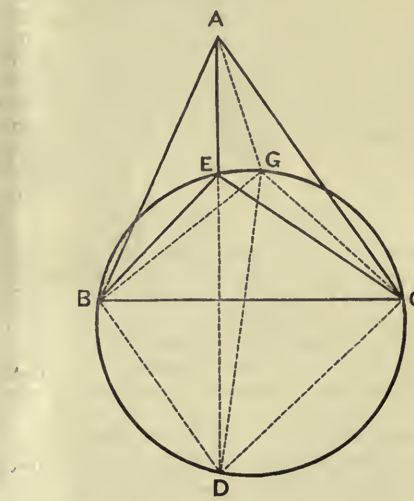
$$\text{Then } \angle B'C'D = \angle BED = \angle BCD.$$
$$\text{Similarly } \angle C'B'D = \angle CBD;$$
$$\therefore \angle B'DC' = \angle BDC;$$
$$\therefore B'C'D \text{ is equilateral.}$$

Hence B′E, EC′ are sides of a regular hexagon inscribed in the circle B′C′D.

$$\therefore B'E + EC' = ED.$$
$$\text{Again, } BD, DB' = CD, DC',$$
$$\text{and } \angle BDB' = \angle CDC';$$
$$\therefore BB' = CC';$$
$$\therefore BE + EC = B'E + EC'$$
$$= ED.$$

(2) If we assume Ptolemy's theorem (conventionally quoted as Euclid, VI. D) we may as well assume the known extension of it to acyclic quadrilaterals given in Todhunter's "Euclid," p. 318, and at the same time generalize the problem thus—

To find a point E within a triangle such that $l \, . \, AE + m \, . \, BE + n \, . \, CE$ may be a minimum; l, m, n being such that any two are together greater than the third.



On BC describe a triangle BCD such that $BC : CD : DB :: l : m : n$; the point required will be the intersection E of AD with the circumcircle of BCD if E is within the triangle ABC.

$$\text{For } BE \, . \, CD + CE \, . \, BD = ED \, . \, BC,$$
$$\therefore m \, . \, BE + n \, . \, CE = l \, . \, ED;$$
$$\therefore l \, . \, AE + m \, . \, BE + n \, . \, CE = l \, . \, AD.$$

But if G is any other point on the arc BEC,

$$m \, . \, BG + n \, . \, CG = l \, . \, GD;$$
$$\therefore l \, . \, AG + m \, . \, BG + n \, . \, CG = l \, . \, AG + l \, . \, GD;$$
$$\therefore l \, . \, AG + m \, . \, BG + n \, . \, CG > l \, . \, AD.$$

And if P be any point within the triangle ABC, but not on the circumference—

$$BP \, . \, CD + CP \, . \, BD > PD \, . \, BC \text{ (Todhunter's "Euclid," [p. 318]);}$$
$$\therefore m \, . \, BP + n \, . \, CP > l \, . \, PD;$$
$$\therefore l \, . \, AP + m \, . \, BP + n \, . \, CP > l \, . \, AP + l \, . \, PD;$$
$$\therefore l \, . \, AP + m \, . \, BP + n \, . \, CP > l \, . \, AD.$$

If l, m, n are proportional to a, b, c, E is the orthocentre of ABC.

If l, m, n are proportional to c, a, b, or b, c, a, E is one of the Brocard points of ABC, and the construction for E is equivalent to that of Mr. R. F. Davis for the Brocard points ("Reprint of Mathematics from the *Educational Times*," vol. xlvii. App. II.).

It will, of course, be seen that the triangle formed by drawing perpendiculars to AE, BE, CE through A, B, C, is the maximum triangle with its sides proportional to l, m, n and passing through A, B, C. Prof. Genese has kindly supplied me with an elementary investigation of the problem, depending on the construction of that triangle.

It may also be seen that the question has an intimate connection with one proposed by Mr. Morgan Jenkins in the *Educational Times* for August 1, 1884:—

If on the three sides of a triangle, ABC, there be described any three triangles, BDC, CEA, AFB, either all externally or all internally having their angles in the same order of rotation, and the angles which are contiguous to the same corner of ABC equal to each other, prove that AD, BE, and CF meet in a point O, which is also the common point of intersection of the circumcircles of BDC, CEA, AFB ("Reprint," vol. xliii. pp. 88–91).

EDWARD M. LANGLEY.

Bedford, April 14.

Self-Induction.

I FIND I am being quoted as having said that an iron conductor has less self-induction than a copper one. You will perhaps spare me a line to disclaim any such statement. It is one which seems to me on the face of it absurd.

OLIVER J. LODGE.

SUGGESTIONS ON THE CLASSIFICATION OF THE VARIOUS SPECIES OF HEAVENLY BODIES.[1]

II.

II.—CLASSIFICATION.

I. FORMER CLASSIFICATIONS OF STARS.

IN the various classifications of the celestial bodies which have been attempted from time to time, nebulæ and comets have been regarded as things apart from the stars; but from what I have stated in the first part of this paper, relating to the origin of the various groups of heavenly bodies, it is clear that it is not only unnecessary but unphilosophical to make such a separation; and indeed, if any such separation were needed, such a result would seem to indicate that the line of evolution is by no means so simple and clear as it really seems to be. But although it is no longer necessary to draw this distinction, it is important that I should state the various spectroscopic classifications which have been attempted in the case of the stars. With this information before us, we shall be better able to see the definite lines on which any new classification must be based to include all celestial forms.

Fraunhofer, Rutherfurd, and Secchi.

When we inquire into the various labours upon which our present knowledge of the spectra of the various orders of "stars" are based, the first we come across are those of Fraunhofer, who may be said to have founded this branch of scientific inquiry in the year 1814.

Fraunhofer not only instituted the method of work which now is found to be the most effective, but his observations at that time were so excellent that he had no difficulty in finding coincidences between lines in the sun and in Venus.

Fraunhofer's reference to his observations runs as follows:—

"I have also made several observations on some of the brightest fixed stars. As their light was much fainter than that of Venus, the brightness of their spectrum was consequently still less. I have nevertheless seen, without any illusion, in the spectrum of the light of Sirius, three large lines, which apparently have no resemblance with those of the sun's light. One of them is in the green, and two in the blue space. Lines are also seen in the spectrum of other fixed stars of the first magnitude; but these stars appear to be different from one another in relation to these lines. As the object-glass of the telescope of the theodolite has only thirteen lines of aperture, these experiments may be repeated, with greater precision, by means of an object-glass of greater dimensions."[2]

He did not attempt to classify his observations on stellar spectra, but, as pointed out by Prof. Dunér ("Les Étoiles à Spectres de la Troisième Classe," p. 3), those that he most particularly mentions are really remarkably diverse in their characteristics.

In these researches Fraunhofer was followed by Rutherfurd, who, in the year 1863, was the first to indicate that the various stellar spectra which he had then observed were susceptible of being arranged into different groups. His paper was published in *Silliman's Journal* (vol. xxxv. p. 71), and, after giving an account of the observations actually made, continues as follows:—

"The star spectra present such varieties that it is difficult to point out any mode of classification. For the present, I divide them into three groups:—First, those having many lines and bands, and mostly resembling the sun, viz. Capella, β Geminorum, α Orionis, Aldebaran, γ Leonis, Arcturus, and β Pegasi. These are all reddish or golden stars. The second group, of which Sirius is the type, presents spectra wholly unlike that of the sun, and are white stars. The third group, comprising α Virginis, Rigel, &c., are also white stars, but show no lines; perhaps they contain no mineral substance, or are incandescent without flame."

Soon afterwards Secchi carried on the inquiry, and began in 1865 by dividing the objects he had then observed into two types. These two types were subsequently expanded in 1867 into three ("Cataloge delle Stelle di cui si e determinato lo Spettro Luminoso," Secchi, Parigi, 1867): first, white stars, like α Lyrae; secondly, yellow stars, like Arcturus; and, thirdly, deeply coloured stars, like α Herculis and α Orionis. The order of these types was not always as stated, but I have not been able to find the exact date at which the order was changed (Dunér, "Sur les Étoiles," p. 128). Secchi subsequently added a fourth type, in which the flutings were less numerous. There is little doubt that Secchi was led to these types not so much by any considerations relating to the chemical constitution of the atmospheres of these bodies as in relation to their colours. His first classifications, in fact, simply separated the white stars from the coloured ones (see on this point "Le Scopirte Spettroscopiche," P. A. Secchi, Roma, 1865).

The fourth type included, therefore, stars of a deeper red colour than those of the third, and Secchi pointed out that this was accompanied by a remarkable change in the spectrum; in fact, of Secchi's four types thus established, the first and second had line spectra and the third and fourth had fluted ones. At that time the important distinction to be drawn between line- and fluted-spectra was not so well recognized as it is at present; and further the relation of spectra to temperature was not so fully considered. Secchi, as a result of laboratory work, however, at once showed an undoubted connection between the absorption flutings in the stars of the fourth type and those seen in the spectrum of carbon under certain conditions; and although this conclusion has been denied, it has since been abundantly confirmed by Vogel and others (see Vogel, *Publicationen*, Potsdam, No. 14, 1884, p. 31).

Relation to Temperature.

At the time that Secchi was thus classifying the stars, the question was taken up also by Zöllner, who in 1865 first threw out the suggestion that the spectra might probably enable us to determine somewhat as to the relative ages of these bodies; and he suggested that the yellow and red light of certain stars were indications of a reduction of temperature (Zöllner, "Photometrische Untersuchungen," p. 243).

In 1868 this subject occupied the attention of Ångström with special reference to the contrasted spectra of lines and flutings. On this he wrote as follows, showing that temperature considerations might help us in the matter of variable stars ("Recherches sur le Spectre solaire," Upsala, 1868):—

"D'après les observations faites par MM. Secchi et Huggins, les raies d'absorption dans les spectres stellaires sont de deux espèces: chez l'une, le spectre est rayé de lignes très-fines, comme le spectre solaire; chez l'autre, les raies constituent des groupes entiers à espaces égaux ou des bandes nuancées. Ces derniers groupes appartiennent vraisemblablement aux corps composés, et je mentionnerai, en particulier, que ceux trouvés dans le spectre de α Orionis ressemblent fort aux bandes lumineuses que donne la spectre de l'oxyde de manganèse. Supposé que ma théorie soit juste, l'apparition de ces bandes doit donc indiquer que la température de l'étoile est devenue assez basse pour que de telles combinaisons chimiques puissent se former et se conserver.

"Entre ces deux limites de température chez les étoiles, limites que l'on peut caractériser par la présence de l'une ou de l'autre espèce des raies d'absorption, on peut s'imaginer aussi un état intermédiaire, dans lequel les gaz

[1] The Bakerian Lecture, delivered at the Royal Society on April 12, by J. Norman Lockyer, F.R.S. Continued from p. 590.

[2] "On the Refractive and Dispersive Power of Different Species of Glass, with an Account of the Lines which cross the Spectrum," Fraunhofer, translated in *Edin. Philosophic Journal*, vol. x., October to April, 1823–24, p. 39.

composés peuvent se former ou se dissocier, suivant les variations de température auxquelles ils sont assujettés par l'action chimique même. Dans cette classe doivent probablement être comprises les étoiles dont l'intensité de lumière varie plus ou moins rapidement, et avec une périodicité plus ou moins constante."

In the year 1873, I referred to this subject in my Bakerian Lecture (*Phil. Trans.* vol. clxiv. pt. 2, 1873, p. 492), in which I attempted to bring to bear some results obtained in solar inquiries upon the question of stellar temperatures. I quote the following paragraphs :—

I. The absorption of some elementary and compound gases is limited to the most refrangible part of the spectrum when the gases are rare, and creeps gradually into the visible violet part, and finally to the red end of the spectrum, as the pressure is increased.

II. Both the general and selective absorption of the photospheric light are greater (and therefore the temperature of the photosphere of the sun is higher) than has been supposed.

III. The lines of compounds of a metal and iodine, bromine, &c., are observed generally in the red end of the spectrum, and this holds good for absorption in the case of aqueous vapour.

Such spectra, like those of the metalloids, are separated spectroscopically from those of the metallic elements by their columnar or banded structure.

IV. There are, in all probability, no compounds ordinarily present in the sun's reversing layer.

V. When a metallic compound vapour, such as is referred to in III., is dissociated by the spark, the band spectrum dies out, and the elemental lines come in, according to the degree of temperature employed.

Again, although our knowledge of the spectra of stars is lamentably incomplete, I gather the following facts from the work already accomplished with marvellous skill and industry by Secchi, of Rome.

VI. The sun, so far as the spectrum goes, may be regarded as a representative of class (β) intermediate between stars (α) with much simpler spectra of the same kind and stars (γ) with much more complex spectra of a different kind.

VII. Sirius, as a type of α, is (1) the brightest (and therefore hottest ?) star in our northern sky ; (2) the blue end of its spectrum is open,—it is only certainly known to contain hydrogen, the other metallic lines being exceedingly thin, thus indicating a small proportion of metallic vapours ; while (3) *the hydrogen lines in this star are enormously distended*, showing that the chromosphere is largely composed of that element.

There are other bright stars of this class.

VIII. As types of γ the red stars may be quoted, the spectra of which are composed of channelled spaces and bands, and in which naturally the blue end is closed. Hence the reversing layers of these stars probably contain metalloids, or compounds, or both, in great quantity ; and in their spectra not only is hydrogen absent, but the metallic lines are reduced in thickness and intensity, which in the light of V., *ante*, may indicate that the metallic vapours are being *associated*. It is fair to assume that these stars are of a lower temperature than our sun.

In the same year, in a letter to M. Dumas, published in the *Comptes rendus*,[1] I again pointed out that, if we consider merely the scale of temperature, a celestial body with flutings in it would be cooler than one which had lines in its spectrum ; and I also pointed out that, taking the considerable development of the blue end of the spectrum in white stars as contrasted with its feeble exhibition in stars like our sun, we had strong presumptive evidence to the effect that the stars like α Lyræ, with few lines in their spectra, were hotter than those resembling our sun, in which the number of lines was very much more considerable, and I added an inference from this : "plus une étoile est chaude, plus son spectre est simple." This related merely, as I have said before, to the consideration of one line of temperature.

[1] "Il semble que plus une étoile est chaude, plus son spectre est simple, et que les éléments métalliques se font voir dans l'ordre de lines poids anatomiques. Ainsi nous avons :—

"(1) Des étoiles très brillantes, où nous ne voyons que l'hydrogène *en quantité énorme*, et le magnésium.

"(2) Des étoiles plus froides, comme notre soleil, où nous trouvons:—

H + Mg + Na.
H + Mg + Na + Ca. Fe, &c. ;

dans ces étoiles, pas de métalloïdes.

"(3) Des étoiles plus froides encore, dans lesquelles tous les éléments métalliques sont associés, où leurs lignes ne sont plus visibles, et où nous n'avons que les spectres des métalloïdes et des composés.

"(4) Plus une étoile est âgée, plus l'hydrogène libre disparait ; sur la terre, nous ne trouvons plus l'hydrogène en liberté."

Vogel's Classification.

In the year following my paper, the most considerable classification which has been put forward of late years was published by Dr. Vogel (*Astr. Nach.*, No. 2000), who, basing his work on the previous types of Secchi, and taking into account the inference I drew in my letter to Dumas, modified Secchi's types to a certain extent, but always along one line of temperature, the leading idea being, as I gather from many remarks made in Dunér's admirable memoir, to be referred to presently, that the classification is based upon descending temperatures, and that all the stars included in it are supposed at one time or other to *have had* a spectrum similar to that of α Lyræ.[1]

This classification is as follows :—

CLASS I. *Spectra in which the metallic lines are extremely faint or entirely invisible.*—The most refrangible parts, blue and violet, are very vivid. The stars are white.

(*a*) Spectra in which the lines of hydrogen are very strong.

(*b*) Spectra in which the lines of hydrogen are wanting.

(*c*) Spectra in which the lines of hydrogen and D_3 are bright.

CLASS II. *Spectra in which the metallic lines are numerous and very visible.*—The blue and violet are relatively weaker ; in the red part there are sometimes faint bands. The colour of the stars is clear bluish white to deep reddish yellow.

(*a*) Spectra with numerous metallic lines, especially in the yellow and green. The lines of hydrogen are generally strong, but never as strong as in the stars of Class I. In some stars they are invisible, and then faint bands are generally seen in the red formed by very close lines.

(*b*) Spectra in which besides dark lines and isolated bands there are several bright lines.

CLASS III. *Spectra in which besides the metallic lines there are numerous dark bands in all parts of the spectrum, and the blue and violet are remarkably faint.*—The stars are orange or red.

(*a*) The dark bands are fainter towards the red.

(*b*) The bands are very wide, and the principal are fainter towards the violet.

It is pointed out that if this classification be true, there must be links between all the classes given. Now it is perfectly obvious that if this classification includes in its view all the stars, and if there is a line of ascending as well as descending temperatures—that is to say, if some of the stars are increasing their temperatures, while others are diminishing them—the classification must give way.

It is not difficult to see, in the light of my communication to the Society of November 17, that it has given way altogether, and principally on this wise.

The idea which underlies the classification is that a star of Class I. on cooling becomes a star of Class II., and that a star of Class II. has as it were a choice before

[1] "Car selon la théorie il faudra que tôt ou tard toutes les étoiles de la première classe deviennent de la seconde, et celles-ci de la troisième" (Dunér).

it of passing to Class III.*a* or Class III.*b*. Thus under certain conditions its spectrum will take on the appearance of Secchi's third type, Class III.*a* (Vogel); on certain other conditions it will take on the appearance of Secchi's fourth type, Class III.*b* (Vogel). There is now, however, no manner of doubt whatever that Secchi's Class III.*a* represents stars in which the temperature is increasing, and with conditions not unlike those of the nebulæ—that is to say, the meteorites are yet discrete, and that they are on their way to form bodies of Class II. and Class I. by the ultimate vaporization of all their meteoric constituents. There is equally no manner of doubt that the stars included in Class III.*b* have had their day; that their temperature has been running down, until owing to reduction of temperature they are on the verge of invisibility brought about by the enormous absorption of carbon in their atmospheres.

Pechüle was the first to object to Vogel's classification, mainly on the ground that Secchi's types 3 and 4 had been improperly brought together; and my work has shown how very just his objection was,[1] and how clear-sighted was his view as to the true position of stars of Class III.*b*.

II. Proposed New Grouping of all Celestial Bodies according to Temperature.

Having, then, gone over the various classifications of stars according to their spectra, I now proceed to consider the question of the classification of celestial bodies from a more advanced point of view. I pointed out in the year 1886 that the time had arrived when stars with increasing temperatures would require to be fundamentally distinguished from those with decreasing temperatures, but I did not then know that this was so easy to accomplish as it now appears to be (NATURE, vol. xxxiv. p. 228); and as I have already stated, when we consider the question of classification at all, it is neither necessary nor desirable that we should limit ourselves to the stars; we must include the nebulæ and comets as well, and the question of variability does not really concern us, because it is as a rule in its extremest form the passage of a body giving one spectrum to a body giving another even if of a different type, owing to sudden changes of temperature.

[1] "M. Vogel a proposé une classification suivant les diverses phases de refroidissement indiquées par les spectres, dans laquelle il fait des types III. et IV. de Secchi deux subdivisions d'une même classe, III.*a* et III.*b*. Mais je trouve certaines difficultés négatives contre cette classification relativement au rôle qu'y joue la III.*b*. En effet, il est admis que le IV. type de Secchi se distingue nettement du III. type, non seulement par la position et la quantité des zones obscures, mais aussi par le fait très-remarquable, que les principales de ces zones sont bien définies et brusquement interrompues du côté du violette dans le III. type du côté du rouge dans le IV. Or, si le IV. type doit représenter une des phases de refroidissement, par lesquelles passent les étoiles, on peut faire deux hypothèses. La première est que le spectre du IV. type soit coordonné au spectre du III. type, de manière qu'il ait des étoiles, qui passent de la phase représentée par le II. type, à la phase représentée par le III. type, et d'autres, qui passent directement du II. type au IV. Mais cette hypothèse est inadmissible. Car on connaît de spectres entremédiaires entre le I. et le II. type, et entre le II. et III.; mais on ne connaît pas, à ce que je sache, de spectres du II. type tendant au IV. Reste donc l'hypothèse, que la phase de refroidissement, représentée par le spectre du IV. type, soit postérieure à la phase représentée par le III. type, de manière que les spectres des étoiles passent du III. au IV. type. Si ce passage se fait peu à peu, il devrait avoir des spectres entremédiaires entre le III. et le IV. type; mais quoique Secchi par exemple le 17 Jan., 1868, ait déterminé le spectre de l'étoile 273 Schjell., comme semblant entremédiaire entre le III. et le IV. type, il l'a plus tard reconnu du IV. type, et l'existence de spectres du III.-IV. type n'est nullement prouvée. On pourrait objecter que les étoiles du IV. type sont peu nombreuses et en général si petites que leurs spectres sont difficiles à voir, et que par conséquent il pourrait y avoir parmi ces spectres quelques-uns, qui se rapprochassent du III. type. Mais je réponds à cette remarque, que les spectres du III.-IV. type, indiquant une phase moins refroidie, devraient au contraire en général appartenir à des étoiles plus grandes que celles ayant des spectres du IV. type. Si on veut supposer que le passage du III. au IV. type se fasse subitement, ou par une catastrophe, pendant laquelle apparaissent des lignes brillantes, cette supposition même constituerait une différence physique bien plus distincte entre le III. et le IV. type, qu'entre le II. et le III.; et le IV. type représenterait une phase bien distincte, la dernière peut-être avant l'extinction totale. Le rôle physique du IV. type est donc encore si mystérieux, que j'ai cru pouvoir encore me conformer à l'exemple de d'Arrest, en suivant la classification formelle de Secchi."—C. F. Pechüle, "Expédition Danoïse pour l'Observation du Passage de Venus, 1882," p. 25 (Copenhagen, J. H. Schultz, 1883).

In the first classification on these lines, which is certain to be modified as our knowledge gets more exact, it is desirable to keep the groups as small in number as possible; the groups being subsequently broken up into sub-groups, or, as I prefer to call them, species, as the various minute changes in spectra brought about by variations of temperature are better made out.

In my paper of November 17 (NATURE, vol. xxxvii. p. 84), I gave a diagram of the "temperature curve," on which is shown the distribution of nebulæ and of stars as divided into classes by Vogel, on the two arms of the curve.

On one arm of this we have those stages in the various heavenly bodies in which in each case the temperature is increasing, while on the other arm we have that other condition in which we get first vaporous combination, and then ultimately the formation of a crust due to the gradual cooling of the mass in dark bodies like, say, the companion to Sirius. At the top we of course have that condition in which the highest temperature must be assumed to exist.

To begin, then, a more general classification with the lowest temperatures, it is known that the nebulæ and comets are distinguished from most stars by the fact that we get evidence of radiation. Absorption has been suspected in the spectra of some nebulæ,[1] and has been observed beyond all doubt in some comets.[2] But there are some stars in which we also get radiation, accompanied by certain absorption phenomena; but there is no difficulty in showing that these bodies are more special on account of their bright lines than on account of their absorption bands. We may therefore form the first group of bodies which are distinguished by the presence of bright lines or flutings in the spectrum.

The presence or absence of carbon will divide this group into two main divisions, which, however, we may neglect in the following very brief sketch which I give in advance of a more detailed treatment.

The first species in this group would contain the nebulæ, in which only the spectrum of the meteoric constituents is observed. In the second species we find the spectrum of hydrogen added.

Another early species would contain those bodies in which the nebula spectrum gets almost masked by a continuous one, such as Comets 1866 and 1867, and the great nebula in Andromeda.

In the second division will be more condensed swarms still, in which, one by one, new lines are added to the spectra, and carbon makes its appearance; while probably the last species in this group would be bodies represented by γ Cassiopeiæ.

The great distinction between the first group and the second would be that evidences of absorption now become prominent, and side by side with the bright flutings of carbon and occasionally the lines of hydrogen we have well-developed fluting absorption.

The second group, therefore, is distinguished from the first by mixed flutings—that is to say, the presence of bright and dark flutings as well as lines in the spectrum.

I give a detailed examination into the species of this group in the next part of this memoir.

[1] "Nebula [No. 117, 51h. 32 M. R.A. 0h. 35m. 5·3s.; N.P.D. 49° 54′ 12″·7. Very, very bright; large, round; pretty suddenly much brighter in the middle].—This small but bright companion of the great nebula in Andromeda presents a spectrum exactly similar to that of 31 M [the great nebula in Andromeda]. The spectrum appears to end abruptly in the orange; and throughout its length is not uniform, but is evidently crossed either by lines of absorption or by bright lines" (Huggins, *Phil. Trans.* vol. cliv. p. 441).

[2] "A dark band was noticed at wave-length 567·9" (Copeland, Comet III., 1881, *Copernicus*, vol. ii. p. 226).

"May 20.—With none of these dispersions could any bright bands, properly so called, be distinguished; but two faint broad *dark* bands, or what gave that impression, crossed the spectrum. . . . A third dark band was suspected near D on the blue side of that line" (Maunder, Comet α 1882 (Wells), "Greenwich Spectroscopic Observations, 1882," p 34).

The dark bands were observed again, and their wave-lengths measured on May 31" (*ibid.* p. 35).

The passage from the second group to the third brings us to those bodies which are increasing their temperature, in which radiation and fluting absorption have given place to line absorption.

At present the observations already accumulated have not been discussed in such a way as to enable us to state very definitely the exact retreat of the absorption, by which I mean the exact order in which the absorption lines fade out from the first members to the last in the group. We know generally that the earlier species will contain the line absorption of those substances of which we get a paramount fluting absorption in the prior group. We also know generally that the absorption of hydrogen will increase while the other diminishes.

The next group, the fourth, brings us to the stage of highest temperature, to stars like α Lyræ; and the division between this group and the prior one must be more or less arbitrary, and cannot at present be defined. One thing, however, is quite clear, that no celestial body without all the ultra-violet lines of hydrogen discovered by Dr. Huggins can claim to belong to it.

We have now arrived at the culminating point of temperature, and now pass to the descending arm of the curve of temperature. The fifth group, therefore, will contain those bodies in which the hydrogen lines begin to decrease in intensity, and other absorptions to take place in consequence of reduction of temperature.

One of the most interesting problems of the future will be to watch what happens in bodies along the descending scale, as compared with what happens to the bodies in Group III. on the ascending one. But it seems fair to assume that physical and chemical combinations will now have an opportunity of taking place, thereby changing the constituents of the atmosphere; that with every decrease of temperature an increase in the absorption lines may be expected, but it will be unlikely that the last species in this group will resemble the first one in Group III.

The next group, the sixth, is Secchi's type IV. and Vogel's Class III.*b*, its distinct characteristics being the absorption flutings of carbon. The species of which it will ultimately be composed are already apparently shadowed forth in the map which accompanies Dunér's volume, and they will evidently be subsequently differentiated by the gradual addition of other absorptions to that of carbon, while at the same time the absorption of carbon gets less and less distinct.

To sum up, then, the classification I propose consists of the following groups:—

GROUP I.—Radiation lines and flutings predominant. Absorption beginning in the last species.
GROUP II.—Mixed radiation and absorption predominant.
GROUP III.—Line absorption predominant, with *increasing* temperature. The various species will be marked by increasing simplicity of spectrum.
GROUP IV.—Simplest line absorption predominant.
GROUP V.—Line absorption predominant, with *decreasing* temperature. The various species will be marked by decreasing complexity of spectrum.
GROUP VI.—Carbon absorption predominant.
GROUP VII.—Extinction of luminosity.

It will be seen from the above grouping that there are several fundamental departures from previous classifications, especially that of Vogel.

The presence of the bright flutings of carbon associated with dark metallic flutings in the second group, and the presence of only absorbing carbon in the sixth, appears to me a matter of fundamental importance, and to entirely invalidate the view that both groups (the equivalents of III.*a* and III.*b* of Vogel) are produced from the same mass of matter on cooling.

This point has already been dwelt upon by Pechüle.

Another point of considerable variation is the separation of stars with small absorption into such widely different groups as the first and fourth, whereas Vogel classifies them together on the ground of the small absorption in the visible part of the spectrum. But that this classification is unsound is demonstrated by the fact that in these stars, such as γ Cassiopeiæ and β Lyræ, we have intense variability. We have bright hydrogen lines instead of inordinately thick dark ones; and on other grounds, which I shall take a subsequent opportunity of enlarging upon, it is clear that the physical conditions of these bodies must be as different as they pretty well can be.

It will be seen also that, with our present knowledge, it is very difficult to separate those stars the grouping of which is determined by line absorption into the Groups III. and V., for the reason that so far, seeing that only one line of temperature, and that a descending one, has been considered, no efforts have been made to establish the necessary criteria. I made this point in the paper to which I have already referred in connection with the provisional curve, and for purposes of completeness I introduce here the chief part of what I wrote on that occasion.

(*To be continued.*)

THE HITTITES, WITH SPECIAL REFERENCE TO VERY RECENT DISCOVERIES.[1]

V.

SOME months ago the Rev. Greville J. Chester brought to this country a quadrangular hæmatite seal found near Tarsus. Though this seal shows, in certain particulars, some analogy with the Yuzgât seal, yet it gives little or no additional aid in the decipherment of the inscriptions. It presents, nevertheless, features of very great interest. Prof. Sayce scarcely goes beyond the merits of the seal when he says that it possesses a "unique and splendid character; nothing like it has ever before been brought to the notice of European scholars."[2] The seal is engraved not only on the base (1), but also on the four sides, while opposite the base the stone was so cut as to serve the purpose of a handle. On four out of the five engraved faces are to be seen two figures—one seated and one standing. These may be supposed to represent men or deities, or possibly, in some cases, ideal personages. At first sight it may seem difficult to discern any general aim or connected purpose in the curious figures depicted. On more attentive examination, however, there is seen to be exhibited a pervading principle of *tri-unity*, especially as exemplified in the triangle and the trident. Moreover, while on three faces of the seal (1, 2, 5) there are figures with the "pig-tail" (an appendage which suggests a connection with the Hittites), it appears tolerably evident that the engraver of the seal intended to represent the personages with this appendage as destitute of the valuable knowledge and power connected with the mysterious three-in-oneness of the triangle and the trident. This is entirely in accordance with the position that the wearers of the pig-tail were still regarded as aliens and intruders when the seal was engraved.

On the base (1), a figure standing or advancing holds in the left hand a trident-like object, which is probably to be understood as a plant; though, like the curious symbols on the Boghaz-Keui bas-relief (*supra*, pp. 513, 514), it must be somewhat idealized. Certainly, it would seem

[1] Based on Lectures delivered by Mr. Thomas Tyler at the British Museum in January 1888. Continued from p. 593.

[2] *Archæological Journal*, December 1887. Prof. Sayce's paper is accompanied by an autotype representation of the seal. I may here mention, also, that impressions of this seal, as also of the Yuzgât seal and the seal of Tarkutimme, may be obtained at a small cost from Mr. A. Ready, of the British Museum.

difficult or impossible to identify it with any known vegetable production. And it would be equally difficult to determine what is the plant held in the hand of the sitting figure wearing the pig-tail,[1] though there seems to be a flower with a long depending and somewhat fibrous root. The two objects apparently are presented in comparison or competition, while that in the hand of the standing figure has the superiority.

On the second face a very curious scene is depicted. Above a kind of altar in the centre is a trident-like object, evidently identical with that already described. The trident-like object is between two symbols of remarkable form, capped with equilateral triangles. On these remarkable symbols, which probably represent life in general, or particularly human life, something more must be said directly. A figure, probably that of a deity, with the head of a hawk or eagle,[2] is pouring out a libation at the foot of the altar, and thus, we may presume, is confessing the superiority of the sacred objects above. On the other side of the altar is, it may be supposed, another deity, having above his head the winged solar disk, and in his left hand a double three-forked thunderbolt, introduced here, it may be supposed, as another emblem of tri-unity.

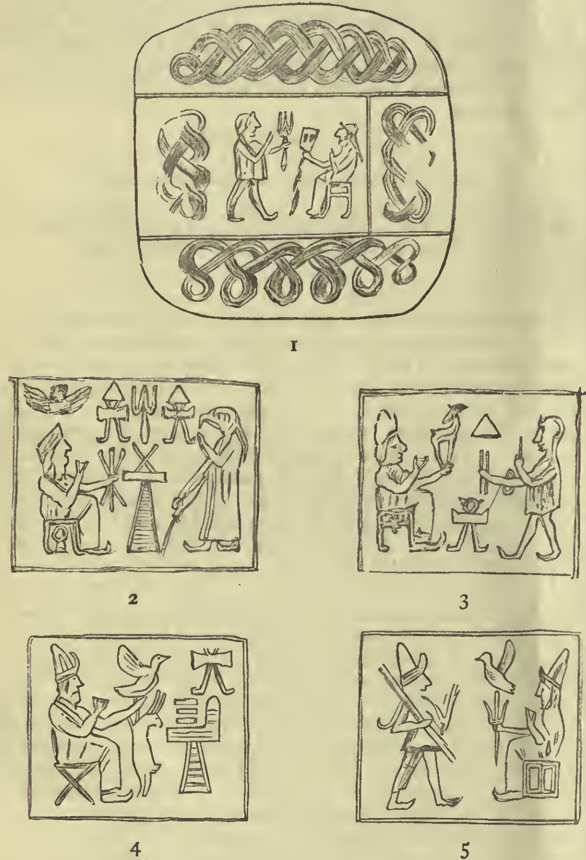


FIG. V.—The Tarsus seal (enlarged).

On the third face there is no personage wearing the pig-tail. Both the seated and the standing figures appear to be occupied with the mystery of the triangle. The engraver of the seal, moreover, as though determined that we should not mistake his meaning, actually represents the seated figure as forming a triangle with one hand. On face (4) the triangle formed by the hand is particularly clear in the impression of the seal; but there can scarcely be a doubt that the intention is the same also on this face (3) as well as on (2) and (5). How the triangle is supposed to be formed by the hand I am unable to say. By comparing the two hands of the seated figure in (3) it becomes evident that the goat standing on the left hand is here introduced as forming a triangle by his position. But still more remarkable and interesting is the personage standing, if considered together with the associated objects. This personage is supporting, apparently by a cord, a figure similar to those spoken of in connection with the second face as probably representing life in general, or more particularly the principle of human life. From the circular head of the figure are projecting what look like ears, but the triangular cap with which the head is covered on the second face is now seen above. The personage supporting the figure has in his left hand one rod held vertically, and in the right two vertical and parallel rods, thus suggesting the triangular number, three, a number regarded in antiquity as especially sacred.

The fourth face presents a single seated figure, making, as said just above, a triangle with the right hand. The left hand holds captive a hare as well as a bird with wings extended. The intention would seem to be to set forth the subjugation of the lower animals through the influence of such supposed occult and mysterious powers as those of the triangle. In front is an altar or table with objects upon it, which, it should be observed, are three in number. Above is a symbol generally identical with those spoken of in connection with faces (2) and (3), but here it is imperfect.

On the last face (5) we have apparently a competition between a pig-tailed figure standing or advancing and another figure seated. The pig-tailed figure holds two parallel rods or spears; and it would certainly appear that he is to be regarded as unequal in power to the seated figure, who is making a triangle with the left hand, while in the right hand is that symbol of tri-unity, the trident, now of more usual form, and differing considerably from the trident-like objects of (1) and (2). On the middle point of the trident is a bird with wings expanded. In this last respect the intention would seem to be somewhat similar to that expressed by the hare and bird held captive on face (4).

The Tarsus seal is probably less ancient than the Yuzgât seal; but there are nevertheless important points of resemblance, which may be reasonably taken as indicating a relationship more or less close. On both seals are to be seen the triangle and the trident, though on the Tarsus seal the form of the latter has become greatly changed. Both seals, also, have the winged solar disk. The wearing a horn in front of the head is another mark of resemblance; and the figures on both seals have the turned-up toes of the so-called "Hittite boots." The eagle-headed figure making the libation has a good deal of similarity to what, as depicted on the Yuzgât seal, I regard as a woman closely veiled, with some object, probably a baby, suspended from her arm (*supra*, p. 560). Notwithstanding any superficial resemblance, however, the objects delineated are certainly very different. The explanation probably is, that the respective engravers had in view a common typical form, which was in each case modified as the particular purpose required. A similar remark may be made with respect to the table or altar on (4), and the object before the king on the Yuzgât seal.

Whether the Tarsus seal will be found of importance with regard to the history of geometry, it would be difficult to say. This may to some extent depend on the date

[1] Prof. Sayce has rightly recognized the presence of the "pig-tail," but when he says that this appendage "characterizes Hittite female figures" (*op. cit.* p. 348), I fail to see any adequate grounds for the assertion. This, I should say, is not the mode of wearing the hair seen on the Boghaz-Keui bas-relief; and I am not acquainted with other evidence which would in any way justify the statement.

[2] The form of this deity suggests a possible relation with the eagle-headed deity on the Assyrian monuments, concerning which Assyriologists have been hitherto unable to give any adequate explanation. Perhaps some fresh light may be eventually derived from the "pig-tail" here appended.

to which the seal is to be referred. But, whatever may be the case with respect to the general history of geometry, certainly there are indications of something very like Pythagoreanism, such as we should by no means have expected to find on a Hittite or Asiatic monument. Still, however unexpected these indications may be, the scientific spirit requires that we should be loyal to facts. Among such indications may perhaps be placed the mysterious powers or properties apparently associated with vegetable forms on the Tarsus seal, as also on the Boghaz-Keui sculpture. But still greater importance and interest attach to the evidence of the seal as to the attribution of occult significance to number and to geometrical form. By the vertical rods of face (3) we are reminded of the Pythagorean doctrine concerning duality and unity, the even and the odd. (Plutarch, *De Is.* 48.) Moreover, we can scarcely mistake the sinister character of duality when we observe that the two parallel rods are carried on face (5) by the pig-tailed figure. Very probably the indications on the seal point to one of the sources whence were derived the doctrines attributed to Pythagoras. And such a view accords very well with the ancient tradition concerning the travels of Pythagoras, and the composite nature of Pythagoreanism.

But some additional consideration requires to be given to the figure on the Tarsus seal (faces 2, 3, 4), which I have spoken of as a symbol of life. In investigating the significance of this figure the most convenient method may be to compare it with the symbol most nearly resembling it which can be found elsewhere. This is to be seen on the coinage of Cyprus (Fig. W, 2). Here we have

1 2 3 4

FIG. W.—1, Symbol on Tarsus seal; 2, symbol on Cypriote coinage 3, *crux ansata*; 4, symbol on Indo-Scythian coin.

the rounded head (though without the projections on the symbol of the Tarsus seal), the horizontal stroke or body, and the divergent legs. True, on the seal there are slight projections at the ends of the horizontal piece, and at the ends of the divergent legs there are the "Hittite boots"; but in this last particular the symbol on the seal resembles the mandrake at Boghaz-Keui (*supra*, p. 514), the ends of the root being similarly turned up and metamorphosed.[1] As to the meaning of the symbol on the Cypriote coinage, we can make a reasonable inference from the fact that it seems to be introduced as an alternative symbol in place of the *crux ansata*, or symbol of life, which, indeed, is quite common on Cypriote coins.[2] The *crux ansata* was possibly derived from Egypt, but still it may very well be regarded as giving an indication of the meaning of the other symbol. If, however, the divergent legs of (2) are supposed to collapse, we have at once a *crux ansata* (3). The evidence so far would go towards the conclusion that the symbol on the Tarsus seal is a symbol of life. But by tracing the Cypriote symbol to its probable origin the evidence may be greatly strengthened.

The coins on which the Cypriote symbol just alluded to occurs are Phœnician. Now there occurs on Phœnician, and especially on Carthaginian, monuments a symbol by which scholars have been much puzzled. It consists of a triangle, normally, as it would seem, equilateral, though varying at times a good deal from this form. At the

[1] The triangular cap of the symbol on face (2) shows a connection between the symbol and the equilateral triangle. But what may be exactly the difference in the significance of the symbol when capped with the triangle and when destitute of this covering it is scarcely possible to say, unless the added triangle is supposed to give power and vitality.

[2] See De Vogüé, "Mélanges d'Archéologie Orientale," plate xi., Figs. 13, 16, 17, 18.

vertex of the triangle is a horizontal stroke or bar, with projections at the ends, which may be taken for arms, or hands held up; and these also are found to present variations. Above is a head of circular form. This frequently occurring symbol,[1] which may be seen to the reader's left in Fig. X, some scholars have taken for a representation of a man or woman praying and holding up his or her hands. But M. Renan justly observes that the position in which the symbol is found on the monuments is not compatible with such an opinion; and in the figure it evidently appears as

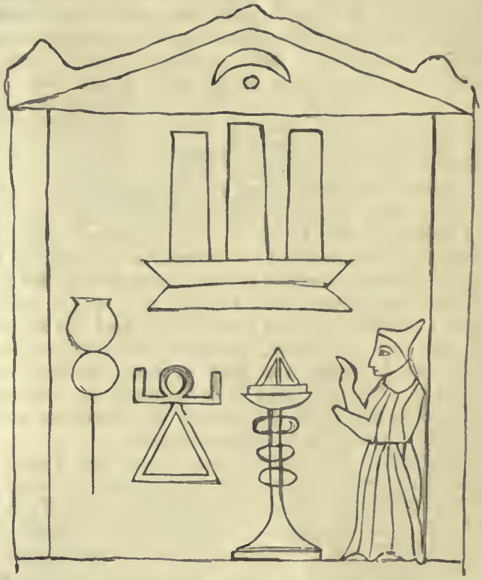


FIG. X.—Upper portion of stele of Lilybæum.

an object of worship. Regarded as denoting life, or as a sort of generalization of deity as the giver of life, its position on the stele becomes intelligible. It corresponds in form with the Cypriote symbol, except that the latter has lost the base of the triangle and the projections at the end of the horizontal piece, but indications of these being retained are clearly to be seen on the Tarsus symbol. M. Renan could make nothing of the triple object above the altar in Fig. X.[2] But when we look at the trident-like object of worship above the altar on face (2) of the Tarsus seal, the problem receives a good deal of light; and we recognize in the mysterious tripartite object of the stele a modification of the trident, expressing, like the triangle, the idea of tri-unity.

1 2

FIG. Y.—1, Portion of broken tablet in the British Museum; 2, archaic cuneiform character, *din*, "life."

With respect to the loss of the base of the triangle in the Cypriote symbol, and the import of this and other symbols before mentioned, we have evidence on a unique tablet in the British Museum. Though it is unfortunately broken, its testimony is still very important. The tablet

[1] M. Renan observes of this symbol: "Præcipua inter figuras religiosas est imago quâ nihil apud Phœnicas et Pœnos frequentius" ("Corp. inscr. sém.," vol. i. p. 281).

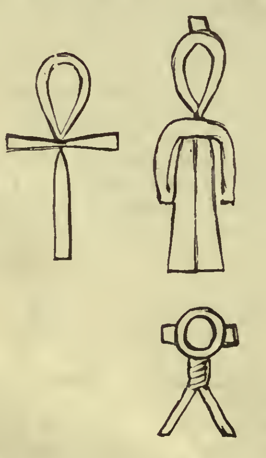
[2] He observes: "Supra figura cernitur tripartita, tribus cippis imparibus constans, cum basi duplici, quæ quid sibi velit non apparet" (*op. cit.* vol. i. p. 179). Fig. X. is a little reduced from the figure in the "Corpus."

gave the ancient hieroglyphic or hieratic forms of some cuneiform characters, with their values. Among these is a triangle the sides of which are represented by doubled lines carefully finished off, while the base, which is to a certain extent dissevered, is represented only by a single line or wedge. We thus see a tendency already to that dropping of the base exemplified in the Cypriote symbol. But is the meaning the same? The cuneiform character giving the value is unfortunately gone, except a small portion of a single wedge, which alone would yield but slender grounds for determining the import of the triangle. We are able, however, to take, together with the small portion of the wedge, the archaic cuneiform character denoting "life," and still retaining the semblance of an equilateral triangle. The value of "life" was that assigned to the triangle as it appears on the broken tablet, by M. Menant; and Mr. Pinches, of the British Museum, is also of opinion that the fragment of the wedge remaining is entirely in accordance with this view.

Whence the idea originated that the primordial source of life was of triangular form, it is of course impossible to say. This was, however, an idea which prevailed very widely indeed in the East. A distinguished scholar and archæologist has directed my attention to symbols found on Indo-Scythian coins as being analogous to the Hittite and Cypriote symbols (see Fig. V, 4). It is not difficult to discern the two sides of the triangle and the horizontal stroke or bar. Instead, however, of the circular or rounded head, there are four vertical strokes, which there can be little difficulty in recognizing as a symbol of fire, a symbol which, in the case of Zoroastrians and fire-worshippers, would be entirely suitable.

In connecting the Hittite symbol of the Tarsus seal with the Babylonian hieroglyphic triangle, we have gone back to an antiquity very remote indeed. But if we are to regard the symbols already discussed as connected also with the well-known Egyptian symbol of life, the *ankh*, and with other Egyptian symbolic forms, our demand on time must probably be much greater. That the Egyptian talismans (Fig. Z, 2, 3, 4) might have been evolved from

1 2 3



4

Fig. Z.—1, *Ankh*, Egyptian symbol of life, from coffin of Men-ka-ra, in the British Museum; 2, 3, 4, Egyptian talismans in the British Museum.

a form identical with, or resembling, the headed triangle of the Phœnician monuments, it requires no great stretch of imaginative power to discern.[1] But with regard to the *ankh* (1), so often seen in the hands of deities, though the points of resemblance are tolerably obvious,[1] yet it may seem difficult to understand how the triangle could have assumed the form of the vertical bar. True, the bar is pointed at the apex; and elsewhere on the Egyptian monuments an acute-angled triangle in the corresponding position is sufficiently common. But it is remarkable that this latter form is not seen on a monument so very ancient as the coffin of the king Men-ka-ra. Still, on the whole, it can scarcely be regarded as other than probable that the *ankh*, like the other Egyptian forms depicted, must be referred ultimately to the headed triangle. But, if this view is just, and the triangle had collapsed, as shown in the figure, when the coffin of Men-ka-ra was constructed, the period of man's existence on the earth in a condition of somewhat advanced civilization must be of exceedingly protracted duration.

As to the age of the greater Hittite monuments, it is impossible to speak. To argue that the Hittite hieroglyphs could not have remained long in use by the side of either the cuneiform syllabary or the Phœnician alphabet would be somewhat perilous. A better argument for their great antiquity is furnished by the total absence, so far as can be seen, of any indication of horses or chariots. Yet, in the wars with the Egyptians some fifteen or sixteen centuries before Christ, the Hittites appear well equipped with this kind of forces, in a state of organization from which lengthened usage may be reasonably inferred.

What has been said may suffice to show the extremely great interest of the questions suggested by the Hittite monuments. Unfortunately the material for investigation is at present but scanty, though there are probably hundreds, perhaps thousands, of monuments awaiting the spade of the excavator. The very important results obtained by the British Museum from the excavations at Jerablûs have not prevented these excavations from being for a considerable time wholly suspended. That this should be the case is certainly matter for regret; for I hope that I have at least succeeded in showing that the idea that the solution of the Hittite problem is hopeless is one which cannot be reasonably entertained.

CLASS EXPERIMENTS.

THE following is a brief account of some experiments shown to the students of the Natural Philosophy Class in the University of Glasgow during the present Session. It is communicated to NATURE with the permission of Sir W. Thomson.

I. (1) Suspend a heavy ball by a long wire, as shown in Fig. 1. To the middle of the ball attach a worsted thread, A D. Pull the thread in the direction of the arrow-head, with a pull that will not break it, and let the pull be finished before the ball is sensibly displaced. Observe the greatest subsequent displacement of the ball.

(2) Bring the ball to rest. Pull it now with a pull sufficient to break the thread. Note that the displacement is smaller than in case (1).

(3) Bring the ball to rest once more. Give a very sudden pull to the thread: it breaks, and the displacement of the ball is hardly perceptible.

In each of the three cases the momentum is equal to $\int F dt$ for the whole duration of the pull. The pull in case (1) is smaller than the pull in cases (2) and (3), but the duration of the pull is greater in a greater ratio; hence the momentum communicated, being the time integral of the pull, is the greatest for case (1). Although the pull in case (2) is equal to the pull in case (3), still its

[1] The distinguished Egyptologist, Mr. Le Page Renouf, now Keeper of Oriental Antiquities at the British Museum, tells me that, while (2) and (3) may not be earlier than the eighteenth dynasty, (4) is of very great antiquity, occurring in the name of Hor-em-sa-f, one of the Pyramid kings.

[1] The connection with, or analogy between, the Tarsus and Cypriote symbols and the *ankh* was suggested by Mr. Pinches, and subsequently, with respect to the Tarsus symbol, on different grounds, by Prof. Sayce.

duration is so exceedingly small in case (3) that the momentum communicated is very small.

II. Support a cylinder with a fly-wheel, as shown in Fig. 2. E E are two pieces of wood, both screwed at the

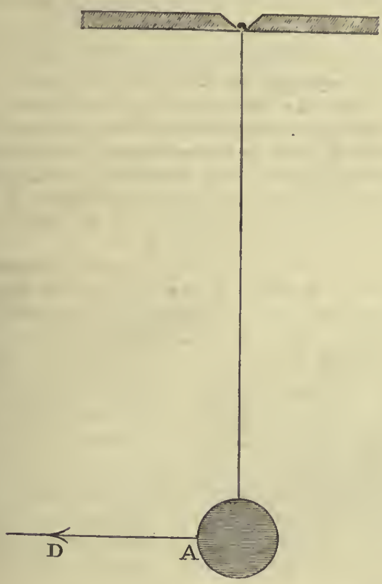


FIG. 1.

top to another piece of wood, L, of convenient thickness. Each has a slot cut along its centre, in which fits a ball, F, to which is attached a stiff wire, a string, and a weight, as shown in the figure. H is an india-rubber band, which presses E E together with a pressure at least sufficient to

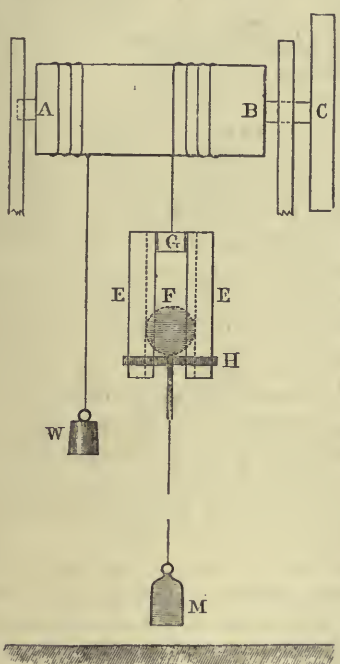


FIG. 2.

cause the ball F not to slip when the weight M is hung on to its string. Another string is wound round the end A of the cylinder, and a weight attached to it so as to balance the weight of the two pieces of wood, E E. The fly-wheel has a friction-brake upon it, and if the retarding force of the brake be constant, the angular displacement of the fly-wheel is proportional to the square of the momentum communicated.

(1) Lift the weight M a distance of about half an inch, and let it fall. The cylinder goes round through a certain angle, and the ball F is not pulled out of its slot.

(2) Lift the weight M through 2 or 3 inches, and let it fall. The ball F is pulled out of its slot; the cylinder goes round, but through a smaller angle than in case (1).

(3) Let the weight M fall through a height of 4 or 5 feet. The ball F is pulled out of its slot, and the angular displacement of the cylinder is barely perceptible.

The same explanations are applicable to the results of II. as were made concerning the results of I., provided couple be substituted for force, and moment of inertia for mass.

III. The following, though somewhat inconvenient as a class experiment, illustrates the same subject. Fix up

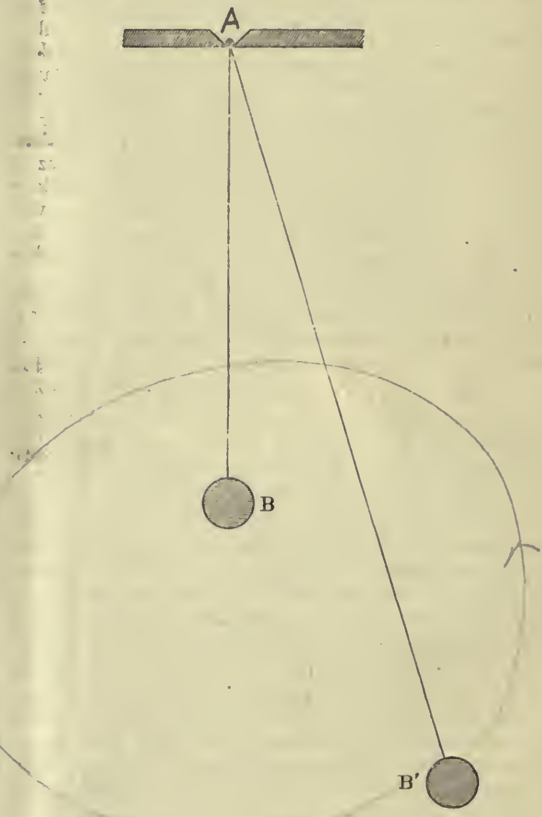


FIG 3.

a plain deal or other board in the manner of Robins' ballistic pendulum. From a rifle with a small charge of powder, fire a bullet into the board, at right angles to its plane, and as near as possible to its centre of inertia. The bullet lodges in the board, which is deflected through a large angle. Increase the charge of powder, so that the bullet pierces the board. The deflection of the board is now smaller. Put the maximum charge of powder in the rifle, and the deflection of the board on firing the bullet into it is exceedingly small.

IV. Suspend a light ivory or other ball by a long india-rubber thread several feet long, as shown in Fig. 3. Pull the ball into the position A B', and let it go. Looking at it as seen in the figure, it first begins to describe a curve against the hands of a watch. After two or three periods it begins to go round in a direction with the hands of a watch.

Bring the ball to the position B′ again, and project it at right angles, or at any angle, to the plane B A B The ball now illustrates three-dimensional motion. The period is slow, and the experiments are very interesting and instructive. MAGNUS MACLEAN.

NOTES.

THE bi-centenary of the publication of Newton's "Principia" was celebrated on Thursday last at Trinity College, Cambridge. A long and admirable address was read by Dr. Glaisher to a distinguished audience which had been invited to Cambridge by the Master and Fellows of the College. At a numerously attended dinner in Hall in the evening, speeches were made by the Master, the President of the Royal Society, the Astronomers-Royal for England and Ireland, and other distinguished guests.

AMONGST the missions just approved by the Special Commission of the French Ministry of Public Instruction are the following: M. Nicklès, mining engineer, to carry out in the provinces of Valencia and Alicante, in Spain, geological investigations; Dr. Morisse, to undertake various medical and natural history studies in the basins of the Upper Orinoco and Amazon; the Abbé Hyvernat, to proceed to Armenia to copy the cuneiform inscriptions on the shores of Lake Van, to investigate the art of Assyria, and to study on the spot the Neo-Syriac dialects spoken in the basin of Lake Urumiyah; M. Gay, to undertake a mission to Nicaragua, Columbia, and Venezuela, to study the natural history, and make collections for the State Museums; M. Thoulet, Professor of Mineralogy in the Faculty of Sciences at Nancy, to study the organization of the Observatory of Christiania, and of the Scottish Marine Biological Station at Edinburgh.

THE Bill to provide for technical education in England and Wales, prepared and brought in by Sir H. Roscoe, Sir U. Kay-Shuttleworth, Sir B. Samuelson, Mr. G. Dixon, and Mr. A. Acland, has been printed. It provides (1) that any School Board may make provision for giving technical education in any school under their management, and either by day or evening classes, or both, as may seem fit, having regard to the daily occupations of the persons to be benefited thereby; (2) that if no such provision is made, or if it is insufficient, and if the local authority by special resolution determine that provision or further provision ought to be made, they may themselves make such provision. The Bill also provides for the rendering of aid by School Boards or local authorities to voluntary schools in which technical instruction may be given; and two sections define the conditions under which Parliamentary grants shall be made for the encouragement of such instruction both in voluntary schools and in Board schools. It is proposed that any School Board or local authority, should they think fit, may institute an entrance examination in reading, writing, and arithmetic, for persons desirous of attending technical schools or classes under their management, or to which they contribute.

THE *Colonies and India*, commenting on the movement in favour of technical education in the colony of Victoria, says it will not be the fault of the Victorian Government if technical education is neglected, as there is a feeling in the Cabinet that if the country is to progress the rising generation should have the advantage of technical teaching. The Minister of Public Instruction has issued a minute on the policy of founding a Victorian Technical University, which is a digest of some of the evidence given before our own Royal Commission on Technical Instruction. Mr. Pearson estimates the initial expenditure involved in the foundation of a separate technical University at from £500,000 to a million, besides a yearly endowment of at least £30,000. The latter sum appears out of proportion to the average endowments of such institutions in Europe and America. It is not doubted that the money required will be freely voted.

THE following resolution was passed at a meeting of the American Philosophical Society on January 6, and has just been received by some of the scientific Societies of Great Britain in a circular dated March 12:—"*Resolved*, That the President of the American Philosophical Society be requested to address a letter to all learned bodies with which this Society is in official relations, and to such other Societies and individuals as he may deem proper, asking their co-operation in perfecting a language for learned and commercial purposes, based on the Aryan vocabulary and grammar in their simplest forms; and to that end proposing an International Congress, the first meeting of which shall be held in London or in Paris."

THE general meeting of the Institution of Mechanical Engineers will be held on Thursday evening, May 3, and Friday afternoon, May 4. The chair will be taken by the President, Mr. Carbutt, at 7.30 p.m. on Thursday evening, and at 2.30 p.m. on Friday afternoon. The following papers will be read and discussed as far as time permits:—Third Report of the Research Committee on Friction: experiments on the friction of a collar bearing; description of the emery testing machine, by Mr. Henry R. Towne, of Stamford, Connecticut; and supplementary paper on the use of petroleum refuse as fuel in locomotive engines, by Mr. Thomas Urquhart, Locomotive Superintendent, Grazi and Tsaritsin Railway, South-east Russia.

SURGEON-MAJOR F. S. B. FRANÇOIS DE CHAUMONT, F.R.S., Professor of Military Hygiene at the Army Medical School, Netley, died at his residence at Woolston, near Southampton, on the 18th inst. He was fifty-five years of age.

AT the meeting of the Society of Arts on the 18th inst., Sir Howard Grubb read a paper on telescopes for stellar photography. His object was to discuss and describe a few of the more important mechanical details of the instruments which are to be used for the international photographic survey of the heavens. The paper is printed in the current number of the Journal of the Society of Arts.

ON March 31, about 10 p.m., a splendid meteor was seen at Asker, in Nerice, in Sweden. It appeared in the southern sky, increasing in brilliancy in its descent. Finally it seemed to burst into three parts, each of which left a trail in the sky observable a few seconds. The colour was intense bluish-white.

SEVERE shocks of earthquake were felt at Oldenburg on April 12. Several houses fell in at Eisenstadt. Shocks were also noticed at Pottendorf, in Lower Austria.

A HYDROGRAPHICAL BUREAU has been opened in Würtemberg, under the direction of Herr von Marten.

WE are glad to hear that a regular meteorological organization is to be established in Spain. The Director, appointed by "competitive examination," is Señor Augusto Arcimis, formerly of the Institution Libre de Ensenanza, Madrid. M. Arcimis has long been known as a meteorologist.

MM. MOHN AND HILDEBRANDSSON have published an important discussion on the "Thunderstorms of Scandinavia" (Upsal, 1888, 55 pp. and 12 plates). The first network of thunderstorm stations was established in France by Leverrier in 1865, and his plan has been adopted in most other countries, almost without change. Norway followed next, in 1867, and Sweden in 1871. The storms are divided into two classes: (1) heat thunderstorms, which occur generally in summer, and mostly originate in the central and eastern parts of the Scandinavian peninsula 2) cyclonic thunderstorms, which generally

occur in winter, on the western coasts, and accompany a barometric depression coming from the Atlantic. An attempt is made at fixing the heights of thunderstorm clouds, but these vary very much with different times and localities; it seems proved, however, that the movements of the cirrus clouds are in no way affected by the storms. The summer storms occur most frequently in the afternoon, and most rarely between 2 and 4 a.m. But on the coast of Norway the maximum frequency occurs about 8 p.m., and the winter storms occur more frequently in the night. These facts have also been pointed out by Dr. Buchan with regard to the storms of the north-west of Scotland. In the annual period the storms occur most frequently in July and August, but there is also a secondary maximum in January. The work contains much that would repay careful study.

THE monthly meteorological notes and rainfall statistics for South Australia, published by Mr. C. Todd, the Government Astronomer, contain very useful climatological data and notices of miscellaneous phenomena. Mr. Todd has taken advantage of his position as Postmaster-General to establish meteorological or rainfall stations at a great number of telegraph offices; the number of reports published for February 1887 is 298, together with the means for all stations having at least seven years' record. The observations in their present form seem to date from 1883, when 235 records were published, but the work commenced as as far back as 1857, since which time it has been steadily pursued. For Adelaide itself, the records of Sir G. S. Kingston extend back as far as 1839, and these observations have been used by Mr. Todd in his excellent article on the climate of the colony in the "Hand-book of South Australia." It is stated in this work that local features are apparently insufficient to explain the large differences in the yearly averages of the rainfall; Mr. Todd's continued exertions must tend to elucidate this subject.

A NEW series of isomorphous double chlorides of the metals of the iron and alkali groups have been prepared by Dr. Neumann (*Liebig's Annalen*). The general formula of the system is $4RCl . M_2Cl_6 + 2H_2O$, where R may represent any member of the group of alkali metals, and M either iron, chromium, or aluminium. Magnesium and beryllium are also included in the series, $2MgCl_2$ or $2BeCl_2$ replacing $4RCl$. They all crystallize in forms belonging most probably to the regular system, generally in octahedrons or rhombic dodecahedrons. The iron salts especially are remarkably beautiful, $4KCl . Fe_2Cl_6 + 2H_2O$ forming octahedrons and dodecahedrons of reddish-brown tint, while crystals of the corresponding ammonium compound possess a magnificent garnet-red colour; the rubidium and magnesium salts are yellow, and the chloride of beryllium and iron separates in fine orange crystals. These iron salts, the first two of which have been known some time, are prepared comparatively readily by dissolving ferric chloride in concentrated hydrochloric acid, adding the necessary quantity of the alkaline chloride, and crystallizing. But Dr. Neumann, in attempting to complete the series, found considerable difficulty in preparing the corresponding chromium and aluminium salts. He eventually succeeded completely, in the case of chromium, by dissolving the chromium chloride in warm 96 per cent. alcohol, adding a proportionately small quantity of the other chloride and passing a rapid stream of hydrochloric acid gas, the whole being gently boiled for some time, using a reflux condenser. It was found that the 4 per cent. of water, together with that liberated during the formation of ethyl chloride, was just sufficient to supply the water of crystallization, hence on cooling the double salt crystallized out in microscopic crystals resembling in shape those of their ferric brethren. These chromium salts are of various shades of violet, are deliquescent like all other members of the series, and are likewise decomposed by water. The only aluminic member of the series yet prepared by Dr. Neumann is the potassium compound $4KCl . Al_2Cl_6 + 2H_2O$, which, however, is one of the finest of the whole class; it crystallizes in splendid octahedrons, resembling large diamonds and refracting light with similar brilliancy. Crystals will ever remain among the choicest fruits of the chemist's labour, and form an inexhaustible source of pleasure to lovers of the beautiful. The new isomorphous group is of great theoretical interest, and will take its rank with the well-known alums and the double sulphates of the ferrous-ammonium type.

A MOST interesting account of the work in mound-exploration carried on by the United States Bureau of Ethnology, has been issued by the Smithsonian Institution. The writer is Mr. Cyrus Thomas. It seems that over two thousand mounds have been explored, including almost every known type as to form, from the low, diminutive, circular burial tumulus of the north, to the huge, truncated, earthen pyramid of the south, the embankment, the effigy, the stone cairn, house site, &c. Every hitherto known variety as to construction, as well as a number decidedly different in details, has been examined. Some of the latter are very interesting and furnish important data. Particular attention has been paid to this branch of the work, because the mode of construction and the methods of burial in the ordinary conical tumuli furnish valuable data in regard to the customs of the builders, and aid in determining the archæological districts. Many ancient graves and cemeteries and several *caches* and cave deposits have also been explored. The number of specimens obtained by the division since its organization is not less than thirty-eight thousand. The specimens procured by the field assistants in person constitute by far the most valuable portion of the collection, since the particulars regarding their discovery and surroundings are known. Not a single stone or tablet with anything like letters or hieroglyphics inscribed on it, by which linguists might be able to judge of the language of the mound-builders, has been discovered.

A SECOND Laura Bridgman is at present attracting the attention of American psychologists. Her name is Helen Keller. Although blind and deaf, she makes rapid progress in her studies. *Science* (April 6) gives her portrait and that of her teacher, Miss Annie Sullivan, a graduate of the Perkins Institute at Boston, and also reproduces in *facsimile* a letter written by Helen Keller to A. Graham Bell, of Washington. It was only in March 1887 that Miss Sullivan was engaged to give the first instruction to her pupil, who was then six years old. In a month the little girl learned to spell about 400 words, and in less than three months could write a letter, unaided by anyone. In six weeks she mastered the Braillé (French) system, which is a cipher for the blind, enabling them to read what they have written. She has also mastered addition, multiplication, and subtraction, and received lessons in geography. She is trained solely through the sense of touch.

THE new number of the "Year-Book of the Scientific and Learned Societies of Great Britain and Ireland" (Griffin and Co.) has just been published. This is the fifth annual issue. It comprises lists of papers read during 1887 before Societies engaged in all departments of research, with the names of their authors. There is also an appendix, presenting a list of the leading scientific Societies throughout the world. The work is a useful one, but it ought to have been more thoroughly revised. On the very first page, in the list of the members of the Council of the Royal Society, two names are wrongly given: Sir A. (instead of W.) Bowman, and R. (instead of W.) T. Thiselton Dyer.

A PAPER entitled "Additional Records of Scottish Plants for the Year 1887," by Mr. Arthur Bennett, has been sent to us. It consists of a list of the new county botanical records which came under Mr. Bennett's notice during 1887, and forms a con-

tinuation of the lists which appeared in the *Scottish Naturalist* for 1886 and 1887. Mr. Bennett says that the results during the year 1887 were probably richer than in any former year, not only in the large number of comital records, but in the new species added to the Scottish flora.

We have received several numbers of the *Annales de la Faculté des Sciences de Toulouse* (Paris: Gauthiers-Villars). This new publication (which is well printed on good paper, with wide margins) consists chiefly of memoirs relating to physics, chemistry, and mathematics pure and applied. It contains also articles on questions of general scientific interest. To papers of the latter class the authors append lists of books on the questions discussed.

According to a paper in the *Board of Trade Journal* for April, the production of attar of roses constitutes one of the most important branches of native industry in Bulgaria. The valley of Kezanlyk, known as the Vale of Roses, is the centre of this production, which extends as far as Carlovo, and the villages which lie sheltered from the north wind by the vast chain of the Great Balkans. In 1885, and no later statistics have been published, the manufacture of attar of roses in the district indicated amounted to a value of 1,100,000 francs. The prosperous condition of the valley of Kezanlyk has led other districts of Bulgaria to develop the same industry, and particularly the inhabitants of Strema, and of Toundja, at the foot of Mount Rhodope. It is not yet certain that the attar from these new countries will equal in quality the famous product of Kezanlyk. The Government, however, is anxious to encourage this movement, and the Department of the Interior has lately authorized the purchase of a certain quantity of attar prepared at Strema and at Toundja. Specimens of each are to be sent for examination at the laboratory of the University of Moscow, and the result is to be published.

From an official report just published it appears that in 1886 there were killed in Norway 114 bears, 37 wolves, 5618 foxes, 950 eagles, 5100 hawks, and 108 other animals of prey. The number of bears was slightly below that of 1885, but above the numbers of previous years, whilst the number of wolves was twice that of 1885. The number of foxes, on the other hand, was only half that of the previous year, whilst those of eagles and hawks were about the same.

In last week's Nature (p. 581), near the middle of the second column, for "Ekholm *of* Hagström," read "Ekholm *and* Hagström."

The additions to the Zoological Society's Gardens during the past week include a Common Marmoset (*Hapale jacchus*) from South-East Brazil, presented by Mrs. Leighton; a Striped Hyæna (*Hyæna striata*) from Morocco, presented by Mr. Herbert E. White; an Indian Wolf (*Canis pallipes* ♂), two —— Foxes (*Canis* ——), a —— Hawk Eagle (*Spizaetus* ——) from India, presented by Colonel Alex. A. A. Kinloch, C.M.Z.S.; two Rock-hopper Penguins (*Eudyptes chrysocome*) from Auckland, New Zealand, presented by Captain Sutcliff, R.M.S.S. *Aorangi*; a Gannet (*Sula bassana*), British, presented by Miss Serrell; three Common Swans (*Cygnus olor*), British, purchased; a Chinchilla (*Chinchilla lanigera*), a Barbary Wild Sheep (*Ovis tragelaphus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Photography in the Determination of the Motions of Stars in the Line of Sight.—Of the many developments of spectroscopy, one of the most interesting is that first made a practical branch of observation by the skill and patience of Dr. Huggins, viz. the determination of the motions of stars in the direction of the visual ray by measures of the displacement of the more prominent lines in their spectra. The research has, however, always been beset with many practical difficulties, one of the most serious being the manner in which the stellar lines seem to elude the sight when the air is disturbed. This hindrance has been especially felt at Greenwich, where this kind of work has been adopted as part of the ordinary routine, and where, in consequence, it has not been possible, as would be the case in a private observatory, to confine observation to nights of faultless definition. Many of the observations have, therefore, been exceedingly rough, or even discordant. Prof. H. C. Vogel, who had made some successful measures of the displacements of lines in three or four of our brightest stars soon after Huggins's first observations, has recently turned his attention to photography as a means of overcoming this difficulty, and his first results, given in a paper read before the Royal Prussian Academy on March 15, are very promising. Prof. Vogel finds that the atmospheric tremors, so wearisome to the eye, exercise no influence upon the photograph, which possesses the additional advantage of being free from all bias or predisposition. Dr. Scheiner, who has been carrying out these experiments, has examined seven spectra, viz. those of Sirius, Procyon, Castor, Arcturus, Aldebaran, Pollux, and Rigel. Of these, Sirius showed a slight displacement to the red, Procyon a decided displacement, and Rigel very large in the same direction, whilst Arcturus showed a considerable displacement towards the violet. The observations were made on the third line of hydrogen, Hγ, a train of two prisms of high dispersion being used.

The Total Lunar Eclipse of January 28.—Dr. E. Lindemann sends the following list of the number of occultations observed at different Observatories during this eclipse, in addition to the lists given already: Albany (U.S.), 7; Christiania, 28; Milan, 23; Bonn, 7; Durban (Natal), 17; Oxford (Radcliffe), 9; Bruxelles, 14; Liège, 5; Palermo, 8; Cape of Good Hope, 21; Madras, 10. The weather was cloudy at Warsaw.

New Minor Planets.—Herr Palisa discovered a new minor planet, No. 274, on April 3, and another, No. 275, on April 13. The latter is his sixty-third discovery. No. 269 has received the name of Justitia.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *APRIL* 29—*MAY* 5.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on April 29

Sun rises, 4h. 36m.; souths, 11h. 57m. 10·3s.; sets, 19h. 18m.: right asc. on meridian, 2h. 28·7m.; decl. 14° 41′ N. Sidereal Time at Sunset, 9h. 51m.

Moon (at Last Quarter May 3, 0h.) rises, 22h. 21m.*; souths, 2h. 46m.; sets, 7h. 6m.: right asc. on meridian, 17h. 15·7m.; decl. 19° 30′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	4 23 ...	11 11 ...	17 59 ...	1 42·0 ...	8 45 N.
Venus.....	4 8 ...	10 44 ...	17 20 ...	1 15·8 ...	6 22 N.
Mars	16 45 ...	22 25 ...	4 5* ...	12 58·3 ...	4 38 S.
Jupiter....	21 25* ...	1 40 ...	5 55 ...	16 9·9 ...	20 1 S.
Saturn....	9 40 ...	17 38 ...	1 36* ...	8 10·8 ...	20 40 N.
Uranus ...	16 42 ...	22 20 ...	3 58* ...	12 53·2 ...	4 58 S.
Neptune..	5 35 ...	13 17 ..	20 59 ...	3 48·6 ...	18 21 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

May.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
1 ...	50 Sagittarii ...	6 ...	3 19 ...	3 33 ...	359 339
3 ...	31 Capricorni ...	6½ ...	2 50 ...	3 43 ...	112 214

May.	h.	
5 ...	18 ...	Mars in conjunction with and 0° 35′ north of Uranus.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	May 2,	2 40	*m*
ζ Geminorum	6 57·5	20 44 N.	Apr. 29,	20 0	*m*
U Monocerotis	7 25·5	9 33 S.	May 1,		*M*
T Geminorum	7 42·6	24 1 N.	Apr. 29,		*M*
R Crateris	10 55·1	17 43 S.	,, 30,		*m*
T Ursæ Majoris	12 31·3	60 6 N.	May 2,		*M*
U Boötis	14 49·2	18 9 N.	Apr. 29,		*M*
δ Libræ	14 55·0	8 4 S.	May 1,	21 38	*m*
U Coronæ	15 13·6	32 3 N.	Apr. 30,	22 56	*m*
U Ophiuchi	17 10·9	1 20 N.	May 2,	2 10	*m*
W Sagittarii	17 57·9	29 35 S.	,, 5,	3 0	*M*
R Scuti	18 41·5	5 50 S.	Apr. 29,		*M*
β Lyræ	18 46·0	33 14 N.	May 5,	1 0	*M*
S Sagittæ	19 50·9	16 20 N.	Apr. 30,	0 0	*m*
			May 3,	0 0	*M*
T Delphini	20 40·2	16 0 N.	,, 1,		*M*
T Vulpeculæ	20 46·7	27 50 N.	Apr. 30,	22 0	*M*
			May 1,	23 0	*m*
δ Cephei	22 25·0	57 51 N.	Apr. 29,	21 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ζ Ursæ Majoris	206°	57° N.	Slow, bright.
,, β Libræ	228	5 S.	Rather slow.
,, δ Serpentis	233	10 N.	Swift.
,, υ Herculis	239	46 N.	Swift, faint.
,, ξ Ophiuchi	255	21 S.	Rather slow, long.
,, η Aquarii	337	2 S.	Swift, long, streaks.

GEOGRAPHICAL NOTES.

THE Founder's Medal of the Royal Geographical Society has been awarded to Mr. Clements R. Markham, C.B., F.R.S., on his retirement, after twenty-five years' service, from the Honorary Secretaryship of the Society, during which he has done so much for the promotion of geography. The announcement of Mr. Markham's retirement will be received with regret by all who know the value of the work he has done, both in connection with the Society and otherwise. But as he is still in his vigour we may look for many more years' good work from him. The Royal Medal has been awarded to Lieut. Wissmann, who has twice crossed Africa, and done a great amount of excellent exploring work in the region south of the Congo. The Murchison Grant has been awarded to Mr. James McCarthy, Superintendent of Surveys in Siam; the Gill Premium to Mr. Charles M. Doughty, for his explorations in Arabia; and the Cuthbert Peek Grant to Major Festing for his services as cartographer on the Gambia River. As honorary corresponding members, have been selected Dr. G. Radde, of Tiflis, Dr. H. Rink, of Copenhagen, and Dr. Rein, Professor of Geography at Bonn University.

Two papers were read at Monday's meeting of the Royal Geographical Society, one by the Rev. T. S. Lea, on the Island of Fernando Noronha, and the other by Colonel Sir Marshall Clarke, on Basuto Land. Mr. Lea accompanied Mr. H. N. Ridley on his mission to Fernando Noronha last year. The islands are 290 miles north-east of Pernambuco. The total length of the whole group from north-east to south-west is about 6¼ geographical miles, and the maximum width of Fernando itself 1½ mile. The north-east cape of that island is very rugged and precipitous, though of no great height. Boobie Island and Egg Island are also raised masses of reef rock, which again appears on the top of the basalt of Platform Island. Mount St. Michael is a phonolite peak on which the weed invasion has hardly found a footing, and the native plants still flourish. This phonolite is a gray, close-grained columnar rock, and it seems to be the key to the very interesting geology of the island. Platform Island and Egg Island have a connection at low water with the main island, a small mass of reef rock. Morro do Chapeo, or the Hat Rock, seems to represent the residue of a larger block. The north cape of the main island is stony, and there is no great wealth of vegetation, though even here many of the endemics may be found. There is a patch of blown sand at San Antonio over which the *Ipomœa pes-capræ* trails, and beyond that the ground rises towards the basaltic height on which the town is built. The basalt is more inclined to be nodular than columnar. Descending from the town hills, the peak stands out clear against the northern sky. It is a huge mass of columnar phonolite, with a talus of *débris* around it, in shape not unlike a church with a tower. About the centre of the plain rises a round mass of phonolite. On the south coast, like bastions, stand two other phonolite masses, with a ridge of basalt between them, steep on its seaward side, but sloping gradually landwards. The islands of the south coast, with the exception of the minute I. Jones, are also phonolite. Tobacco Point is basaltic, and Morro Branco, in Leâo Bay, altered phonolite. There are raised beaches of reef rock on Tobacco Point and to the east of Look-out Hill. Mr. Lea hazards the following observations with regard to the structure and possible history of the main island. Though undoubtedly volcanic in origin, the date at which it was in any way active must be exceedingly remote. No hot springs, or any trace of them, occur; no earthquakes or tidal waves are felt. No site of a crater can be pointed to with certainty, and indeed any attempt to reconstruct its pristine shape from the attenuated remains that are left us must be undertaken with extreme diffidence. As the island is surrounded by deep sea, and as nothing volcanic occurs, as far as he is aware, on the coast of Brazil in its neighbourhood, he is inclined to think that it marks the site of an isolated vent. The number of species of plants, &c., peculiar to the island seems also to point to this, or at any rate to the extreme remoteness of any connection with other land. But there is at least one thing which may throw some light on this matter. All round the island, though interrupted in places, especially on the northern coast, there is a sort of reef formation laid bare at low water, and closely resembling the Recife of Pernambuco. At certain points a very similar rock is found at considerable heights above the sea. On Rat Island this reef attains no great elevation. It rests upon a beach of rounded boulders near the landing, which may be seen underlying it. Boobie Island and Egg Island also have it, and there are traces of it at the summit of Platform Island. On basalt in Cotton-tree Bay, close by Look-out Hill, it occurs at a yet greater height, and again on Tobacco Point and I. Jones it also occurs above high-water mark. Raised beaches, therefore, seem only to exist on basalt, and in close connection with a phonolite peak. Mr. Lea suggests that the phonolite regions mark the sites of the ancient vents of the volcano, the phonolite itself being the plug which remained fixed during subsequent eruptions of lava. The scoria is all but gone, only remaining where the basalt covers it, but the harder phonolite still remains in its place, and the raised beaches show that beneath it lay the forces which manifested themselves in the last expiring efforts of the volcano. The flora and fauna of the group have already been very fully described by Mr. Ridley.

SIR MARSHALL CLARKE'S paper described an official tour he made in Basuto Land, last October, to visit the Baltokoa tribe settled among the mountains. He traversed 400 miles of country, a large proportion of which had never been visited by Europeans. The highest point attained was 10,750 feet; but from thence, both north and south, distant heights appeared at great elevations.

ANTAGONISM.[1]

SOME months ago, shortly after I had resigned my office of Judge of the High Court, I was expressing to a friend my fear of the effect of having no compulsory occupation, when he said, by way of consolation, "Never mind, 'for Satan finds some mischief still for idle hands to do.'" You may possibly in the course of this evening think he was right. I have chosen a title for my lecture which may not fully convey to your minds the scope of the views which I am going to submit to you. I propose to adduce some arguments to show that "antagonism," a word generally used to signify something disagreeable, pervades all things; that it is not the baneful thing which many consider it; that it produces at least quite as much good as evil; but that, whatever be its effect, my theory—call it, if you will, speculation—is that it is a necessity of existence, and of the organism of the universe so far as we understand it; that motion and life cannot go on without it; that it is not a mere casual adjunct of Nature, but that without it there would be no Nature, at all events as we conceive

[1] Lecture delivered at the Royal Institution, on April 20, by the Right Hon. Sir William R. Grove, F.R.S.

it; that it is inevitably associated with unorganized matter, with organized matter, and with sentient beings.

I am not aware that this view, in the breadth in which I suggest it, has been advanced before. Probably no idea is new in all respects in the present period of the world's history. It has been said by a desponding pessimist that "There is nothing new, and nothing true, and nothing signifies," but I do not entirely agree with him; I believe that in what I am about to submit there is something new and true in the point of view from which I regard the matter; whether it signifies or not is for you to judge.

The universality of antagonism has not received the attention it seems to me to deserve from the fact of the element of force, or rather of the conquering force, being mainly attended to, and too little note taken of the element of resistance unless the latter vanquishes the force, and then it becomes, popularly speaking, the force, and the former force the resistance.

There are propositions applying more or less to what I am going to say of some antiquity.

Heraclitus, quoted by Prof. Huxley, said: "War is the father and king of all things." Hobbes said war is the natural state of man, but his expressions have about them some little ambiguity. In Chapter I. of the "De Corpore Politico" he says "Irresistible might in a state of nature is right," and "The estate of man in this natural liberty is war." Subsequently he says: "A man gives up his natural right, for when divers men having right not only to all things else, but to one another's persons, if they use the same there arizeth thereby invasion on the one part and resistance on the other, which is war and therefore *contrary to the law of Nature, the sum whereof consisteth in making peace.*" I can only explain this apparent inconsistency by supposing he meant "law of Nature" to be something different from "the natural estate of man," and that the making peace was the first effort at contract, or the beginning of law; but then why call it the "*law of Nature*," where he says might is right? There is however some obscurity in the passage.

The Persian divinities, Ormuzd and Ahriman, were the supposed rulers or representatives of good and evil, always at war, and causing the continuous struggles between human beings animated respectively by these two principles. Undoubtedly good and evil are antagonistic, but antagonism, as I view it, is as necessary to good as to evil, as necessary to Ormuzd as to Ahriman. Zoroaster's religion of a Divine being, one and indivisible, but with two sides, is, to my mind, a more philosophical conception. The views of Lamarck on the modification of organic beings by effort, and the establishment of the doctrine of Darwin as to the effects produced by the struggle for existence and domination, come much nearer to my subject. Darwin has shown how these struggles have modified the forms and habits of organized beings, and tended to increased differentiation, and Prof. Huxley and Herbert Spencer have powerfully promoted and expanded these doctrines. To the latter we owe the happy phrase, "survival of the fittest," and Prof. Huxley has recently, in a paper in the *Nineteenth Century*, anticipated some points I should have adverted to as to the social struggles for existence. To be anticipated, and by a very short period, is always trying, but it is more trying when what you intended to say has been said by your predecessor in more terse and appropriate language than you have at your command.

I propose to deal with "antagonism" inductively, *i.e.* with facts derived from observation alone, and not to meddle with spiritual matters or with consequences.

Let us begin with what we know of the visible universe, viz. suns, planets, comets, meteorites, and their effects. These are all pulling at each other, and resisting that pull by the action of other forces.

Any change in this pulling force produces a change, or, as it is called, perturbation, in the motion of the body pulled. The planet Neptune, as you know, was discovered by the effect of its pulling force on another planet, the latter being deflected from its normal course. When this pulling force is not counterbalanced by other forces, or when the objects pulled have not sufficient resisting power, they fall into each other. Thus, this earth is daily causing a bombardment of itself by drawing smaller bodies—meteorites—to it; 20,000,000 of which, visible to the naked eye, fall on an average into our atmosphere in each twenty-four hours, and of those visible through the telescope, 400,000,000 are computed to fall within the same period. Mr. Lockyer has recently given reasons for supposing the luminosity of nebulæ, or of many of them, is due to collisions or friction among the meteorites which go to form them; but his paper on the subject is not yet published. You must get from Mr. Lockyer the details of his views. I hope he may, at one of these evening meetings, give you a *résumé* of them from the place I now occupy.

What is commonly called centrifugal force does not come from nothing; it depends upon the law that a body falling by the influence of attraction, not upon, but near to, the attracting body, whirls round the latter, describing one of the curves known as conic sections. Hence, a meteorite may become a planet or satellite (one was supposed to have become so to this earth, but I believe the observations have not been verified); or it may go off in a parabola as comets do; or, again, this centrifugal force may be generated by the gradual accretion of nebulous matter into solid masses falling near to, or being thrown off from, the central nucleus, the two forces, centrifugal and centripetal, being antagonistic to each other, and the relative movements being continuous, but probably not perpetual. Our solar system is also kept in its place by the antagonism of the surrounding bodies of the Kosmos pulling at us. Suppose half of the stars we see, *i.e.* all on one side of a meridian line, were removed, what would become of our solar system? It would drift away to the side where attraction still existed, and there would be a wreck of matter and a crash of worlds. It is very little known that Shakespeare was acquainted with this pulling force. He says, by the mouth of Cressida—

> "But the strong base and building of my love
> Is as the very centre of the earth
> Drawing all things to it"—

a very accurate description of the law of gravitation, so far as this earth is concerned, and written nearly a century before Newton's time.

But in all probability the collisions of meteorites with the earth and other suns and planets are not the only collisions in space. I know of no better theory to account for the phenomena of temporary stars, such as that which appeared in 1866, than that they result from the collision of non-luminous stars, or stars previously invisible to us. That star burst suddenly into light, and then the luminosity gradually faded, the star became more and more dim, and ultimately disappeared. The spectrum of it showed that the light was compound, and had probably emanated from two different sources. It was probably of a very high temperature. If this theory of temporary stars be admitted, we get a nebula of vapour or star dust again, and so may get fresh instances of the nebular hypothesis.

Let us now take the earth itself. It varies in temperature, and consequently the particles at or near its surface are in continuous movement, rubbing against each other, being oxidized or deoxidized, either immediately or through the medium of vegetation. This also is continuously tearing up its surface and changing its character. Evaporation and condensation, producing rain, hail, and storms, notably change it. Force and resistance are constantly at play. The sea erodes rocks and rubs them into sand. The sea quits them and leaves traces of its former presence by the fossil marine shells found now at high altitudes. Rocks crumble down and break other rocks or are broken by them; avalanches are not uncommon. The interior of the earth seems to be in a perpetual state of commotion, though only recurrent to our observation. Earthquakes in various places from time to time, and, doubtless, many beneath the sea of which we are not cognizant, nor of other gradual upheavals and depressions. Throughout it nothing that we know of is at rest, and nothing can move without changing the position of something else, and this is antagonism. Metals rust at its surface, and probably they or their oxides, chlorides, &c., are in a continuous state of change in the interior. Nothing that we know of is stationary. The earth as a whole seems so at first sight, but its surface is moving at the rate of some seventeen miles a minute at the equator; and standing at either of the Poles—an experiment which no one has yet had an opportunity of trying—a man would be turned round his own axis once in every twenty-four hours, while the earth's motion round the sun carries us through space more than a million and a half of miles a day.

The above changes produce motion in other things. The earth pulls the sun and planets, and in different degrees at different portions of its orbit.

Before I pass from inorganic to organized matter I had better deal with what may perhaps strike you as the most difficult part of my subject, viz. light. Where, you may say, is there antagonism

in the case of light? Light exercises its force upon such minute portions of matter that until the period of the discovery of photography its physical and chemical effects were almost unknown. Such effects as bleaching, uniting some gases, and affecting the colouring matter of vegetables, were partly known but little attended to; but photography created a new era: I shall advert to this presently. The theories of light, however, involved matter and motion. The corpuscular theory, as you well know, supposed that excessively small particles were emitted from luminous bodies, and travelled with enormous velocity. The undulatory theory, which supplanted it, supposed that luminous bodies caused undulations or vibrations in a highly tenuous matter called ether, which is supposed to exist throughout the interplanetary spaces and throughout the universe so far as we know it. Some suppose this ether to be of a specific character differing from that of ordinary gases, others that it is in the nature of a highly attenuated gas; but, whatever it be, it cannot be affected by undulations or vibrations without being moved, and when matter is moved by any force it must offer resistance to that force, and hence we get antagonism between force and resistance. Light also takes time in overcoming this resistance, *i.e.* in pushing aside the ether. It travels no doubt at a good pace—about 190,000 miles in a second; but even at this rate, and without being particular as to a few millions of miles, it takes three years and a quarter to reach us from the star which, so far as we know, is the nearest to us, viz. α Centauri. The ether, or whatever it may be called, tenuous as it is, is not unimportant, though it be not heavy. Without it we should have no light and possibly no heat, and the consequences of its absence would be rather formidable. I believe you have heard Dr. Tyndall on this subject. Supposing the visible universe to be as it is now supposed to be, *i.e.* in no part a mere vacuum, there can be no force without resistance in any part of it.

But photography carries us further, it shows us that light acts on matter chemically, that it is capable of decomposing or forcing asunder the constituents of chemical compounds, and is therefore a force met by resistance. In the year 1856 I made some experiments published in the *Philosophical Magazine* for January 1857, which seemed to me to carry still further what I may call the molecular fight between light and chemical affinity, and among them the following. Letters cut out of paper are placed between two polished squares of glass with tin-foil on the outsides. It is then electrized like a Leyden jar, for a few seconds, the glasses separated, the letters blown off, and the inside of one of the glasses covered with photographic collodion. This is then exposed to diffuse daylight, and on being immersed in the nitrate of silver bath the part which had been covered with the paper comes out dark, the remainder of the plate being unaffected. (This result was shown by the electric light lantern.) In this case we see that another imponderable force, electricity, invisibly affects the surface of glass in such a way that it conveys to another substance of definite thickness, viz. the prepared collodion, a change in the chemical relations of the substance (iodide of silver) pervading it, enabling it to resist that decomposition by light which but for some unseen modification of the surface of the glass plate it would have undergone, and no doubt the force of light being unable to effect its object was reflected or dispersed, and instead of changing its mode of motion in effecting chemical decomposition, it goes off on other business. The visible effect is in the collodion film alone. I have stripped that off, and the imprint remains on it, the surface of the glass being, so far as I could ascertain, unaffected. Thus in the film over the protected part, light conquers chemical affinity; in that over the non-protected part, chemical affinity resists and conquers light, which has to make an ignominious retreat. It is a curious chapter in the history of the struggles of molecular forces, and probably similar contests between light and chemical or physical attractions go on in many natural phenomena, some forms of blight and some healthy vegetable changes being probably dependent on the varying effects of light, and conditions, electrical or otherwise, of the atmosphere.

Let us now pass on to organic life. A blade of grass, as Burke, I believe, said as a figure of speech, is fighting with its neighbours. It is robbing them, and they are trying to rob it—no agreement or contract, simply force opposed to force. This struggle is good for the grass; if it got too much nutriment it would become diseased. The struggle keeps it in health. The rising of sap in trees, the assimilation of carbon, the process of growth, the strengthening themselves to resist prevalent winds, and many other instances might be given, which afford examples of the internal and external struggles in vegetable life.

I will now proceed to consider animal life, and in this case I will begin with the internal life of animals, which is a continual struggle. That great pump the heart is continuously beating—that is, conquering resistance. It is forcing the blood through the arteries, they assisting in squeezing it onwards. If they give way the animal dies; if they become rigid and resist too much, the animal dies. There must be a regulated antagonism, a rhythmical pulsation, the very term involving force and resistance. That the act of breathing is antagonistic scarcely needs argument. The muscular action by which the ribs are made to open out and close alternately, in order to inhale and exhale air, and other physiological changes which I cannot here go into, necessitate a continuous fight for life. So with digestion, assimilation, and other functions, mechanical and chemical forces and resistances come into play.

Since this lecture was written, I have heard of a discovery made, I am informed, by Prof. Metschnikoff, and which has brought to light a singular instance of internal antagonism. He is said to have proved that the white corpuscles of the blood are permanent enemies of Bacteria, and by inoculation will absorb poisonous germs; a recurrent war, as it appears, going on between them. If the corpuscle is the conqueror, the Bacteria are swallowed up, and the patient lives. If the corpuscles are vanquished, the patient dies, and the Bacteria live, at all events for a time. If the theory is founded, it affords a strong additional argument to the doctrine of internal antagonism. Possibly if there were no Bacteria, and the corpuscles had nothing to do, it would be worse for them and the animal whom they serve.

Let us now consider the external life of animals. I will take as an instance, for a reason which you will soon see, the life of a wild rabbit. It is throughout its life, except when asleep (of which more presently), using exertion, cropping grass, at war with vegetables, &c. If it gets a luxurious pasture it dies of repletion. If it gets too little it dies of inanition. To keep itself healthy it must exert itself for its food; this, and perhaps the avoiding its enemies, gives it exercise and care, brings all its organs into use, and thus it acquires its most perfect form of life. I have witnessed this effect myself, and that is the reason why I choose the rabbit as an example. An estate in Somersetshire, which I once took temporarily, was on the slope of the Mendip Hills. The rabbits on one part of it, viz. that on the hill-side, were in perfect condition, not too fat nor too thin, sleek, active, and vigorous. and yielding to their antagonists, myself and family, excellent food. Those in the valley, where the pasturage was rich and luxuriant, were all diseased, most of them unfit for human food and many lying dead on the fields. They had not to struggle for life, their short life was miserable and their death early, they wanted the sweet uses of adversity—that is, of antagonism.

The same story may be told of other animals. Carnivora, beasts or birds of prey, live on weaker animals; weaker animals herd together to resist, or, by better chance of warning, to escape, beasts of prey; while they, the Herbivora, in their turn are destroying vegetable organisms.

I now come to the most delicate part of my subject, viz. man (I include women of course!). Is man exempt from this continual struggle?

It is needless to say that war is antagonism. Is not peace so also, though in a different form? It is a common-place remark to say that the idle man is worn out by *ennui*, *i.e.* by internal antagonism. Kingsley's "Do-as-you-like" race—who were fed by a substance dropping from trees, who did no work, and who gradually degenerated until they became inferior to apes, and ultimately died out from having nothing to do, nothing to struggle with—is a caricature illustrative of the matter. That the worry of competition is nearly equivalent to the hardships and perils of military life seems proved to me by the readiness with which military life is voluntarily undertaken, ill as it is paid. If it were well paid, half our men would be in the military or naval service, and I am not sure that we should not have regiments of Amazons! The increased risk of life or limbs and the arduous nature of the work do not prevent men belonging to all classes from entering these services, little remunerative as they are. Others take the risks of travelling in the deserts of Africa or wintering in the Polar regions, of being eaten by lions or frozen to death, of falling from a Swiss mountain or foundering in a yacht, in preference to a life of tranquillity; and sportsmen elect the danger of endeavouring to kill an animal that can and may kill

them, to shooting tame pheasants at a *battue* or partridges in a turnip-field.

Then, in what is euphemistically called a life of peace, buyer and seller, master and servant, landlord and tenant, debtor and creditor, are all in a state of simmering antagonism; and the inventions and so-called improvements of applied science and art do not lessen it. Exercise is antagonism; at each step force is used to lift up our bodies and push back the earth; as the eminent Joseph Montgolfier said, that when he saw a company dancing, he mentally inverted his view and imagined the earth dancing on the dancers' feet, which it most unquestionably did. Indeed, his great invention of balloons was guessed at by his witnessing a mild form of antagonism between heat and gravitation. He, being a dutiful husband, was airing his wife's dresses, who was going to a ball. He observed the hot air from the fire inflated the light materials, which rose up in a sort of spheroidal form (you may some of you have noticed this form in dress!). This gave him the idea of the fire-balloon, which, being a large paper-maker at Annonay, he forthwith experimented on, and hence we got aërial navigation. This anecdote was told me by his nephew M. Seguin, also an eminent man. Even what we call a natural death is a greater struggle than that which other animals go through, and is, in fact, the most artificial of all deaths. The lower animals, practically speaking, do experience a natural death, *i.e.* a violent or unforeseen death. As soon as their powers decline to such an extent that they cannot take part in the struggle for existence, they die or are killed, generally quickly, and their sufferings are not protracted by the artificial tortures arising from the endeavours to prolong life.

Let us now pass from individuals to communities. Is there less antagonism now than of yore? Do the nations of Europe now form a happy family? Are the armaments of Continental nations, or is the navy of this country, less than in former years? The very expression "the Great Powers" involves antagonism.

As with wars and revolutions, so, as I have said, with regard to individuals, during our so-called peace, the fight is continuous among communities. If the water does not boil, it simmers. Not merely are there the struggles of poor against rich going on, but the battles for position and pre-eminence are constant. The subjugated party or sect seeks first for toleration, then for equalization, and then for domination.

We call contentment a virtue, but we inculcate discontent. A father reproaches his son for not exerting himself to improve his position, and at school and college and in subsequent periods of life efforts at advancement in the social scale are recommended. Individual antagonisms, class antagonisms, political, trading, and religious antagonisms take the place of war. Can war exhibit a more vigorous and persistent antagonism than competition does? Take the college student with ruined health; take the bankrupt tradesman with ruined family; take the aspirants to fashion turning night into day, and preferring gas or electric light to that of the sun: there is, to be sure, some excuse for this, as we so rarely see the latter. But our very amusements are of a combative character: chess, whist, billiards, racing, cricket, football, &c. And in all these we, in common parlance, speak of *beating* our opponent.

Even dancing is probably a relic and reminiscence of war, and some of its forms are of a military character. I can call to mind only one game which is not combative, and that is the game you are in some sort now playing, viz. "patience," and with, I fear, some degree of internal antagonism!

Take, again, the ordinary incidents of a day's life in London. 15,000 to 20,000 cabs, omnibuses, vans, private carriages, &c., all struggling, the horses pushing the earth back and themselves forwards, the pedestrians doing the same, but the horses compulsorily—they have not as yet got votes. The occupants of the cabs, vans, &c., are supposed to act from free will, but in the majority of cases they are as much driven as the horses. Insolvents trying to renew bills, rich men trying to save what they have got by saving half an hour of time. Imagine, if you can, the friction of all this, and add the bargaining in shops, the mental efforts in counting-houses, banks, &c., and road repair, now a permanent and continuous institution. Take our railways: similar efforts and resistances. Drivers, signal-men, porters, &c., and the force emanating from the sun millions of years ago, and locked up in the coal-fields, as Stephenson suggested, now employed to overcome the inertia of trains and to make them push the earth in this or that direction, and themselves along its surface. Take the daily struggles in commerce, law, professions, and legislation, and sometimes even in science and literature. Politics I cannot enter upon here, but must leave you to judge whether there is not some degree of antagonism in this pursuit. In all this there is plenty of useful antagonism, plenty of useless—much to please Ormuzd and much to delight Ahriman; but of the two extremes, over-work or stagnation, the latter would, I think, do Ahriman's work more efficiently than the former. We cry peace when there is no peace. Would the world, however, be better if it were otherwise? Is the Nirvana a pleasing prospect? Sleep, though not without its troubles and internal antagonism, is our nearest approach to it, but we should hardly wish to be always asleep.

Shakespeare not only knew something about gravitation, but he also knew something about antagonism. He says, by the mouth of Agamemnon—

> "Sith every action that hath gone before
> Whereof we have record, trial did draw
> Bias and thwart, not answering the aim,
> And that unbodied figure of the thought
> That gav't surmised shape."

In no case is the friction of life shown more than in the performance of "duty," *i.e.* an act of self-resistance, a word very commonly used; but the realization of it is by no means so frequent. Indeed, faith in its performance so yields to scepticism that it is said that when a man talks of doing his duty, he is meditating some knavish trick.

The words good and evil are correlative: they are like height and depth, parent and offspring. You cannot, as far as I can see, conceive the existence of the one without involving the conception of the other. In their common acceptation they represent the antagonism between what is agreeable or beneficial and what is painful or injurious.

An old anecdote will give us the notion of good and evil in a slenderly educated mind. A missionary having considered that he had successfully inculcated good principles in the mind of a previously untutored savage, produced him for exhibition before a select audience, and began his catechism by asking him the nature of good and evil. "Evil," the pupil answered, "is when other man takes my wife." "Right," said the missionary, "now give me an example of good." The answer was: "Good is when me takes other man's wife." The answer was not exactly what was expected, but was not far in disaccord with modern views among ourselves and other so-called civilized races. I don't mean as to running away with other men's wives! But we still view good and evil very much as affecting our own interests. At the commencement of a war each of the opposing parties view victory—*i.e.* the destruction of their enemies—as good, and being vanquished as evil. Congregations pray for this. Statesmen invoke the God of battles. Those among you who are old enough will call to mind the Crimean War. Each combatant nation gives thanks for the destruction of the enemy, each side possibly believing that they respectively are in the right, but in reality not troubling themselves much about that minor question. We (unconsciously perhaps) "compound for sins we are inclined to by damning those we have no mind to." So in the daily life of what is called peace. The stage-coach proprietor rejoiced when he had driven his rival off the road, railway directors and shareholders now do the same, so do publicans, shopkeepers, and other rivals. We are still permeated by the old notion of good and evil. But "antagonism," as I view it, not only comprehends the relation of good and evil, but, as I have said, produces both, and is as necessary to good as to evil. Without it there would be neither good nor evil. Judging of the lives of our progenitors from what we see of the present races of men of less cerebral development, we may characterize them as having been more impulsive than ourselves, and as having their joys and sorrows more quickly alternated. After the hunt for food, accompanied by privation and suffering, comes the feast to gorging. Their main evil was starvation, their good repletion. Even now the Esquimaux watches a seal-hole in the bitter cold for hours and days, and his compensation is the spearing and eating the seal. The good is resultant upon and in the long run I suppose equivalent to the evil. These men look not back into the past, and forward into the future as we do. We, by extending our thought over a wider area, are led to more continuing sacrifices, and aim at more lasting enjoyment in the result. The child suffers at school in order that his future life may be more prosperous. The man spends the best part of his life in arduous toil, physical or mental, in order that he may not want in his later years, or that his family may reap the benefit of his

labour. Further-seeing men spend their whole lives on work little remunerative that succeeding generations may be benefited. The prudent man transmits health and wealth to his descendants, the improvident man poverty or gout. One main element of what we call civilization is the capability of looking further back into the past, and further forward into the future; but, though measured on a different scale, the average antagonism and approximate equivalence appear to me to be the same.

Can we suppose a state of things either in the inorganic or the organic world which, consistently with our experience or any deduction drawn from it, would be without antagonism? In the inorganic world it would be the absence of all movement, or, what practically amounts to the same thing, movement of everything in the same direction, and the same relative velocity; for, as movement is only known to us by relation, movement where nothing is stationary or moving in a different direction or with a different velocity would be unrecognizable.

So in the organic but non-sentient world, if there were no no struggle, no absorption of food, no growth, nothing to overcome, there would be nothing to call life. If, again, in the sentient world there were no appetites, no hopes—for both these involve discontent—no fear, no good or bad, what would life be? If fully carried out, is not a life without antagonism no life at all, a barren metaphysical conception of existence, or rather alleged conception, for we cannot present to the mind the form of such conception?

In the most ordinary actions, such as are necessary to sustain existence, we find, as I have already pointed out, a struggle more or less intense, but we also find a reciprocal interdependence of effort and result. The graminivorous animal is during his waking hours always at work, always making a small but continuous effort, selecting his pastures, cropping vegetables, avoiding enemies, &c. The Carnivora suffer more in their normal existence; their hunger is greater, and their physical exertion when they are driven by hunger to make efforts to obtain food is more violent than with the Herbivora, if they capture their prey by speed or battle, or their mental efforts are greater if they capture it by craft. But then their gratification is also more intense, and thus there is a sort of rough equation between their pain and their pleasure, the more sustained the labour the more permanent is the gratification.

As, with food or exercise, deficiency is as injurious in one as is excess in another direction, so, as affecting the mind of communities, as I have stated it to be with individuals, the effect of a life of ease and too much repose is as much to be avoided as a life of unremitting toil. The Pitcairn islanders, who managed in some way to adapt their wants to their supply and to avoid undue increase of population, are said never to have reached old age. In consequence of the uneventful, unexcited lives they led, they died of inaction, not from deficiency of food or shelter, but of excitement. They should have migrated to England! They died as hares do when their ears are stuffed with cotton, *i.e.* from want of anxiety. We have hope in our suffering, and in the mid gush of our pleasures something bitter surges up.

> "We look before and after, and pine for what is not,
> Our sincerest laughter with some pain is fraught,
> Our sweetest songs are those which tell of saddest thought."

The question may possibly occur to you, Have we more or less antagonism now than in former times? We certainly have more complexity, more differentiation, in our mental characteristics, and probably in our physical, so far as the structure of the brain is concerned; but is there less antagonism? With greater complexity come increased wants, more continuous cares. Higher cerebral development is accompanied with greater nervous irritability, with greater social intricacies—we have more frequent petty annoyances, and they affect us more. With all our so-called social improvements, is there not the same struggle between crime and its repression? If we have no longer highway robberies, how many more cases of fraud exist, most of it not touched by our criminal laws? As to litigation I am perhaps not an impartial judge, but it seems to me that if law were as cheap as is desired, every next-door neighbour would be in litigation. It would seem as if social order had never more than the turn of the scale which is necessary to social existence in its favour when contrasted with the disorganizing forces. Without that there would be perpetual insurrections and anarchy. But though antagonism takes a different form it is still there. Are wars more regulated by justice than of yore? I venture to doubt it, though probably many may disagree with me. National self-interest or self-aggrandisement is, I think, the predominant factor, and is frequently admittedly so. I also doubt if the old maxim "If you wish for peace, prepare for war," is of much value. Large armaments and improvements in the means of destruction (whose inventors are more thought of than the discoverers of natural truths) are as frequently the cause of war as of its prevention. Are wars less sanguinary with 100-ton guns than with bows and arrows? I cannot enter into statistics on this subject, but a sensible writer who has, viz. Mr. Finlaison, came to the conclusion that wars ceased now as anciently, not in the ratio of the improvements in killing implements, but from exhaustion of men or means. Wars undoubtedly occur at more distant intervals, or the human race would become extinct. Probably the largely increased competition supplies their place: we fight commercially more and militarily less. It is a sad reflection that man is almost the only animal that fights, not for food or means of life or of perpetuating its race, but from motives of the merest vanity, ambition, or passion. War is, however, not wholly evil. It develops noble qualities—courage, endurance, self-sacrifice, friendship, &c.—and tends to get rid of the silly incumbrances of fashion and ostentation. But do the much be-praised inventions of peace bring less antagonism? Consider the enormous labour and waste of time due to competition in the advertizing system alone. Paper-making, type-founding, printing, pasting, posting or otherwise circulating, sandwich-men, &c., all at work for purposes which I venture to think are in great part useless; and those who might add to the productiveness of the earth, or to the enriching our knowledge, are helping to extend the limits of the black country, and wasting their time in interested self-laudation. And the consumer pays the costs. "Buy my clothing, which will never wear out." "Become a shareholder in our Company, which will pay cent. per cent." "Take my pills, which will cure all diseases," &c. These eulogies come from those highly impartial persons the advertisers, all promising golden rewards, but, as with the alchemists, on condition that gold be paid in advance for their wares; and the silly portion of the public, no small body, take them at their word. Though you may not fully agree in this my anathema of the advertising system, and though there may be some small modicum of good in it, I think you will agree that it affords a notable illustration of antagonism. If I were a younger man, I think I should go to Kamchatka to avoid the penny post; possibly I should not be satisfied when I got there. Civilization begins by supplying wants, and ends by creating them, and each supply for the newly-created want begets other wants, and so on "*toties quoties*."

As far as we can judge by its present progress, mankind seems tending to an automatic state. The requirements of each day are becoming so numerous as to occupy the greater portion of that day; and when telegrams, telephones, electro-motion, and numerous other innovations which will probably follow these, reach their full development, no time will be left for thought, repose, or any spontaneous individual action. In this mechanical state of existence, in times of peace, extremes of joy and sorrow, of good and evil, will become more rare, and the necessary uniformity of life will reduce passion and feeling to a continuous petty friction. The converse of the existence contemplated by the Stoics will be attained, and, instead of a life of calm contemplation, our successors will have a life of objectless activity. The end will be swallowed up in the means. It will be all pursuit and no attainment. Is there a *juste milieu*, a point at which the superfluous *commoda vitæ* will cease? None probably would agree at where that point should be fixed, and the future alone can show whether the human race will emancipate itself from being, like Frankenstein, the slave of the monster it has created. In the cases I have given as illustrations—and many more might be adduced—the evil resulting from apparently beneficial changes is not a mere accident: it is as necessary a consequence as reaction is a consequence of action. In the struggle for existence or supremacy, inevitable in all social growths, the invention, enactment, &c., intended to remedy an assumed evil will be taken advantage of by those for whom it is not intended; the real grievance will be exaggerated by those having an interest in trading on it, and the remedy itself will have collateral results not contemplated by those who introduce the change. I could give many instances of this by my own experience as an advocate and judge, but this would lead me away from my subject. Evils, indeed, result from the very change of habit induced by the alleged improvement. The carriage which saves fatigue induces listnessness, and tends to prevent healthy exercise. The knife and fork save the labour of mastication, but by their use

there is not the same stimulus to the salivary glands, not the full healthy amount of secretion, whereby digestion suffers; there is not the same exercise of the teeth whereby they are strengthened and uniformly worn, as we see in ancient skulls. It seems not improbable that their premature decay in civilized nations is due to the want of their normal exercise by the substitution of the knife and fork and stew-pan. According to the evolution theory, our organs have grown into what they are, or ought to be, by long use, and the remission of this tends to irregular development, or atrophy. Every artificial appliance renders nugatory some pre-existing mode of action, either voluntary or involuntary; and as the parts of the whole organism have become correlated, each part being modified by the functions and actions of the others, every part suffers more or less when the mode of action of any one part is changed. So with the social structure, the same correlation of its constituent parts is a necessary consequence of its growth, and the change of one part affects the well-being of other parts. All change, to be healthy, must be extremely slow, the defect struggling with the remedy through countless but infinitesimally minute gradations.

Lastly, so the forms of government give us any firm ground to rest upon as to there being less undue antagonism in one than in another form. Whether it is better to run a risk of, say, one chance in a thousand or more of being decapitated unjustly by a despot, or to have what one may eat or drink, or whom one may marry, decided by a majority of parish voters, is a question on which opinions may differ, but there is abundant antagonism in either case.

Communism, the dream of enthusiasts, offers little prospect of ease. It involves an unstable equilibrium, *i.e.* it consists of a chain of connection where a defect in one link can destroy the working of the whole system, and why the executive in that system should be more perfect than in others I never have been able to see. Antagonism, on the other hand, tends to stability. Each man working for his own interests helps to supply the wants of others, thus ministering to public convenience and order, and if one or more fail the general weal is not imperilled.

You may ask, Why this universal antagonism? My answer is, I don't know; Science deals only with the How? not with the Why? Why does matter gravitate to other matter, with a force inversely as the square of the distance? Why does oxygen unite with hydrogen? All I can say is that antagonism is, to my mind, universal, and will, I believe, some day be considered as much a law, as the law of gravitation. If matter is, as we believe, everywhere, even in the interplanetary spaces, and if it attracts and moves other matter, which it apparently must do, there must be friction or antagonism of some kind. So with organized beings, Nature only recognizes the right, or rather the power, of the strongest. If twenty men be wrecked on a secluded island which will only support ten, which ten have a right to the produce of the island? Nature gives no voice, and the strongest take it. You may further ask me, *Cui bono?* what is the use of this disquisition? I should answer, If the views be true, it is always useful to know the truth. The greatest discoveries have appeared useless at the time. Kepler's discovery of the relations of the planetary movements appeared of no use at the time; no one would now pronounce it useless. I can, however, see much probable utility in the doctrine I have advocated. The conviction of the necessity of antagonism, and that without it there would be no light, heat, electricity, or life, may teach us (assuming free will) to measure effort by the probable result and to estimate the degree of probability. It may teach us not to waste our powers on fruitless objects, but to utilize and regulate this necessity of existence; for, if my views are correct, too much or too little is bad, and a due proportion is good (like many other useful things, it is best in moderation), to accept it rather as a boon than a bane, and to know that we cannot do good without effort—that is, without some suffering.

I have spoken of antagonism as pervading the universe. Is there, you may ask, any limit in point of time or space to force? If there be so, there must be a limit to antagonism. It is said that heat tends to dissipate itself, and all things necessarily to acquire a uniform temperature. This would in time tend practically, though not absolutely, to the annihilation of force and to universal death; but if there be evidence of this in our solar system and what we know of some parts of the universe, which probably is but little, is there no conceivable means of reaction or regeneration of active heat? There is some evidence of a probable zero of temperature for gases as we know them, *i.e.* a temperature so low that at it matter could not exist in a gaseous form; but passing over gases and liquids, if matter becomes solid by loss of heat, such solid matter would coalesce, masses would be formed, these would gravitate to each other, and come into collision. It would be the nebular hypothesis over again. Condensation and collisions would again generate heat; and so on *ad infinitum.*

Collisions in the visible universe are probably more frequent than is usually supposed. New nebulæ appear where there were none before, as recently in the constellation of Andromeda. Mr. Lockyer, as I have said, considers that they are constant in the nebulæ; and if there be such a number of meteorites as are stated to fall daily into the atmosphere of this insignificant planet, what numbers must there be in the universe? There must be a sort of fog of meteorites, and this may account, coupled with possibly some dissipation of light or change of it into other forces, for the smaller degree of light than would be expected if the universe of stellar bodies were infinite. For if so, and the stars are assumed to be of an equal average brightness, then if no loss or obstruction, as light decreases as the square of the distance and stars increase in the same ratio, the night would be as brightly illuminated as the day. We are told that there are stars of different ages—nascent, adolescent, mature, decaying, and dying; and when some of them, like nations at war, are broken up by collision into fragments or resolved into vapour, the particles fight as individuals do, and like them end by coalescing and forming new suns and planets. As the comparatively few people who die in London to-night do not affect us here, so in the visible universe one sun or planet in a billion or more may die every century and not be missed, while another is being slowly born out of a nebula. Thus worlds may be regenerated by antagonism without having for the time more effect upon the Kosmos than the people now dying in London have upon us. I do not venture to say that these collisions are in themselves sufficient to renew solar life; time may give us more information. There may be other modes of regeneration or renewed activity of the dissipated force, and some of a molecular character. The conversion of heat into atomic force has been suggested by Mr. Crookes. I give no opinion on that, but I humbly venture to doubt the mortality of the universe.

Again, is the universe limited? and if so, by what? Not, I presume, by a stone wall! or if so, where does the wall end? Is space limited, and how? If space be unlimited and the universe of suns, planets, &c., limited, then the visible universe becomes a luminous speck in an infinity of dark vacuous space, and the gases, or at all events the so-called ether, unless limited in elasticity, would expand into this vacuum—a limited quantity of ether into an infinite vacuum! If the universe of matter be unlimited in space, then the cooling down may be unlimited in time. But these are perhaps fruitless speculations. We cannot comprehend infinity, neither can we conceive a limitation to it. I must once more quote Shakespeare, and say in his words, "It is past the infinite of thought." But whatever be the case with some stars and planets, I cannot bring myself to believe in a dead universe surrounded by a dark ocean of frozen ether.

Most of you have read "Wonderland," and may recollect that after the Duchess has uttered some ponderous and enigmatical apophthegms, Alice says, "Oh!" "Ah," says the Duchess, "I could say a good deal more if I chose." So could I; but my relentless antagonist opposite (the clock) warns me, and I will only add one more word, which you will be glad to hear, and that word is—Finis.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The list of Physical Science lectures this term includes Prof. Liveing on Spectroscopic Chemistry, Mr. Robinson on Agricultural Chemistry, Mr. Ruhemann on Gas Analysis and on Aromatic Compounds, Mr. Shaw on Electrolysis, Mr. Wilberforce on Dynamo-electric Machines, Mr. Lyon on Machine Construction.

Prof. Stokes lectures on Hydrodynamics, Dr. Besant on Differential Equations and Solid Geometry, Dr. Glaisher on Theory of Errors, Mr. Stearn on Attractions and Theory of Potential.

In Biology, Mr. Langley is lecturing on the Central Nervous System, Prof. Macalister on the Rudimental Structures of the Human Body, Mr. Gadow on the Morphology of Mammalia recent and extinct, Mr. F. Darwin on the Physiology of Plants (advanced demonstrations).

In Geology, Prof. Hughes lectures on the geology of the

neighbourhood of Cambridge, Mr. Marr on Advanced Physical Geology, Mr. Roberts on the Crinoidea.

The above are only a selection out of a long list.

Mr. J. G. Adams, of Christ's College, has been appointed Demonstrator of Pathology on Mr. Rolleston's resignation.

SCIENTIFIC SERIALS.

American Journal of Science, April.—The absolute wave-length of light, by Louis Bell. The final results are here given of the research partially reported in the *Journal* for March 1886. Owing to the wide discrepancies in the value of this constant as determined by various observers and methods, the author gives a brief historical summary of the subject, with a critical discussion of the standards of length, methods, and apparatus employed in the present investigation. The details of the experimental work, together with some remarks on the final results, and some questions of theoretical and practical interest connected with the work of recent experimenters in this field, are reserved for a future number.—History of the changes in the Mount Loa craters; Part 1, Kilauea (continued), by James D Dana. Here are discussed questions connected with the ascensive action in the conduit lavas, the effects of heat, the hydrostatic and other gravitational pressure.—The electromotive force of magnetization, by Edward L. Nichols and William S. Franklin. At the Ann Arbor meeting of the American Association for the Advancement of Science the authors described some singular modifications in the relation of iron to acids which occur when the reaction takes place within the magnetic field. In the present paper, which was read at the New York meeting of the Association in 1887, they deal with the behaviour of iron when that metal acts as one electrode in a voltaic circuit, and is at the same time subjected to magnetization.—Notes on certain rare copper minerals from Utah, by W. F. Hillebrand. A series of rare copper ores, including olivenite, erinite, tyrolite (?), chalcophyllite, clinoclasite, mixite (?), and bronchantite, are here subjected to careful chemical and physical examination.—The Taconic system of Emmons, and the use of the name Taconic in geological nomenclature (continued), by Chas. D. Walcott. The main subject of this paper is the geology of the Taconic area as known to Dr. Emmous, with a comparison of its area as now known. As a result of this comparative study, the author finds that the Lower Taconic is essentially a repetition of the Lower Silurian (Ordovician) of the Champlain Valley, while the Upper Taconic appears to be conformably subjacent to the Stockbridge Limestone of the Lower Taconic, and to include the Potsdam horizon at or near its upper portion.—Three formations of the Middle Atlantic Slope (continued), by W. J. McGee. This paper is occupied with the Appomattox formation, its character, and distribution.—W. Le Conte Stevens describes a new lecture apparatus of an extremely simple character for the demonstration of reflection and refraction phenomena.

SOCIETIES AND ACADEMIES.

London.

Royal Society, March 8.—"Further Observations on the Electromotive Properties of the Electrical Organ of *Torpedo marmorata*." By Francis Gotch, Hon. M.A. Oxon., B.A., B.Sc. London, M.R.C.S. Communicated by Prof. J. Burdon Sanderson, F.R.S.

In the present treatise the author details the results of further observations as to the electromotive properties of the electrical organ of Torpedo, the experiments being carried out in October, 1887, at the laboratory of the Société Scientifique d'Arcachon.

I. The first part of the work deals entirely with the phenomena of "irreciprocal conduction" in the organ of Torpedo, as described by du Bois-Reymond.

From du Bois Reymond's experiments it would appear that the organ possesses the remarkable property of conducting an intense current of short duration, led lengthwise through its columns, better when the current is directed from its ventral to its dorsal surface than when directed the reverse way. The former direction coincides with that of the current of the shock of the organ, and is therefore termed by him "homodromous," the latter, being opposite in direction, is termed "heterodromous." The evidence rests upon the value of the galvanometric deflections obtained when both currents are allowed to traverse a strip of organ and a galvanometric circuit. The deflections are markedly unequal, particularly when induced currents are used, the homodromous effect being always much greater than the heterodromous. The homodromous current must therefore either encounter less resistance than the heterodromous, or its electromotive force must be suddenly strengthened, and that of the heterodromous current weakened, by the sudden establishment in the tissue of a new source of electromotive energy. The first is the view taken by Prof. du Bois-Reymond.

(1) The present rheotome experiments reveal (*a*) the new fact that the passage of such intense currents of short duration is always followed by an excitatory response (shock) in the tissue; (*b*) that if the intense current due to this response is allowed to affect the galvanometer as well as the induced or other exciting current, then by obvious algebraic summation the homodromous deflection must be much larger than the heterodromous; (*c*) and that when by means of a fast-moving rheotome the induction shock only is allowed to affect the instrument, no irreciprocity is found.

The author therefore assumes that the phenomena of irreciprocal conduction are in reality excitatory phenomena, the nature of which, from the methods of investigation used, have not been recognized.

(2) The time relations of this response of the isolated strip of the organ to direct stimulation by the traversing induction shock are now for the first time investigated, by means of the rheotome, and the influence of temperature and other conditions upon these is shown by experimental evidence.

II. The second part deals with entirely novel phenomena—namely, the excitation of the organ by the current of its own excitatory state. It is shown that in vigorous summer fish every response of the whole or part of the organ to a single excitation of its nerves is followed by a second response, due to the passage through its own substance of the intense current of the first response. In other words, the shock of the organ excites its own nerve fibres and nerve endings, producing a feebler second shock, which in a similar manner evolves a feebler third shock; this a fourth, and so on.

The response of the isolated organ to nerve excitation is thus multiple; a primary, secondary, tertiary response following the application to the nerve of a single stimulus. Since all these responses produce currents similarly directed through the columns of the organ, each column during its activity must reinforce by its echoes the force of the primary explosion, both in its own substance and also in that of its neighbours.

Linnean Society, April 5.—Mr. W. Carruthers, F.R.S., President, in the chair.—Amongst the exhibitions of the evening Mr. D. Morris (Kew) showed a curious native bracelet from Martinique. Although formed apparently of seeds, or beads of wood, or bone, its real composition had puzzled both botanists and zoologists, and until microscopically examined could not be determined.—Mr. J. G. Baker, F.R.S., exhibited a series of specimens of *Adiantum Fergusoni* and *Capillis Veneris*, and offered some remarks upon their specific and varietal characters.—Mr. J. E. Harting exhibited a specimen of a rare British animal, the pine-marten, which had been trapped in Cumberland; and made some observations on the present distribution of the species in the British Islands.—Mr. Clement Reid exhibited a series of fruits and seeds obtained by Mr. J. Bennie from inter-glacial deposits near Edinburgh, affording evidence of a colder climate formerly than that now prevailing in the Lowlands of Scotland.—Mr. F. Crisp exhibited some fragmentary remains of a wild goose shot in Somersetshire, which had been reported as the lesser whitefronted goose (*Anser erythropus*, Linn.), but which was apparently an immature specimen of *Anser albifrons*, Scopoli.—In the absence of the author, a paper by Mr. A. W. Waters, on some ovicells of the Cyclostomatous Bryozoa, was read by the Zoological Secretary, Mr. W. Percy Sladen; and after an interesting discussion, the meeting adjourned.

Chemical Society, March 28.—Annual General Meeting.—Mr. W. Crookes, F.R.S., in the chair.—The President delivered an address on which we have already commented.—The following Officers and Council were elected for the ensuing session:—President: Mr. W. Crookes, F.R.S. Vice-Presidents who have filled the office of President: Sir F. A. Abel, F.R.S.; Dr. Warren de la Rue, F.R.S.; Dr. E. Frankland, F.R.S.; Dr. J. H. Gilbert, F.R.S.; Dr. J. H. Gladstone, F.R.S.; Dr. A. W. Hofmann, F.R.S.; Dr. H. Müller, F.R.S.; Prof. Odling, F.R.S.; Dr. W. H. Perkin, F.R.S.; Sir Lyon Playfair, F.R.S.; Sir H. E. Roscoe,

F.R.S., and Dr. A. W. Williamson, F.R.S. Vice-Presidents: Prof. G. Carey Foster, F.R.S.; Mr. David Howard; Prof. J. W. Mallet, F.R.S.; Prof. H. McLeod, F.R.S.: Mr. Ludwig Mond; and Prof. Schorlemmer, F.R.S. Secretaries: Prof. H. E. Armstrong, F.R.S., and Prof. J. M. Thomson. Foreign Secretary: Dr. F. R. Japp, F.R.S. Treasurer: Dr. W. J. Russell, F.R.S. Ordinary Members of Council: Prof. T. Carnelly, Mr. A. H. Church, Prof. Clowes, Prof. Dunstan, Dr. P. F. Frankland, Mr. R. J. Friswell, Mr. C. W. Heaton, Mr. E. Kinch, Dr. H. F. Morley, Dr. R. T. Plimpton, Prof. Purdie, and Prof. Ramsay.

April 5.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—Researches on the constitution of azo- and diazo-derivatives; part iii., compounds of the naphthalene β-series, by Prof. R. Meldola, F.R.S., and Mr. F. J. East.—The action of finely divided metals on solutions of ferric salts, and a rapid method for the titration of the latter, by Mr. D. J. Carnegie.

Anthropological Institute, April 10.—Francis Galton, F.R.S., President, in the chair.—Captain Strachan exhibited a young Papuan boy brought by him from the north-west coast of New Guinea.—Mr J. Allen Brown read a paper on some small highly specialized forms of stone implements, found in Asia, North Africa, and Europe.—A paper by MM. Henri and Louis Siret, on the early age of metal in the south-east of Spain, was read.

PARIS.

Academy of Sciences, April 16.—M. Janssen, President, in the chair.—On the spectra of oxygen, by M. J. Janssen. Attention is called to Olszewski's recent experiments with liquefied oxygen, which fully confirm the results of the author's researches on the phenomena of elective absorption in oxygen gas. The bands already determined by him have been observed by Olszewski with a thickness of 7 millimetres of liquid oxygen, while a thickness of from 4 to 5 millimetres would be required to detect the presence of the strongest band, which occurs in the neighbourhood of D. This is a remarkable confirmation of the law of the product of the thickness by the square of the density regulating one of the two systems of bands described by M. Janssen.—On the relations of atmospheric nitrogen to vegetable soil, by M. Th. Schlœsing. This is a reply to the objections recently urged by M. Berthelot against the character of the author's researches, and the general conclusions based on them. He denies the validity of M. Berthelot's criticisms, and insists that he does not deny the fixation of atmospheric nitrogen in vegetable soils. He maintains, however, that the phenomenon is neither determined by his own experiments nor demonstrated with sufficient accuracy by M. Berthelot's analyses.—On a source of algebraic equations whose roots are all real, by M. G. Fouret. An algebraic process is explained, by means of which equations, all of whose roots are real, may be combined in such a way as to obtain from them fresh equations possessing the same property. The following theorem is proposed and discussed: If the equation

$$F(x) \equiv a_0 x^n + a_1 x^{n-1} + a_2 x^{n-2} + \ldots + a_{n-1} x + a_n = 0$$

has all its roots real, then the equation

$$\phi(x) \equiv a_0 f(x) + a_1 f'(x) + a_2 f''(x) + \ldots + a_{n-1} f^{(n-1)}(x) + a_n f^{(n)}(x) = 0,$$

in which $f(x)$ represents an entire polynome of equal or higher degree to n, has at least as many real roots as the equation $f(x) = 0$; and if it has more, the excess is an even number.—On Foucault's gyroscope, by M. E. Guyou. An elementary solution is given of the problem connected with the rotation of a solid body suggested by the movement of this apparatus.—On a new method of measuring the heat of evaporation of liquefied gases, by M. E. Mathias. The calorimetric methods usually employed are either those of *variable* temperature or of the *fixed* temperature of melting ice. But for the purpose of his researches the author has had to employ one of *constant* temperature, the nature and advantages of which are here described. It is specially applicable in the case of gases which, like ethylene, carbonic acid, and the protoxide of nitrogen, have their critical point at the ordinary temperature.—On a class of electric currents set up by the ultra-violet rays, by M. A. Stoletow. Hertz, Wiedemann, and others having shown the influence of the ultra-violet rays on electric discharges at high tension, the author here inquires whether a similar effect may not be obtained with electricity of feeble potential.—On a regulator of electric light, by M. Charles Pollak. In the apparatus here described the movement required to be communicated to the carbons in order to supply and maintain the electric arc is obtained by the thermic expansion of the conducting wires. This appliance, which regulates the electric arc for a period of three hours consecutively, has the advantage of extreme simplicity, dispensing with all intricate mechanism, as well as with electro-magnets.—On a sodico-potassic carbonate, by MM. L. Hugounenq and J. Morel. The authors have obtained this substance by exposing to the open air at a temperature of 12° to 15° C. a solution of carbonate of soda containing carbonate of potassa in the presence of a great excess of iodide of potassium mixed with phosphate and chloride of sodium. It approaches the formula—

$$CO_3K_2,6H_2O + (CO_3Na_2,6H_2O).$$

These researches show generally that the carbonates of soda and of potassa may crystallize together, yielding isomorphous mixtures, which can scarcely be represented by definite formulas.—New experiments on inoculation against rabies, by M. G. Galtier. These experiments, made on sheep and goats, show that herbivorous animals may be successfully preserved from the bite of mad dogs by the usual processes of inoculation, whether applied before or immediately after the attack.—A communication was received from the Minister of Public Instruction announcing the results of the measures recently taken to determine the exact superficial area of France calculated by the planimetric method. This estimate gives 536,408 square kilometres, which is ·8012 more than that indicated by the Bureau of Longitudes, and 2929 more than that of the Russian General Strelbitsky.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Outlines of Qualitative Analysis: G. W. Slatter (Murby).—Text-book of Biology: J. R. A. Davis (Griffin).—British Birds: Key List: Colonel L. H. Irby (Porter).—In Pursuit of a Shadow: A Lady Astronomer (Trübner).—A Treatise on Alcohol, 2nd edition: Dr. T. Stevenson (Gurney and Jackson).—Allgemeine Geologie: Dr. Karl von Fritsch (Engelhorn, Stuttgart).—Arithmetic for Beginners: Rev. J. B. Lock (Macmillan).—Nature Readers, Sea-Side and Way-Side, No. 1: J. W. Wright (Heath, Boston).—Mr. Tebbutt's Observatory, Windsor, New South Wales: J. Tebbutt (Sydney).—Bulletin du Musée Royal d'Histoire Naturelle de Belgique, Tome v. No. 1.

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